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"Let Knowledge grow from more to more."—*Tennyson.*

KNOWLEDGE

AN ILLUSTRATED MAGAZINE OF SCIENCE

PLAINLY WORDED—EXACTLY DESCRIBED

CONDUCTED BY RICHARD A. PROCTOR.

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TO OUR READERS.

"KNOWLEDGE" is a weekly magazine intended to bring the truths, discoveries, and inventions of Science before the public in simple but correct terms—to be, in fact, the minister and interpreter of Science for those who have not time to master technicalities (whether of Science generally or of special departments).

Knowledge will contain *Original Articles* by the ablest exponents of Science; *Serial Papers* explaining scientific methods and principles; *Scientific News* translated into the language of ordinary life; a *Correspondence Section* (including columns of *Notes and Queries*) for free and full discussion, and especially for inquiry into matters which the readers of original articles may find difficult or perplexing; and *Reviews* of all scientific treatises suitable for general reading. In addition to these portions, there will be a section for Mathematics, and columns for Chess and Whist (regarded as scientific games), conducted on a novel plan intended to render these portions at once useful to learners and interesting to proficients.

I have long regarded the material benefits derived from Science, great though these unquestionably are, as small by comparison with those to be derived from Science as a means of mental and moral culture. Nearly ten years have passed since, recognising this, I pointed out the necessity for such a journal as the present. We have none doing the work which Knowledge is intended to perform.

But I felt that before attempting to conduct such a Journal, I should obtain as wide an experience as possible of the wants of the class of readers for which it is intended. During the last ten years I have come in contact, as lecturer and writer, with tens of thousands belonging to that class. The experience I have thus gained is altogether exceptional. From letters addressed to me during my lecture-tours in Great Britain, the United States, Canada, Australia, Tasmania, and New Zealand, as well as those almost equally numerous written to me from other countries, I have learned the nature of the difficulties which commonly perplex scientific students and the readers of scientific treatises. I believe this experience must prove of great value to me in conducting Knowledge.

The tone of the articles will be that which I have found most useful in lecturing and writing. The general public do not want Science to be presented to them as if they were of intelligence inferior to their teachers. But they cannot be expected to take interest in statements couched in abstruse or technical terms. Nor is Science degraded when plain untechnical language, such as we propose to use in Knowledge, is employed; when, for instance, instead of speaking about the "mean equatorial horizontal solar parallax," a writer refers to the sun's distance; or when a race of men is not described as "microseme and dolichocephalic," but as small-faced and long-headed.

Discoveries and inventions communicated to scientific societies at home and abroad will not be presented until they have been translated from technical language which to the general reader is mere jargon.

The price of Knowledge is lower than that which has heretofore been assigned to periodicals of the same class. It is trusted that those who approve of the plan above sketched, and wish to see the Magazine firmly established, will help to

"Let knowledge grow from more to more,"

by making its existence known to their friends. In this and other matters cordial co-operation from all quarters is invited.

Richard A. Proctor.

SCIENCE AND RELIGION.

By the Editor.

MANY seem to imagine that the tendency of Science, especially in its more recent developments, is irreligious. Some give a special reason for this strange opinion, namely, the inconsistency which they conceive to exist between some of the results to which modern Science unmistakably points, and ideas which have been derived from poetical descriptions found in the Bible. So far as this particular form of objection is concerned, Science need be at no pains to formulate a reply. It would be as reasonable to do so, I conceive, as it would be seriously to answer such a question as this: "How can the Darwinian theory of the remote cousinship of man and monkeys be reconciled with Job's statement (Job, xxx. 29), 'I am a brother to dragons'?" or this: "How can the views of modern medical men about the intestines be reconciled with Job's assertion (Job, xxx. 27), 'My bowels boiled and rested not'?" Moreover, the world is not interested (or should not be) in hearing the views of Science as to the real meaning of words which theologians, after much time and trouble given to a matter lying

specially within their province, are not at one in interpreting. But when the question is of the truth of those scientific views which are oppugned, or as to the bearing of Science generally on Religion, the case is different. Science may reasonably answer questions relating to the influence of scientific discoveries on the human mind.

The great objection raised against modern science appears to be in the main this, that it enlarges unduly our ideas of the vastness of God's domain in space, of the immensity of the time periods during which He acts, and in fine, of His inconceivable power and wisdom. We may admire the wisdom of the Almighty, as shown in the pebble, or the rock, in the flower or in the tree, in the insect or the animal, nay, we may even so far extend our vision as to recognise the laws under which a stratum, or a forest, or a race of animals, perhaps even a continent, or a flora, or a fauna, had their origin and passed through their various stages of development. But we must not extend our survey further. To see God's hand in these, His wisdom in the laws by which they are formed, is to be religious and good, but to trace His power and wisdom on a larger scale is to be irreligious and wicked. Evolution on the small scale we may admit without harm; but to see evolution in the dev lopment of a world or a world-system, and still more toesee evolution throughout the entire universe as revealed to man, this is "to set God on one side in the name of Universal Evolution."

It is unfortunate that those who take this view of the general scope of modern scientific research had not been careful at an earlier date to explain, when admitting the growth of a tree, a forest, or a flora—of an animal, a race, or a fauna, according to natural laws, and even explaining (as many of them did) the wonderful nature of the laws according to which such growths took place, that they wished it to be clearly understood that in thus recognising the action of law they were rejecting the idea that the Almighty fashioned the plant or the animal, the forest or the race, the flora or the fauna, or indeed, aught (animate or inanimate) the development of which man is able to study through all its stages. Because, if it had been clearly understood that wherever they recognised growth and development as the results of law, they were assured such results could not possibly be attributed to the Almighty, Science might perhaps, (though it seems unlikely) have been deterred from researches leading to the distressing conclusion that there is development according to law on the greater scale as well as on the less; nay, that to all appearance law prevails throughout the entire domain of the Almighty in space and during the entire period of time in which he acts—that is, throughout infinity of space, and during eternity of time.

As regards the actual evidence of the vastness of space and the immensity of time throughout which the action of law extends, it may suffice to say that only the very ignorant or the very dull can for a moment entertain doubt. Unless the evidence given by earth and heaven has been specially devised to mislead man, or unless the reasoning powers bestowed on man by God have been given but to lead him astray (conceptions alike blasphemous and unreasonable), there can be no manner of doubt that on the one hand the universe is infinitely larger than it was supposed to be before the days of Copernicus and Kepler, Galileo and Newton; or that, on the other hand, our earth has lasted, and will last, thousands of times as long as had been supposed before its structure had been examined; the solar system millions of times as long as had been supposed before its movements had been studied; the galaxy of stars yet longer; the higher order of systems to which that galaxy belongs for periods so vast, that to all intents and purposes they extend (in our conception) to absolute eternity—in the past as in the future.

As to the influence which a result such as this should have upon men's minds, it should perhaps suffice to say that those who believe that the Almighty is all-wise as well as all-powerful ought not to fear lest the discovery of truth from the study of His universe should produce evil effects.

But I go much further than this, and say that of all possible forms of teaching, those derived from or based upon science must be most beneficial in the religious sense, not using the words science and religion in their ordinary narrow significance, but in their widest and noblest. "Doubtless," as Herbert Spencer has well said, "science is antagonistic to the superstitions that pass under the name of religion; but not to the essential religion which these superstitions merely hide. Doubtless, too, in much of the science that is current there is a pervading spirit of irreligion; but not in the true science which has passed beyond the superficial into the profound." Or, as Huxley has even more pointedly remarked, "True Science and true Religion are twin-sisters, and the separation of either from the other is sure to prove the death of both. Science prospers exactly in proportion as it is religious, and religion flourishes in exact proportion to the scientific depth and firmness of its basis. The great deeds of philosophers have been less the fruit of their intellect than of the direction of that intellect by an eminently religious tone of mind. Truth has yielded herself rather to their patience, their single-heartedness, and their self-denial, than to their logical acumen." To which may be added the noble saying of Carlyle, that "to know the Divine laws and harmonies of this universe must always be the highest glory of a man, and not to know them the greatest disgrace for a man."

But we may fairly go even further than this. We need not be content to defend, or merely to justify, or even to laud, Science in its relation to Religion. We may assert without fear of valid contradiction that the neglect of science is irreligious. For what is such neglect (where men have time and leisure for the work) but the refusal to study the works of the Creator? And in what position, logically, does a man stand who praises the Creator in words, but declines to study His creation? "Suppose," says Spencer, "a writer were daily saluted with praises couched in superlative language. Suppose the wisdom, the grandeur, the beauty of his works were the constant topics of the eulogies addressed to him. Suppose those who unceasingly uttered these eulogies on his works were content with looking at the outside of them, and had never opened them, much less tried to understand them. What value should we put upon their praises? What should we think of their sincerity? Yet, comparing small things to great, such is the conduct of mankind in general in reference to the universe and its cause."

The study of science implies the surest belief that God's works are worth study, the fullest recognition that the author of those works is worthy of our reverence. It is the truest kind of homage, in that it is not homage expressed merely in words, but homage shown in work, in service, in sacrifice. The man of science, in fine, refuses to offer to the Almighty "the unclean sacrifice of a lie." He offers him instead (in the search for truth) the sacrifice of time, of labour, and of thought. His very questions imply the fulness of his faith:—

> *This* is his homage to the mightier powers,
> To ask his boldest question, undismayed
> By muttered threats that some hysteric sense
> Of wrong or insult will convulse the throne
> Where Wisdom reigns supreme.

THE RELATION OF FOOD TO MUSCULAR WORK.

By Dr. W. B. Carpenter, F.R.S.

[In an article on the "Use and Abuse of Food," republished in my "Pleasant Ways in Science," there is a passage in which Liebig's mistake about the relation between nitrogenous or flesh-forming food and work is quoted without being corrected. I was not aware when the article was written (1867) that scientific experiments were in progress which were eventually to completely expose the fallacy of Liebig's position. When the article was republished these experiments had long since been brought to a satisfactory issue. Although the point does not importantly affect my essay regarded as a whole (for the material of a machine, as well as the source of its working energy must be kept in repair, and the workman does not get less good from his food because he takes it under a misapprehension as to the particular benefit it will do him), the mistake is one which should have been corrected. The interpreter of scientific statements, too technical for general comprehension, must not be content with presenting correctly and intelligibly the accepted teaching of an authority in any special branch of science. He must assure himself, as time passes, that the teaching which was regarded as sound when the subject was first dealt with, has not undergone correction in the mean time. If I had done this in the present case (as I have in general been careful to do), the error in question would not have appeared in the pages of "Pleasant Ways in Science." The following exceedingly interesting paper, by Dr. Carpenter, puts the matter in the proper light. He speaks, I need hardly say, "as one having authority." I may point out that it is one of the great advantages of a journal like the present that errors into which even the most careful will fall from time to time, will here be corrected at once. In the columns of a monthly magazine correspondence would be inconvenient, even if permitted. Here the freest questioning and discussion is invited, and it is particularly desired that those having special knowledge of a subject will call attention to, and correct, any statements which may appear to them erroneous.—Ed.]

PART I.

THAT "the evil which men do lives after them," is often exemplified by the continued prevalence of scientific doctrines accredited by the authority of great names, long after their fallacy has been demonstrated by the subsequent researches of other inquirers to the satisfaction of all competent judges. For, if these demonstrations be not presented to the world under the sanction of a like authority, the old errors are continually reproduced by popular expositors, and unquestioningly accepted by ordinary readers.

Having met with a notable instance of this kind not long ago, in the reproduction, as an accepted physiological verity, of the doctrine of Liebig as to the direct dependence of muscular energy on the expenditure of nitrogenous food, I think that a journal which aims to communicate positive "knowledge" to its readers may be an appropriate medium for a brief statement of what are now accepted by all scientific Physiologists as the facts of the case.

It is no derogation to the well-established fame of Liebig as one of the greatest Chemists of his day,* to affirm that when he passed out of his own domain into that of Biology, he made many and flagrant mistakes. Looking back after an interval of nearly forty years, at his "Organic Chemistry in its Relations to Physiology and Pathology," I am really astonished at the reckless audacity of some of his assertions; as, for instance (1st edit. 1842, p. 219), that "we know with certainty that the nerves are the conductors of mechanical effects, and that by means of them motion is propagated in all directions;" and that "the heart and intestines do not generate the moving power in themselves, but receive it from other quarters." He might just as well have said that "we know with certainty that when a charge of gunpowder or dynamite is exploded by an electric spark, it is the conducting wire that supplies the energy which rends asunder the rock." For nothing was even then more certain, than that the heart, intestines, and all other muscles furnish, in virtue of their own contractility, the power (or, as it would now be called, the "potential energy") which produces their mechanical effects, this being simply called into action by the nervous stimulus.

Another most noteworthy example presents itself in Liebig's denunciation of the "germ-doctrine" of fermentation and contagion, which was then being built-up on the basis supplied by the microscopic discoveries of Cagniard de la Tour on the fungoid nature of yeast, and of Audouin and Milne-Edwards on the like character of the Muscardine-disease of silkworms. "A theory," he says (3rd edit. 1846, p. 212), "of the cause of fermentation and putrefaction, which is utterly fallacious in its fundamental principles, has hitherto furnished the chief support of the parasitic theory of contagion. The advocates of this theory regard putrefaction as a decomposition of organic beings caused by infusoria and fungi, and consider every putrefying body as a breeding-place for infusoria or a nursery for fungi; and where organic bodies putrefy over a large surface, the whole atmosphere, according to this view, must be filled with the germs of these infusoria and fungi. The germs of these organised beings are, in this theory, the germs of disease or of the causes of disease." Yet it is this very doctrine, the complete establishment of which by the admirable researches of Pasteur, and of those who have followed in the line of inquiry which he so clearly marked out, has not only given the true interpretation of the phenomena of Fermentation, but, in its application to Pathology, is now serving as the basis for "preventive" medicine and "antiseptic" surgery. And I feel sure that Liebig himself, had he lived into the present era, would have been quite ready to admit its truth; for he was the last man to persist in views no longer tenable, merely because he had himself advanced them. "There is no harm in a man's making mistakes," he used to say, "but great harm in his *making none*, for he is sure not to have worked." And Professor Hofmann records having been exhorted by Liebig "not to keep in your house from night till morning an error you have become cognisant of."

Now Liebig's chemical division of food-materials into plastic, or "tissue-forming," and respiratory, or "heat-producing," was unquestionably an immense advance; and the basis of it is still universally recognised as sound. I can myself remember the time when it was a question whether the nitrogenous components of the blood, and the tissues formed at their expense, can be built-up in the animal body, with the aid of atmospheric nitrogen, out of starch, sugar, fat, and other non-nitrogenous hydrocarbons. Chemistry had not then shown the almost exact conformity of many Vegetable compounds to the albumen which was regarded as the fundamental constituent of Animal food; and while it was by Mulder that the doctrine of the *protein*-compounds was worked out, which, with some modifications, is now generally accepted, it was by Liebig that the impossibility of supposing that animals can form their tissues out of anything else than the "proteids" originally generated by Plants, was first definitely insisted on. He was not aware, however, that the formation of "protoplasm" requires *fat* as well as proteids; and that thus fat is to some extent a tissue-food. But he was unquestionably right in affirming that animal tissues cannot be manufactured out of *saccharine* compounds (as starch and sugar), unless these have been previously

* See "The Life-Work of Liebig," by Prof. Hofmann, the "Faraday Lecturer" for 1875.

changed into fat—a change whose possibility he went far to prove,—and then only in the same measure as fat itself. And he quite correctly assigned as the ultimate destination of the great bulk of the non-nitrogenous components of food, the being (so to speak) "burned" within the body, by combination with oxygen taken in through the lungs; their hydro-carbons being exhaled by these organs in the form of water and carbonic acid. But he assumed that the only purpose served by this oxidation was the production of *heat*, affirming that all the *mechanical force* exerted by the Animal body is the product of a transformation of *living* muscular fibre into *dead*, the "vital force" which was possessed by the living tissue being "expended in the shape of motion." The oxidation of the dead material he seems to have regarded as only a consequence of its loss of power to resist chemical agencies, and as a means of its removal,—serving, at the same time, as an additional source of heat. And rightly judging that this chemical metamorphosis should show itself by the increased excretion of urea in the urine (the kidneys furnishing the channel through which most of the nitrogenous "waste" is carried out of the body), he appealed, in support of his doctrine, to what he supposed to be the fact of such increase, and its proportion to the amount of work done.

Notwithstanding the general acceptance of Liebig's doctrine among the Physiologists of that time, there were those who saw that it was attended with considerable difficulties, notably as regards the amount of work done by man and animals upon a very small allowance of nitrogenous food. It was well known, for instance, that Bengalee labourers (who, for their size, are very fair workers*) live all but exclusively on rice, which consists almost entirely of starch; a pound of this, with a little ghee (butter), or a small bit of fish, constituting their ordinary diet. And it appeared from the carefully conducted experiments of Mr. Joule, of Manchester, that the quantity of work done by a grass- or hay-fed ox (taking into account that done by its heart *within* its body, as well as that done *outside* its body) was more than could be accounted-for by the metamorphosis of the nitrogenous constituents of its food.

Mr. Joule was at that time engaged upon an inquiry into the Mechanical equivalent of Heat, which led him to suspect that the combustive oxidation of the non-nitrogenous constituents of food might be a source of *mechanical power*, as well as of heat. But this doctrine was first definitely stated, and shown to be one expression of the great general fact (or law) now known as the Conservation of Energy, in the "epoch-making" treatise published in 1845, by Mayer; who urged (1) that the chemical force contained in the ingested food and in the inhaled oxygen, is the source of the motion and heat which are the two products of animal life; and (2) that the production of these forces varies in amount with the chemical changes to which it is due. The animal body, he maintained, is comparable, in these respects, to a steam-engine, in which work is being done, and heat produced, by the combustion of fuel; and he calculated that the quantity of carbon burnt off by the body in a day (as measured by the amount of carbonic acid given off in the expired air) is far more than sufficient for the whole day's work,—about four-fifths of it being used in the production of heat, while the remaining fifth suffices for the production of the muscular force ordinarily exerted within and without the body. He affirmed that "the muscle is the instrument by which chemical change is transformed into mechanical effect, not the material which is itself transformed;" and he regarded the blood stream that flows through the capillaries of the muscle, as bringing both the fuel and the oxygen needed to burn it. With the prevision which marks true genius, Mayer asserted that as soon as experimental methods should become sufficiently perfect to render it possible to determine with precision the amount of chemical change, either in the whole animal body or in a single muscle, during a given period, and to measure the production of heat and the work done during the same period, the result would show a definite correlation between them.

This (as will be shown hereafter) has been completely verified by subsequent research; the only point in which Mayer's doctrine has been found to need correction, being one of secondary importance.

(*To be continued.*)

* I have been informed by an engineer who superintended the construction of part of the Bengal system of railways, that the average quantity of earthwork done by a native labourer on the above diet is about *two-thirds* that of a highly-fed English "navvy."

ARE WOMEN INFERIOR TO MEN?

A FRENCHMAN named Delaunay has recently published a pamphlet, the object of which is to show that women are intellectually and physically inferior to men. At the outset it should be noticed that this M. Delaunay must not be mistaken for the great French astronomer Delaunay, (who died some nine years ago). The point requires noting, because the present Delaunay has ere this dealt with statistical evidence relating to astronomical matters, endeavouring to prove therefrom that the planets Jupiter and Saturn exercise a marked influence on the phenomena of earthquakes. It is hardly necessary, perhaps, to say that the proof was hardly so perfect as M. Delaunay imagined. In fact, his reasoning would not have attracted attention if his name had not sounded like that of a very eminent astronomer, of whose death many had not heard.

M. Delaunay now devotes himself to the task of showing that those who regard the two sexes as intellectually or morally equal, are absurdly sentimental. He seeks to show that women are in a lower stage of development than men, on grounds which he describes as purely anthropological, though he should rather, it would seem, have described them as biological, since his inferences respecting the tests of development are derived quite as much from the study of other animals as of men. He admits that among certain lower forms of life, as insects, some fishes, and reptiles, the females show a superiority to the males; but among the higher races of vertebrates (backboned animals) it is different. Among birds and mammals (he might have added marsupials, or pouched animals, as kangaroos, opossums, &c.), the male is nearly always superior to the female. The inference is, that whereas in lower races the female is superior to the male, the male is equal to the female in races more advanced, and superior to the female in all the superior species. "The supremacy of the female is *therefore*," he says, "the first form of the evolution undergone by sexuality, while the supremacy of the male is the last form." The conclusion is calculated to be so satisfactory to men that they may be permitted, perhaps, to pass over the manifest weakness of the reasoning. The law, which should be universal to be of avail, is admitted to be only general; and it is noteworthy that the reasoning really points to the conclusion that the higher the race, the lower relatively is the female: so that it would seem that the first

point to be established by one who would prove his nation in advance of others, should be the inferiority of the women of his race. If this were really so, we imagine that there are some races of men, not supposed to be far from the very forefront of human progress, who would not care, at such a cost, to establish their claims to a high position.

M. Delaunay is careful to show why he thinks the female inferior to the male in the higher races. First, we find that among birds and mammals, including man (who shares with the pig, the monkey, the dog, &c., the privilege of breast nourishment), the nutritive phenomena are more intense than in the female. Man eats more than woman; and he is, therefore, incontestably her superior. "Yet, although she eats less, the woman is more of a *gourmande*, and eats more frequently." (This may be proved by comparing aldermanic feasts and club dinners with the favourite forms of feminine gourmandism.) "Next we observe that the breathing of men is more intense than that of women. For an equal height he has a greater capacity of chest, and a larger thoracic index than the woman" (a greater breathing capacity). "He also absorbs more oxygen, though his breathing is slower." (Why should not this be regarded as a proof of inferiority? Suppose a woman were writing, and should put it this way, Woman requires less oxygen than man, and is therefore superior to him; how then?) "The temperature is higher in man than in woman, and the pressure of the blood greater, though the pulse is less frequent." This, again, might be quoted the other way by women. It would be almost as logical to say, Man is superior to woman because he wears a beard, and the hair on his head is shorter than hers.

It appears that the frame of man, as compared with that of woman, proves incontestably his superiority. He has a heavier skeleton, absolutely as well as relatively. "The woman in all the physical characteristics of her skeleton is intermediate between the child and the man, according to Topinaud." Must we then rank the elephant as man's superior? his skeleton is certainly heavier than man's, not only absolutely but relatively. "The woman is not so right-handed as the man; the pre-eminence of the right side over the left is not so marked with her as with the male." Charles Reade would probably regard this as evidence of superiority rather than inferiority; and many men who have not been thought inferior to their fellows, but the reverse, have been at no small pains to train the left side to equality, or as near equality as they could attain, with the right side.

Worse remains, however, to be told. "The male externally is always larger than the female. The woman is not so heavy as the man, although she would often appear larger on account of the development of the adipose system, which in her is greater than the man."

The next point, if established satisfactorily, would be rather more to the purpose. "In all our Hindo-European races," says Topinaud, "the woman is more prognathous than the man," that is, her jaws are relatively more projecting than those of man. This would be a strong point, because there really appears to be a correlation between the position of the jaws in animals and the capacity (relative, of course,) of their brains. But it would be desirable to have all the particulars by which the peculiarity referred to has been established. It would be easy to select classes either of men or of women who would be very unfair representatives of their sex taken as a whole; and though I would be far from saying that anything of this sort has been done, I would note that it is a much more difficult matter than many would suppose (who have not tried) to obtain a large number of either sex who could be regarded as absolutely free from class influences. The task would be easier if men and women pursued similar avocations. As they do not, the statistics collected by Torpinaud may, for aught that appears, have tended, if rightly interpreted, to show that such and such avocations either affected intellectual development, or were selected by persons of inferior intellect, rather than anything specifically distinguishing men from women."

A similar objection applies, but yet more strongly, to M. Delaunay's next point. He says that, "according to his own researches, woman is more flat-footed than man, and has a foot less arched (a sign of inferiority); hence the preference of women for high-heeled boots." It is a rather bold assumption that the form of the feet thus indicates intellectual development. But assuming that it were so, I apprehend that the statistics of feet-forms would show very different results according to the classes that might be selected. Supposing, for instance, M. Delaunay had measured the feet of a few hundred waiters, and compared them with those of a few hundred opera-dancers, he might, perhaps, have inferred either that opera-dancing conduced far more than waiting at table to intellectual development, or else that women were far superior to men. I do not say that a selection so unlikely to lead to a correct result as this has actually been made; but we require to know much more than we do about M. Delaunay's statistics before we can accept his conclusion, even if we admit that the mental and moral qualities can be deduced from the shape of the feet. The fancy for wearing high-heeled boots may reasonably be regarded as showing that women regard a high instep as a natural feminine beauty, to be enhanced where present or imitated where wanting; that, in fact, women are more particular in this respect than men. So viewed, it would no more prove that women are commonly flat-footed than their wearing chignons or switches would show that they commonly have shorter hair than men. In fact, M. Delaunay's argument here is decidedly unfavourable to his theory, if a high instep really is a proof of intellectual superiority; for women try more to enhance those qualities which they regard as feminine, and therefore attractive, than those which they share with men.

"The female voice is sharper than that of the male. Both in wild and domesticated animals the male has the muscular system more developed than the female." All this may be granted; but it proves nothing to the purpose. The bull has a deeper voice than man, and the gorilla has the muscular system far more developed; but we do not infer that the bull or the gorilla stands higher in development on either account than man.

"The movements are more precise in man than in woman." (What movements?) "Among pianists of the two sexes the mechanism reaches a much higher degree of perfection in men." As musicians, whether we consider composition or execution, women certainly are surpassed by men; and I may note among the absurdities of our system of education that almost every girl in the middle and upper classes "learns the piano" (save the mark!) though not one in a hundred has any natural aptitude for music, while among boys music is comparatively neglected. There is little, however, in feminine execution to justify the statement that their movements are less precise. On the contrary, numbers of girls acquire wonderful precision in playing; only, unfortunately, most of them want that aptitude for music which can alone make mere perfection of digital mechanism of any account. It is not easy, however, to see why musical skill should be regarded as a test of mental or moral qualities. I am disposed, on the contrary, to consider that women in our day are handicapped by their musical education; and that pro-

bably, if so many were not compelled (without musical talent to help them) to waste many years of their life in the weary work of "practising," we might have better opportunities of learning what women are capable of in other ways than we are actually afforded. Music, at any rate, is one of the weak points of modern feminine education, if not its worst feature. Even as musicians, women would be far likelier to show power, if only those who possessed aptitude for music received a musical training. How can the real musician among fifty girls get any chance, when she has to go through, with the forty nine who are not musicians, the weary music killing work of our present system of so called musical training? (Is not this matched, however, by the existing systems of classical and mathematical training for boys, irrespective altogether of any aptitude for classical or mathematical work?)

We come next, in M. Delaunay's paper, to the more important question of cranial capacity and form: for certainly (setting aside, of course, phrenological absurdities), the shape and size of the brain are more likely to indicate mental and moral capacity correctly, than are the shape of the feet, the tone of the voice, and the muscular development. As, however, my limits are already exceeded, I leave the rest of M. Delaunay's paper to another occasion.

So far as we have gone, M. Delaunay's arguments remind us, more than we should have thought possible in a scientific *brochure*, of the reasoning in a humorous article which appeared a year or two ago in the *New York Times*, wherein it was gravely argued that the inferiority of woman to man is proved conclusively by women's habit of sitting on the ground to take off, or to put on, their shoes and stockings. In fact, I am not sure that M. Delaunay might not find more in favour of his theory in this argument than in any of his own that have thus far been considered.

COMETS.

THE year which was to have seen the end of the world, because of planetary conjunctions and perihelion passages, because Mother Shipton had said so (or was said to have said so), and because the ascending gallery in the Great Pyramid is 1882 inches long (so that the year 1882 is to introduce a new era), has been remarkable in astronomical annals for the number of comets which have been seen. Already six have been numbered, and the year is not over yet. Something still remaining—more, indeed, than we are always ready to admit—of old superstitions respecting comets, has led many to regard the coincidence as full of meaning. Others, not quite so credulous, have supposed that though comets *may* not come in flights of half-a-dozen together to portend the end of the world, they may yet affect our weather in some way; perhaps directly, as the moon is supposed to do (with very little reason); perhaps indirectly, by acting on the sun. To the astronomer the appearance of so many comets—some of them large ones—has been full of interest, because he hopes by the application of the new methods of research discovered within the last quarter of a century to solve some of the mysteries with which the whole subject is still fraught, despite a number of interesting discoveries which have recently been made.

A brief inquiry into some of the facts which have been discovered respecting comets, and a discussion of some of those peculiarities which still remain among the greatest mysteries of science, will probably prove acceptable at the present time, when comets attract so much interest and attention.

Elsewhere in the solar system we meet with relations not differing greatly in kind from those presented by our own earth. We see a set of globular bodies revolving around the sun in nearly circular orbits, nearly in one plane, and all in the same direction; we find that these globes rotate upon their axis—still in the same direction; they have, apparently, atmospheres proportioned to their dimensions; and many of them are attended upon by bodies resembling our own moon. And therefore, without entering upon the vexed question of the plurality of worlds, we are able to pronounce that, *if* these globes are inhabited, dwellers upon them have, like us, their years, their days, their seasons; a sun rising in the east and setting in the west; twilight and moonlight; air and vapour; winds and rain; all things, in fact, as it would seem, necessary to their comfort and convenience. Here and there—as in the zone of asteroids and the rings of Saturn—we meet with novelty of arrangement or configuration; but even then we find a stability, either of figure or motion, which renders such objects comparable, so to speak, with those we are accustomed to.

But with comets the case is wholly different. When we have said that these objects obey the law of gravity, we have mentioned the only circumstance—as it would appear—in which they conform to the relations observed in terrestrial and planetary arrangements. And even this law—the widest yet revealed to man—they seem to obey half unwillingly. We see the head of a comet tracing out systematically enough its proper orbit, while the comet's tail is all unruly and disobedient.

The paths followed by comets show no resemblance either to the planetary orbits or to each other. Here we see a comet travelling in a path of moderate extent and not very eccentric; there another which rushes from a distance of two or three thousand millions of miles, approaches the sun with ever-increasing velocity until nearer to him than parts of his own corona (as seen in eclipses), sweeps around him with inconceivable rapidity, and makes off again to where the aphelion of its orbit lies far out in space beyond the most distant known planet, Neptune. Some comets travel in a direct, others in a retrograde, path; a few near the plane of the earth's orbit, many in planes showing every variety of inclination. Some comets regularly return after intervals of a few years; some after hundreds of years; others are only seen once or twice, and then unaccountably vanish; and not a few show by the paths they follow that they have come from interstellar space to pay our system but a single visit, passing out again to traverse we know not what other systems or regions.

The ancients believed comets to be of the same nature as meteors, or shooting stars—either in the earth's atmosphere, not far above the clouds, or, at all events, much lower than the moon. These views are, however, much less ancient than the more correct views maintained by the Pythagoreans. Their doctrine was that comets are planetary objects, having long periods of revolution. From whom this opinion was derived is uncertain. Like other opinions attributed to Pythagoras, it was doubtless obtained from Eastern philosophers; but of what country—whether Egyptian, Persian, Indian, or Chaldæan—we have no means of learning. Apollonius, the Myndian, ascribes the opinion to the Chaldæans. He says they spoke of comets as of travellers penetrating far into the upper (or most distant) celestial spaces. Seneca and Pliny held similar views, exhibiting in this respect, says Humboldt, the imitative faculty of the Romans. But the Greek

philosopher preferred to look for a theory of the universe in the conceptions of his own brilliant and imaginative mind. As if to show future ages how little was likely to be achieved by the highest mental powers without the habit of patient observation, he endeavoured to educe a system of philosophy from fancies, and to found it upon syllogisms. Aristotle—who may be considered the typical philosopher of the Greek school—included comets in the wide range of phenomena which he claimed the privilege of explaining. To him was due the opinion mentioned above—an opinion confidently maintained during the many centuries in which the philosophy of Aristotle held sway over men's minds. To him, also, was due a yet more remarkable opinion, the view, namely, that the Milky Way is a vast comet which continually reproduces itself! Xenophanes and Theon, in the fifth century, adopted a rather singular view of the Aristotelian theory of comets, when they spoke of these objects as "travelling light-clouds."

To these fancies the ancients added the idea that the shapes of comets indicated their character as portents. Thus in Fig. 1 five views of comets are shown, as an arrow-head, a sea monster, a sword, a lance, and in flames.

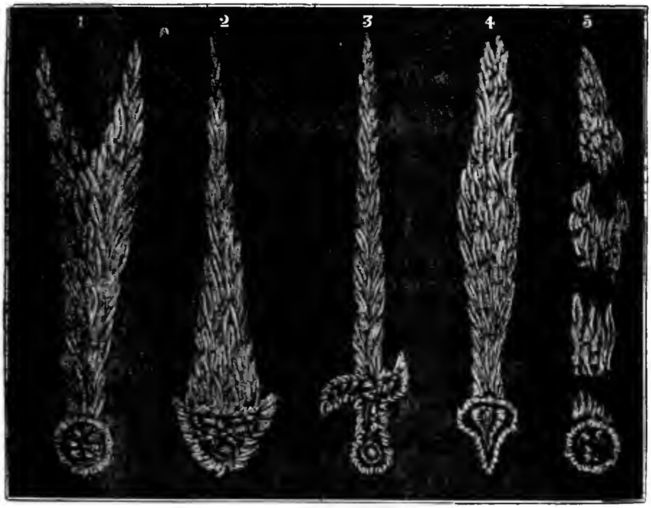


Fig. 1.—Various fanciful views of Comets, according to Pliny. From the *Cometographia* of Hevelius.

Tycho Brahe was the first to express doubts respecting the views of Aristotle. From a careful series of observations, he demonstrated that the orbits of comets are certainly situated beyond the moon's orbit. He thought the orbits must be circular, for he lived at a time when none but circular orbits were conceded to the celestial bodies. Dörfel, a native of Upper Saxony, proved that the orbits of comets are either very elongated ovals, or parabolas, and that the sun occupies a focus of the curve. It happens, singularly enough, that this discovery was effected but a year or two before Newton propounded the theory of gravitation. Newton himself examined the orbit of the great comet of 1680 (known as "Newton's comet") and others; and he found that they all accord with the law of gravity.

But before long, Newton's friend and pupil, Halley, effected a yet more remarkable discovery. In hopes of confirming Newton's views by results founded on actual observation, he collected all the records of comets which seemed entitled to confidence, and attempted—as well as his meagre materials would allow him—to calculate the elements of their orbits. In this way he computed the paths of no less than twenty-four. Among these, three presented a remarkable similarity. One appeared in 1531, and was described by Appian; another appeared in 1607, and was observed by Kepler; the third was traced by Halley himself in 1682. The equality of the intervals between these epochs led to the suspicion that the same comet had appeared three times. And Halley found, on searching historical records, that a comet appeared in 1305, another in 1380, and a third in 1456. Combining these appearances with those mentioned before, he thought he had satisfactory evidence of identity. For he was sufficiently familiar with the results which might be expected to flow from the law of gravity, to be aware that *absolute* regularity of motion was not to be expected in a body traversing the solar system in an eccentric orbit, and swayed from its proper path by the attraction of such giant planets as Jupiter and Saturn. Indeed it happens, singularly enough—one out of many remarkable coincidences in the history of comets—that the comet of 1830 was *not* Halley's comet, which really appeared in 1378, a date bringing in a yet greater discordance in the intervals than Halley had suspected and accounted for. With remarkable acumen—since no means existed in his day for anything like accurate computation—he not only pointed out the possible influence of the great planets in disturbing the comet in past revolutions, but he made a rough approach to an estimate of the effect that they would have on the period of its next visit. "Instead of appearing in August, 1757, as it would if its period remained unaltered, it will not appear," he said, "until the end of 1758, or the beginning of 1759, for it will be retarded by the action of Jupiter. Wherefore," he adds, with a pardonable anxiety to secure the credit of his ingenious investigations, "if it should return, according to our prediction, impartial posterity will not refuse to acknowledge that this was discovered by an Englishman."

As the time for the fulfilment of the prediction approached, an intense interest was excited in the minds of astronomers. In 1757, Clairut, Lalande, and Madame Lepaute undertook the computation of the epoch at which the comet might be expected to return. They applied methods of investigation invented by Clairaut himself. It resulted from their laborious computations that April 13, 1759, was fixed on for the epoch at which the comet should attain its closest approach to the sun, or, as it is technically expressed, should pass its perihelion. But Clairaut was careful to allow a month either way, on account of unavoidable omissions in the calculation, and for the effects of unknown forces, "such as the action of some planet too far off to be seen" (a happy anticipation of modern discoveries).

And now the heavens were swept diligently by all the telescopists of Europe, each eager to be the first to

announce the discovery of an object whose appearance or non-appearance was to confirm or to disprove the Newtonian theory. It was actually discovered, however, without telescopic aid, by a Saxon farmer, George Palitsch, on Christmas-day, 1758. It reached its perihelion on March 13, 1759, confirming at once the accuracy of Clairaut's computation, and the justice of his caution in assigning rather wide limits of error.

It was now evident that comets travel, like the planets, in determined paths; and also, that the investigation of their motions is a subject worthy the study of the ablest mathematicians, and sufficient to tax their highest powers. An account of their labours would be out of place in such an article as the present; but we recommend the subject to the notice of the agricultural student, as one of the most interesting chapters in the history of modern science.

One comet, however, discovered not long after astronomy had achieved this triumph, seemed at first to teach a different lesson. In 1770 a comet appeared whose path turned out to be not a long oval or parabola, as had been the case with all the orbits yet examined but an ellipse of moderate extent, and not very eccentric. The orbit lay also much closer than usual to that thin slice of space (so to speak) within which the planets are observed to move. Lexell, who computed the path, found that the period of the comet was about five-and-a-half years. Its return was carefully watched for, *but no one has ever seen the comet since.* The cause of its disappearance, and also of its sudden appearance—for this was equally remarkable, when we remember that so conspicuous a comet could not have been circulating long in its small orbit without discovery—was carefully inquired into. The result was singular. On tracing back the path of the comet, it was found that it must have passed very near to the great planet Jupiter. "It had intruded," says Herschel, "an uninvited guest into his family circle—actually nearer to him than his fourth satellite." Accordingly, the comet's path, originally a long oval, had been bent into a curve of less extent. Having once entered on this new path, the comet was free to follow it—always returning, be it noticed, to the point at which it had started on it—so long as Jupiter was not interfered with. But it happened, unfortunately for the stability of the comet's motions, that, after going twice round the new path, it again presented itself near Jupiter's track, when the planet (which had meanwhile gone once round his orbit) was not very far from the scene of his former encounter. He accordingly again exerted his influence upon the unfortunate comet, and this time dismissed it on a path which will not admit of its approaching the earth near enough to be seen.*

Let us return, however, to Halley's comet.

It so chances that the comet which was the first to show full obedience to the law of gravitation, was one which exhibited in a very remarkable and significant manner the characteristics which distinguish comets from other heavenly bodies, and make them so mysterious to the student of science. At the return of Halley's comet, in 1836, all that had signalised the return in 1759 was repeated, but the mathematical triumph was far greater. Damoiseau, Rosenberger, and Pontecoulant calculated the comet's return to perihelion within two or three days, instead of a month, and the time when it passed this point of its orbit corresponded, within a few hours, to the mean of their several estimates. On the northen heavens where it was first seen, the comet presented a remarkable appearance, with a long and brilliant tail stretching over an arc of many degrees upon the sky. When it had passed from our northern skies, it was carried (after a short interval, during which it was lost to view in the sun's rays) to the southern heavens. Sir John Herschel, and Maclear (Astronomer Royal at the Cape), were prepared to receive it; but when first observed by them it showed none of the features which made it so remarkable in our skies. It had no tail and scarely any head. In fact, Sir John Herschel, in one account, says, that as first seen it could only be distinguished from a fixed star by its motion. The study of its gradual change of aspect from that time threw so much light on the nature of comets' tails and other appendages (or at any rate of that particular comet's tail) that Sir John Herschel, not accustomed to be over confident, said there could be no doubt as to the true interpretation of the observed phenomena. What these phenomena were shall be considered further on.

* It must be noticed, however, that Leverrier, who very carefully re-examined the question, was led to question the accuracy of the results recorded above. Admitting that Jupiter had twice disturbed the comet, he thinks there is no certainty (for want of sufficiently accurate observations) respecting either the original path of the comet, or that in which it is at present circulating unobserved—if, indeed, it has not been *absorbed* by Jupiter.

ILLUSIONS.

By Thomas Foster.

THE senses are the means by which, directly or indirectly, all observations are made, and science can only make real advance in so far as it is based on observation and experiment. It is most important, therefore, that either our senses should be trustworthy in their action, that is should give us true information, or (if they neither are absolutely trustworthy originally nor can be so trained as to become so) that we should be able to test and to correct their indications.

Now it very soon appears, when we put the matter to the test, that the direct evidence of the senses is not to be accepted without careful cross-examination. The science of our day may be regarded as having been established in opposition to the apparently obvious evidence of the senses. Take, for instance, astronomy. Nearly everything that the eyes tell us about the heavenly bodies, and nearly all that the sight and touch tell us about the earth (so far as astronomy has to deal with the earth as one of the planets) is false. Not one of all the stars we see in the skies is really where we see it. The earth seems flat, large, and fixed; it is really a globe, small compared with the seemingly small stars, and it is moving in many ways, not one of which the senses correctly appreciate. It is the same with other sciences.

We are not concerned, however, to discuss here how far the apparent teaching of the senses has to be analysed before its real meaning can be understood. The examples illustrating this would cover the whole range of science. For instance, to show how the real place of a star can be determined—more or less exactly—from its apparent place in the sky, we require to discuss the laws of refraction, aberration, the proper motion of stars, and a number of other matters. In such cases as this, though what the eye tells us is in a sense incorrect, the eye is supposed to do its work correctly. The eye tells us *truly* that the rays received from the star by it have come in such and such a direction, and what science has really to do is to determine in what direction those rays must have set out in order after various changes of direction, due to the various media through which they passed, to reach the eye situate on a moving and rotating body like the earth, in the direction which they had, or at least seemed to have—or, more

strictly, in order to produce an image of the star on such and such precise part of the retina.

But there are many cases in which the senses seem actually to convey false information, the eye telling us wrongly about the shape, size, position, &c., of an object, the touch deceiving us as to its form and qualities, the hearing, the smell, the taste, each in its own way deceiving us. It is such cases as these that I propose to examine. It is most important for the student of science that he should be aware of the various forms of error into which the direct action of the senses may lead him. I could cite instances where, for want of precisely such information, the student of science has been at the pains to explain a phenomenon which had no real existence, or which was precisely the reverse of reality. Mr. Proctor mentions somewhere a case where a French astronomer (Chacornac), was careful to explain *why* the edge of Jupiter's disc is brighter than the middle, the fact being that the middle is brighter than the edge, the apparent brightness of the edge being a mere delusion. It may be added that the discussion of the class of illusions referred to is full of interest. The reader will find that the careful consideration of the cause of illusions will generally suggest other illusory experiments, sometimes more striking than those here described. There are few occupations more interesting as pastimes, and at the same time more instructive, than the invention and testing of various forms of illusion.

It should be mentioned at the outset that such illusions as these are wrongly called sensory illusions. As Huxley points out, "there is no such thing as a fictitious or delusive sensation. . . . . But the judgments we form respecting the causes and conditions of the sensations of which we are aware, are very often erroneous and delusive enough; and such judgments may be brought about in the domain of every sense, either by artificially-contrived combinations of sensations, or by the influence of unusual conditions of the body itself." He adds, "mankind would be subject to fewer delusions than they are if they constantly bore in mind their liability to false judgment. Men say, 'I felt,' 'I heard,' 'I saw' such and such a thing, when, in ninety-nine cases out of a hundred, what they really mean is, that they judge that certain sensations of touch, hearing, or sight, of which they were conscious, were caused by such and such things." It is precisely this lesson which I want to enforce in the present series of papers.

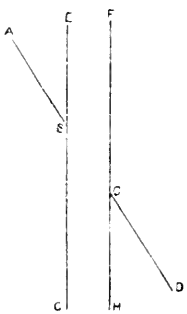


Fig. 1.

The simplest illusions are those affecting straight lines, causing them to appear crooked, or curved, or *vice versa*.

Take, for instance, the case illustrated in Fig. 1. Here the parts of the broken line AD appear to be parallel, but not—as they really are—in the same straight line. The part CD seems lower than the true prolongation of the part AB. The explanation of this is not so obvious as in some less simple cases which I shall consider further on. Manifestly, the mind recognises that, owing to the interposition of the space EH, the point where the prolongation of AB should appear must be lower down than B, but it underestimates the allowance to be made on this account. This is equivalent to saying that the eye under estimates the breadth of the strip EH. Why this should be? It seems to me it is because the space EH appears to lie above the plane containing AB and CD, so that the real breadth of EH is unconsciously judged to be less than it really is: for, of course, a strip nearer the eye than EH is would *seem* broader than EH if really of the same breadth, and if seeming *no broader* than EH, would really be of less breadth. The eye judges that EH *is* nearer, and infers unconsciously that it is of less breadth than EH really is.

Let us test this explanation, first seeing whether a strip which breaks a curve, as EH breaks the straight line AD, really seems nearer to the eye than the curve.

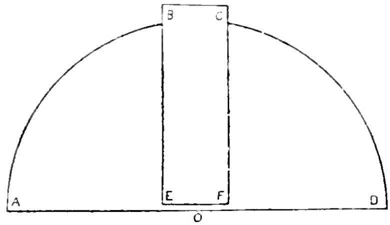


Fig. 2.

Describe a semicircle AD (Fig. 2) broken at BC, and between B and C draw the outline of strip BF as in the figure. We see at once that the space BF appears to be nearer the eye than the portions ABE, CFD. Doubtless the judgment unconsciously reasons that the strip which breaks the outline ABCD must lie upon the semicircle and hide the portions wanting.

Again, it seems to me and to some others, but not to all to whom I have shown Fig. 2, that the figure formed by carrying on the curves AB, DC, to meet over space BC would not be a complete circle, but somewhat contracted horizontally. This would correspond with the above explanation of the illusion illustrated in Fig. I. As, however, all eyes do not recognise this second illusion of Fig. 2, let us modify the experiment, noting that the same eyes which might not recognise an illusion apparently affecting breadth only, would readily be affected by an illusion affecting direction. (We may remember what Brewster points out in dealing with one of the illusions mentioned in his Natural Magic, that in many cases it is the most observant eye that is affected by an illusion. In the present case the eye which has the best power of estimating breadths would be the one most affected by illusion as to the breadth of the space BF.)

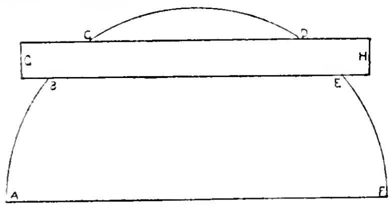


Fig. 3.

Draw then the strip GH, Fig. 3, crossing the semicircle ABDF as shown. Here the curve CD seems to belong to a smaller circle than that of which it really forms part. It looks as though the curves AB, FE caried on beyond the strip GH would lie about considerably outside CD.

It will be found that if the parts CD, BE of the sides of

the strip GH are erased, the illusion ceases. The erasure restores, as it were, the space GH to the level of the paper to which it really belongs.

The case illustrated in Fig. 1 is commonly regarded (but without sufficient reason, or, indeed, any assigned reason) as belonging to the class of illusions illustrated in Fig. 4 (first noticed by Zollner). Here the lines AB, CD, EF, GH, &c., which are really parallel, appear to converge alternately towards AC, DF, EG, HL, and KM. This illusion is different from that of Fig 1, as affecting the apparent directions of lines, whilst the other does not (parallel lines are regarded by geometricians as in the same direction).

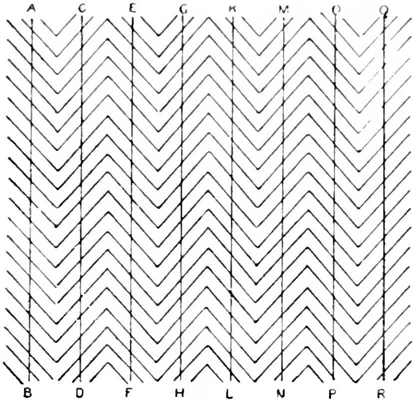


Fig. 4.

The illusion of Fig. 4 appears to result from the attention being drawn unduly to the circumstance that the vertical lines pass athwart the other sets at an angle, so that the angle is, as it were, exaggerated. The eye notices, for instance, that AB passes the parallels in order from right to left, the uppermost parallel crossed by AB being farther to the right than the lowest, and thus the idea is conveyed that B, instead of being a point vertically below A, is to the left of such a point. So D seems to the right of a point vertically below C, and so on.

The illusion of Fig. 4 admits of being varied and, in some cases, strengthened by substituting waving lines of various forms for the diagonal hatching. Several cases of this kind will be shown in the next number. I conclude this paper with a few simple illusions, affecting not only the direction and position of lines really straight, but also their straightness, making them appear as curved lines.

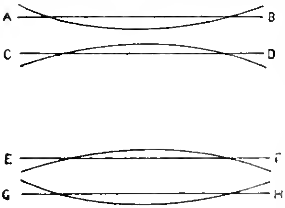


Fig. 5.

The lines AB, CD of Fig. 5 appear to be curved so as to be nearer at the middle than at either end; while the lines EF, GH, in the same figure, appear to be so curved as to be farther apart in the middle than at either end. Obviously, the eye unconsciously notes the varying distance between the really curved lines and the straight lines, and attributes part of the change of distance to a curvature of the really straight lines.

This form of illusion may be modified, as shown in Fig. 6. Some find the apparent curvature produced by the arrangement of Fig. 6 stronger than the other. For my own part, I find the apparent curvature of the lines AB, CD strongest in Fig. 5, that of the lines EF, GH strongest in Fig. 6.

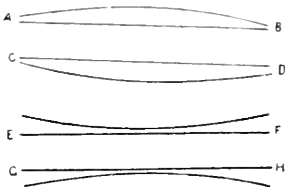


Fig. 6.

The illusion seems intensified in the case of lines AB (Fig. 7), which seem to be convex towards each other; the symmetry of the pair of curves lying between these parallels ought theoretically to restore the idea of parallelism. The lines EF and GH in the same figure may be made to seem either parallel or concave toward each other, according as the eye takes together the curves which approach EF and GH (respectively) nearest, or the concentric pairs; for the two curves which lie between EF and GH produce opposite effects on each of the lines EF, GH.

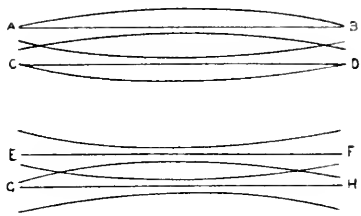


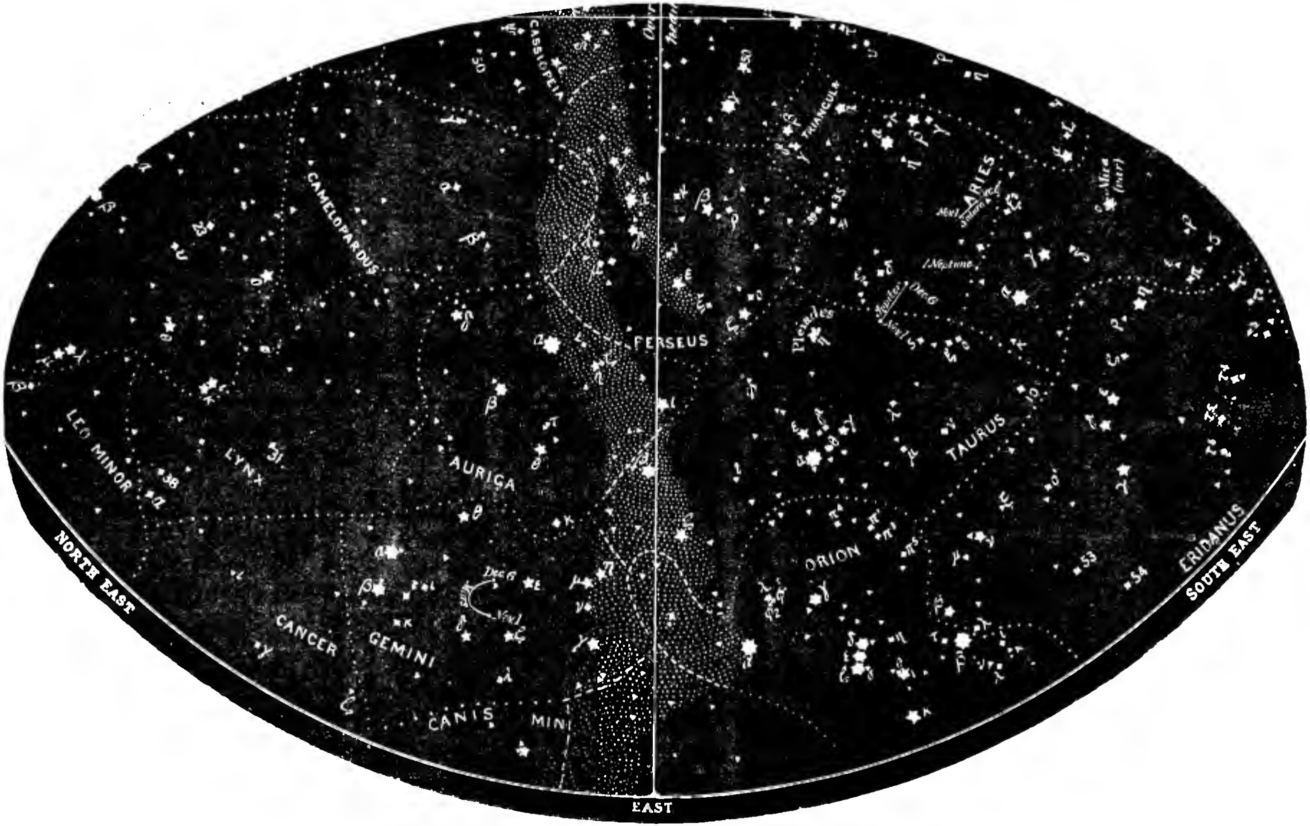
Fig. 7.

Next week I shall consider other cases of apparent curvature, but in the meantime I may note that I shall be glad to hear from readers who have noticed illusions akin to these, or may have been led to other explanations than those I have suggested.

What is a man,
If his chief good, and market of his time,
Be but to sleep and feed? A beast, no more.
Sure, He that made us with such large discourse,
Looking before and after, gave us not
That capability and godlike reason
To fust in us unused.—*Shakespeare.*

Electrical Exhibitions and the Government.—Our correspondent probably reflects the general opinion of British exhibitors in Paris when he says that "the conduct of our Government has prevented our country from holding the place it deserved to hold among European nations in an industry which promises before long to be one of the utmost importance." But it is perfectly well known to foreigners, and also to ourselves, that the British exhibitor, so to speak, must swim without corks. He may not make so large a show as some of his rivals; but what he does show is even more likely to be over-estimated, as being the result of his unaided efforts, than to be undervalued in consequence of any unfair comparison with those who have enjoyed advantages which he does not possess. The difference between the conditions which surround British and foreign exhibitors is thoroughly understood by all who are interested in understanding it; and we do not believe that a steady adherence to the general rule of Government non-intervention, although it may detract from the prettiness or completeness of a display, will ever be really detrimental to the interests of a national manufacture.—*Times.*

THE EASTERN SKIES IN NOVEMBER.



This Map shows the position of the stars in the Eastern Skies:—

On October 30, at 11½ o'clock. | On November 7, at 11 o'clock. | On November 14, at 10½ o'clock. | On November 22, at 10 o'clock. | On November 29, at 9½ o'clock.
On November 3, at 11¼ o'clock. | On November 10, at 10¾ o'clock. | On November 18, at 10¼ o'clock. | On November 25, at 9¾ o'clock. | On December 3, at 9¼ o'clock.

The movements of the planets Mars, Jupiter, Saturn, and Neptune, which are travelling in this part of the stellar heavens at present, are indicated by the lines marked respectively with the names of these planets. The planet Mars, it will be noticed, passes, during this month, what is called a stationary point; he is advancing (moving from right to left) till Nov. 17, after which he retrogrades. But he does not really come to apparent rest, owing to the wide spread of the loop he forms between his stationary points. Jupiter, Saturn, and Neptune are all three retrograding. They are in opposition, or exactly opposite the sun, so that they come to the south at midnight on the following days:—Saturn on Nov. 1, Neptune on Nov. 7, Jupiter on Nov. 13.

DARWIN ON MOULD AND WORMS.*

No man of science of our day understands better, or applies more thoroughly, than Darwin, the principle laid down by Lord Bacon, that "Man, as the minister and interpreter of nature, does and understands as much as his observations on the order of nature permit him, and neither knows nor is capable of more." To one who rightly apprehends this, the fundamental principle of modern scientific research, small things and great, so only that they illustrate the order of nature, are alike worthy of study. He may carry his survey over the depths of space, or into the structure of a microscopic creature; he may extend his view into the remote past and the distant future, or he may limit the range of his vision to phenomena taking place in a second or in a yet shorter time: but only in so far as his purpose is to determine the order of nature's works, is he the true minister and interpreter of nature. The modern student of science, following this principle, is in strong contrast with the philosophers of the Greek school, who, little disposed to pursue observations, evinced, as Humboldt has said, "inexhaustible fertility in giving the most varied interpretation of half-perceived facts;" and, as Bacon himself said, "Laid their whole stress upon intense meditation, and a continual exercise and perpetual agitation of the mind;" and so were led to frame systems on insufficient knowledge, and to explain false systems by false hypotheses. Doubtless, a philosopher of that school would have looked with contempt on a Darwin studying the movements of plants, the ways of bees, the breeds of pigeons, and analysing the play of features in joy or in sorrow, in anger or in pleasure. It would have seemed to him far worthier to educe from his moral consciousness ideas as to the true position of worms in the scale of being, than to devote years to the actual study of their ways and works. But by the humbler and more laborious method the student of science in our day manages to attain, or at least approach, the truth; the more brilliant philosopher of the olden school educed from his active mind multitudinous errors.

Darwin's latest treatise, on Vegetable Mould and Earthworms, affords perhaps the best illustration of his method of all the works that he has yet published. His "Origin of Species" and "Descent of Man" mark an epoch in science; but such a work as the present illustrates the way by which the new paths have been entered. It is true no one can read those epoch-making works without recognising in every page the kind of work on which their author's mind has been engaged while establishing his theories, or the tone of modern scientific thought. But results have there to be touched on which, in a work like his "Monograph on the Cirripedia," "The Movements and Habits of Climbing Plants," and, above all, the present treatise, are exhibited in detail.

Forty-four years ago Darwin announced the first results of his study of the formation of vegetable mould, in a paper read before the Geological Society of London. In the interval which has elapsed since then he has been accumulating the stores of knowledge about mould and mould makers which are presented in the work now before us.

Perhaps the most remarkable results of Darwin's observations is the "stupendous work" accomplished by creatures so small and weak. It was objected against the views which he published in 1837, that worms could not possibly bury to a depth of several inches fragments of cinders, burnt marl, &c., which had been strewn over the surface of meadow land. But now Darwin is able to speak confidently of their burial of the remains of Roman villas and pavements. He shows also how ancient encampments and tumuli have been gradually lowered by the agency of worms. Grass-covered slopes undergo perpetual denudation through their operations, the covering of grass remaining all the time intact, and even the inclination of the slope remaining unchanged. It may well seem incredible to the superficial reasoner, that creatures like worms,—small, weak, and soft-bodied—should produce such results; nay, results far greater in the course of time, changing as they do the entire aspect of a country. It is this inability, as Darwin well remarks, "to sum up the effects of a continually-recurring cause, which has often retarded the progress of science, as formerly in the case of geology, and more recently in that of the principle of evolution." When men like Sir John Herschel or Sir Charles Lyell have spoken of the effects of slowly-acting causes in modifying continents and seas, they have been ridiculed by the thoughtless, who cannot see how the downfall of rain, the slow movement of rivers, the play of waves on shore-lines, can produce such results. In like manner the biologist is ridiculed who, noting small changes in various races in short periods (or even in periods which to our conceptions seem long), points to the effect of such changes when multiplied during the lapse of those long periods of time of which the earth's crust tells us. But our author has shown how even creatures so tiny and weak as the coral animal have made large islands and long lines of sea-resisting reef, by constant labour; and now he shows how under our very feet the despised earthworm is changing the form and nature of the land we live on. When we learn that the rich dark mould in which vegetation thrives best, is made by worms, we see that not only the aspect of a country, but the condition of its inhabitants, and even its history, have been modified by their work. So that we may accept in its widest significance his remark that "it may be doubted whether there are many other animals which have played so important a part in the history of the world as have these lowly creatures."

The study of the habits of worms in this work is full of interest. As in nearly all the author's books, the language is clear and simple. It may be said indeed of this treatise, presenting the fruit of observations so long continued on a subject apparently so little promising, that great though its scientific value unquestionably is, it is better fitted than nine-tenths of our works of fiction to while away a weary hour. It merits, however, more than mere reading. It is a work not to be tasted merely, but to be chewed and digested.

We shall hereafter return to this work, to consider more at length some of the interesting results of Mr. Darwin's researches.

* "The Formation of Vegetable Mould through the Action of Worms, with Observations on their Habits." By Charles Darwin, LL.D., F.R.S. (London: John Murray, 1881.)

Applications of Electricity.—The public hardly realise, as yet, a tenth part of the uses to which electricity can be readily and conveniently adapted; and exhibitions will furnish the best possible means of rendering them familiar with these uses, which, in many cases, are of an essentially domestic character. Electric bells, for example, although almost universal in large hotels, offices, and public buildings, make way but slowly in private houses, notwithstanding their many advantages. Telephones, in like manner, are far less numerous than they ought to be; and many forms of electrical arrangement quite common in the United States, and found to have great value in saving labour, are scarcely at all in use among ourselves. The electric light is not yet employed in many places for which it is eminently suitable; and its employment is still impeded by difficulties of detail which a larger demand would set aside.—*Times.*

Letters to the Editor.

[The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with their writers. All communications should be as short as possible, consistently with full and clear statements of the writer's meaning.]

All Editorial communications should be addressed to the Editor of KNOWLEDGE; *all Business communications to the Publishers, at the Office, 74, Great Queen-street, W.C.*

All Remittances, Cheques, and Post-Office Orders should be made payable to Messrs. Wyman & Sons.

⁂ All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.

All Letters or Queries to the Editor which require attention in the current issue of KNOWLEDGE, *should reach the Publishing Office not later than the Saturday preceding the day of publication.*

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

Our Correspondence Columns.

I AM very anxious that Correspondence should become a distinguishing feature of this magazine. I wish all readers to feel that in these columns, including the section for Queries and Replies, they have a means of resolving doubts which may occur to them in scientific study or investigation, when reading articles on science in magazines and journals, and in studying the pages of this magazine itself. Our space will indeed be too limited to permit of our dealing with all such questions as occur to students; so that simple and easily-resolved questions cannot occupy space in these pages, which could be better employed. Those who ask such questions must not be angry if they find a reply in our "Letter Box" couched in very brief terms. Still the wish of myself and others, who will join with me in conducting these columns, will be to leave, if possible, no question unanswered. And as we shall in many cases leave reply to readers who may have special means of information on particular subjects, so also shall we often join the ranks of those who ask questions.

A valued correspondent, who will, I hope and believe, help largely in dealing with difficulties which come into these columns for discussion, advises (see letter 1) that paradoxers should be rigidly excluded at the outset. He has had a very wide experience in this matter; but mine has been even wider, and I must confess to still feeling some tenderness for paradoxers. So many of them have originally been victims of ill-written text-books, difficulties left unexplained, and so forth, that hopeless though the attempt may seem of putting them on the right track, I do not yet feel disposed to give it up entirely in every case. In these pages the honest paradoxer, at any rate, may at least state his difficulties; but space will not be given to him to urge theories in defiance of known facts or established doctrines. I shall venture to ask that even those who are surest of their ground in meeting paradoxers will deal tenderly with these weaker brethren. The paradoxer finds it hard enough to give up a theory which he has, perhaps, nursed for years in the belief that it was legitimate, without being badly ridiculed or harshly rebuked. (*Nescit vox missa reverti;* were it otherwise there are few words of my utterance I would more wish to recall than those in which I have exposed, with unnecessary energy, mistakes which might equally well have been corrected in a gentler manner.)

In more equal arguments, where, perhaps, each party to the discussion has some truth on his side, a greater liveliness of tone may, perhaps, be permissible. Yet, after all, it is nearly always [illegible] who is in error.

On [illegible] to exclude from [illegible] whether [illegible].

HINTS TO CORRESPONDENTS.

1.—Believing as I do [illegible] laid down in your prospectus is a sound one, and that, if it be carried out in its integrity, KNOWLEDGE cannot fail [illegible] before it, I would, with your permission, [illegible] put on record certain ideas of mine in connection with one department of your journal. I mean the "Correspondence Column." I am moved to do this from the consideration that the perusal of the similar portion of such of your contemporaries and predecessors as have had anything like a kindred aim with that which you profess, has led me to the conclusion that of all parts of a scientific paper this is the most apt to degenerate into the weakest, unless a tight hand is kept upon those whose chief glory it is to see themselves in print. Amid many earnest students and seekers after knowledge, whose legitimate thirst for information it should be at once the duty and the privilege of the man of science to gratify, are always to be found men who, under pretence of seeking instruction, will obtrude their own "fads" on the public, and unless restrained in the outset, speedily develop into paradoxers of the most aggravated type. Or again, there are the people who put solemnly on records things which are as "familiar in their (and other peoples') mouths as household words," and who will tell you that two and two make four, as though it were a direct and immediate revelation from Heaven. Furthermore, we have the gentlemen who conceive that anything in the shape of the observation of a phenomenon, no matter in how slipshod a style it is described, must necessarily be of the highest scientific interest. These are the people who write to the papers and say that "while crossing Salisbury plain on Friday night between nine and eleven p.m., I noted a sudden illumination of the sky, which, I have no doubt, proceeded from a falling meteorite; inasmuch as, on turning round, I observed a bright object as big as a good-sized stone just disappearing on the horizon." Or "happening to look at the sun on Monday I could detect two spots on it." I would appeal to any who has studied the quasi-scientific correspondence which appears from time to time in different journals, whether I am caricaturing no inconsiderable part of the letters which, in some occult way, their editors suffer to pass? The people, too, who take counsel on matters of almost purely personal interest, who "have built a greenhouse 8 ft. by 4, and will feel obliged if any of your readers will tell me how to keep it stocked with plants throughout the winter." The youths entering into competitive examinations, who wish to be told how to simplify the fraction $a + \frac{a}{+3}$ and so forth. All these occupy space which should be devoted either to those who have something really to learn or something to teach. The person, though, to be rigidly excluded and forcibly ejected from the pages of every scientific periodical whatsoever is the paradoxer; the man who has sparred the circle with a two-foot rule and a bit of string; who has been down into the Essex marshes with a drainage-level and shown, conclusively, that the earth is as flat as a pancake; or he who, by dividing the number of days in the year by the height of the Lord Hill Monument at Shrewsbury, finds the number 3·1416, and forthwith proclaims that Mr. Haycock, the architect must have been "inspired!"* There is a record, more or less authentic, of a man, that his servant ran up to him in great trepidation, exclaiming, "Oh, sir, there is a bailiff down stairs!" "All right," was the response of the master of the house, "ask him to take a chair." "He has taken six already, sir," replied the servant. Upon a cognate principle I would, in *limine*, try to deprecate the offer of a chair in these pages to any paradoxer whatsoever. Depend upon it, should such be made, it will be found that he has taken his "six already."

I am, Sir, yours, &c.,

A FELLOW OF THE ROYAL ASTRONOMICAL SOCIETY.

IS THE SUN HOT?

2.—In your lecture on the Sun we are told that the sun is himself hot, and the source of heat for this earth, just as a fire is the source of heat for a room. Now, I would ask how this can possibly be the case, and I would take the very illustration you employ to show how entirely erroneous is the idea that the sun can be the source of the earth's heat, in the sense, at least, in which you speak. If I

* For an amusing travestie of such reasoning, see "The Tichbone Riddle" further on.—*Ed.* KNOWLEDGE.

draw nearer to a fire in my room, I get more of its heat I find myself warmer. But we know perfectly well that by drawing nearer to the sun we get colder instead of warmer. The tops of lofty mountains are nearer to him than the valleys and the plains which lie around their base. Yet, while it is warm in the valleys and the plains, it is intensely cold at the tops of the mountains so cold that if the mountains are high enough (*and are so much nearer the sun*) the snows never melt there. How can this be if the sun warms the earth as a fire warms a room? Again, if we go up in a balloon, we find that the greater the height to which we ascend the colder is the air.

Indeed, if meteorologists are right in saying, as they do, that the highest clouds, the feathery cirrus clouds, are composed of particles of ice, it becomes simply absurd to maintain that the sun is himself an intensely hot body, for those clouds can be seen on summer days in the full blaze of a solar heat (if that view of his nature were correct). I do not myself understand how meteorologists can have sufficient reason for maintaining that cirrus clouds are composed of ice crystals, unless they believe with me, that the sun is not himself hot (though, of course, he is the source of heat to the earth, acting, I believe, upon the atmosphere in such a way as to generate much heat where the air is dense, and very little where the air is rare); in fact, when I hear the statement made that the cirrus clouds are composed of particles of ice, I recognise another illustration of what I regard as the undue confidence of scientists. They tell us in the same page that cirrus clouds are formed of snow crystals, and (see the accounts of Glaisher's balloon ascents) that even when a balloon is at its highest, the cirrus clouds are still far above, looking no nearer than they appear as seen from the earth's surface. How, then, can the idea that the cirrus clouds are composed of ice crystals be other than a theory, and a very wild theory in my judgment, to be entertained by the very same men of science who believe that the sun is a fire warming the earth?

Although I do not profess to be an astronomer or a meteorologist, I have for many years given great attention to the subject on which I now write; and I have collected together a number of considerations which all tend to show that the solar rays only generate heat when they act in combination with the atmosphere. I would invite readers of KNOWLEDGE to study this question apart from any preconceived ideas they may have, and uninfluenced by the names of so-called authorities in science.

In one respect the theory which I have given above, besides being obviously in better accordance with observed facts than the accepted one (which is, indeed, entirely opposed to them), is much more acceptable to those who recognise the mind of a Supreme Being of infinite wisdom in all the workings of creation. For, observe, if the accepted theory is true, by far the greater part of the sun's rays are wasted. I think Tyndall has stated that more than two thousand millions of rays pass away from the earth for each ray which falls upon it, and that even taking all the planets together, only one ray falls on some planet for two hundred and thirty millions which pass into space and are utterly wasted. Now, what low, and, therefore, what utterly incorrect ideas are given of the great Creator's plans, by a theory which thus tells us that only an exceedingly minute fraction of the work done by the vast orb which He has set to rule and illuminate our solar system is of any use to that system? What should we think of a man who wasted £999 out of an income of £1,000? Yet that would be but a small waste compared with that which scientists assure us is going on in the case of our own sun; and, by parity of reasoning, this waste is repeated millions of times among the millions of solar systems of which astronomers tell us. For my own part, I cannot believe that the picture thus given of the Creator's ways, in the case of these the noblest of the orbs He has made, can be a true one.—I am, sir, yours faithfully, ANTI-GUEBRE.

["Anti-Guebre's" tone is rather dogmatic for one who desires to oppose what he regards as scientific dogmatism. His questions shall not remain unanswered; but we prefer to wait until either he or others who consider the theory of the sun's heat untenable shall have given more of their reasons for want of faith.—ED.]

CAN ICE-YACHTS SAIL FASTER THAN THE WIND?

[3]—I have seen it stated that the American ice-yachts often travel faster than the wind, and I have been told that in the *Newcastle Weekly Chronicle* you have explained that, though before the wind one of these yachts cannot go faster than the wind, which is, indeed, obvious, yet with a beam wind they will go faster than the wind—sometimes twice as fast. Surely there must be some mistake here. Everyone who has considered the usual explanation of the way in which a ship sails with a side wind knows that the driving force exerted by such a wind is but a part—often but a small part—of the driving force which the same wind would exert on a ship sailing directly before it. In determining the effective force of such a wind we first resolve the full force into two—one perpendicular to the sail, the other parallel to the sail; each of these is less than the full force, being represented by the two sides of a right-angled triangle, while the full force is represented by the hypothenuse. The latter of these, the force parallel to the sails, produces no effect. The force perpendicular to the sail is then resolved into two, one perpendicular to the ship's course, which produces only leeway, the other parallel to that course, which is the only part of the wind's force effective in propelling the ship. Each of the two last-named parts is less than the force perpendicular to the sails, which is itself less than the force of the wind. *A fortiori*, the effective propulsive force of a side wind is less than the full force, and must, therefore, produce a smaller velocity; in other words, the ship sails faster before any given wind than on any other course. What is true of a ship sailing at sea must be equally true of a ship sailing on ice. It seems to me, therefore, sheer absurdity to assert that an ice-boat can go faster than the wind, when we know that, sailing before the wind, she can never have a velocity quite equal to that of the wind.

If there is any flaw in this reasoning I should be glad to have it pointed out. UPSILON.

["Upsilon's" difficulty is a very natural one, and his reasoning seems at a first view just. It is, however, incorrect. It is quite true that regarding a ship as at rest, a wind of given velocity cannot exert a more effective influence than that which it produces as a stern wind. But the same is not true when the ship is in motion. If "Upsilon" compares the two cases—an ice-yacht running before the wind at the same rate as the wind, and one running at the same rate with the same wind abeam—he will see that whereas in the former case the wind exerts no driving action at all on the ice-yacht, there still remains in the latter case a driving force; so that unless the frictional resistance balances this force, the velocity of the yacht will increase. I leave this as an exercise for "Upsilon," but if he should find any difficulty with the problem I will give a fuller explanation, with an illustration or two later.—ED.]

THE BIRMINGHAM AND MIDLAND INSTITUTE.

SOME EGOTISTICAL REMINISCENCES, BY W. MATTIEU WILLIAMS.

AN intellectual festival was held in Birmingham on the 20th of last month, when a breakfast party, including the Mayor, his brother, the President of the Board of Trade, Dr. Siemens, and other notables, filled the Town Hall, the guests afterwards making procession to inspect and inaugurate the new extension of the Midland Institute; and in the evening another meeting was held in the Town Hall to distribute prizes to the students, and listen to the thoughtful address of Mr. Siemens on the scientific training of artisans.

This Institute having been established for the purpose of doing for the Midland metropolis nearly the same work as KNOWLEDGE promises to do for all English-speaking peoples, a short sketch of the working progress of the institution by its first teacher cannot be out of place in the first number of this magazine.

In 1854 Birmingham was in a condition of intellectual destitution that to a young native of the present day must be difficult to conceive. A Mechanics' Institution had been tried, and had failed. Various other minor efforts of the same kind had met with the same fate, and the old Philosophical Society that at one time had done honourable work, and was, if I remember rightly, associated in its beginnings with the great Priestley and his friends, was dying of inanition, little remaining but its physical home, at No. 7, Cannon-street. It seemed as though the infamous outrages upon Priestley by the "Church and King" mob had left a blighting curse upon the intellect of the town.

But in spite of this there were a few men of strong faith; there was Arthur Ryland, the now justly recognised father of the Institute; there were Captain Tindal, Sir F. E. Scott, Follet Osler, John Jaffray, William Matthews, and others, who refused to despair, and they united to do something worthy of so important a centre as Birmingham. They held meetings, subscribed money, and induced others to do the like. Charles Dickens gave readings in the Town Hall, which left a clear profit of £339. 16s. A comprehensive scheme was drawn out, including a "General Department," to supply the usual elements of a Literary and Scientific Institution for the well-to-do classes, and a "School of Industrial Science," since named "The Industrial Department," in which should be supplied systematic instruction, including "Chemistry," as applied to the various Manufactures and Agriculture, Mechanics, Metallurgy,

Mineralogy, and Geology, Ventilation of Mines, and Mining Engineering." Besides these, there were to be "Museums common to both Departments," and "arrangements for associating the School of Design with the new Institute."

The first beginning of the practical work of the Institute was the formation of three classes, "Class A, Physics; Class B, Chemistry; and Class C, Physiology." They were conducted by myself in the building of the old Philosophical Society in Cannon-street, where I had already delivered an introductory lecture, and commenced work in October, 1854. Six months later I added to these two junior classes (day and evening) for Elementary Physics, and two "Female Classes" for the same subject applied especially to household matters, and other extensions gradually followed.

The public meetings and general agitation of the subject throughout the town brought many students, who began with the enthusiasm of novelty, and held on for awhile, especially in the Chemistry Class, where the first twenty lectures devoted to the metalloids, and including the brilliant combustion experiments, seldom failed to comply with Brande's formula for a successful chemical lecture, "a flash, a bang, and a stink." When, however, we came to the common metals and their prosaic salts, there was a sad falling-off, in spite of the local importance of the subject.

This alarmed the Council, but being behind the scenes myself, I understood it. Presently, however, some truly alarming symptoms appeared. I found that we were exhausting our material, that the whole population of Birmingham only contained a very limited number of artisans and others who could appreciate the advantages and the pleasure of systematic study of science; that we had already nearly satisfied their limited demand, and that the rate of growth of a new generation of students was ruinously slow. The further I extended my inquiries and the greater the efforts made by the best men in our Council to recruit the classes, the more clearly and seriously was the difficulty presented.

What was to be done? Must we follow the too common example of substituting clap-trap for sound instruction in order to maintain an appearance of success, or should we stand firmly by our original intent and continue to supply solid instruction even to a beggarly number of students? Mr. Ryland, the majority of the Council, and myself agreed in choosing the latter alternative, but in spite of this it was evident that the Institution had arrived at a very critical stage of its existence, and my anxiety was considerable, having been so continually warned by "practical" men that "this sort of thing" had been tried again and again in Birmingham, and had always failed in the long run, however promisingly it may have begun.

The time had now arrived for proposing a scheme that I had been considering for some time past, and, accordingly, on one of my Sunday afternoon visits to Mr. Ryland, I laid before him the project of superadding to our regular courses of solid instruction occasional single lectures of a very popular introductory character, which anybody, however ignorant, might attend, and thus possibly be cured of the common mental epidemic of supposing that science is necessarily dry and repulsive, and ultimately be tempted to become students in the classes.

Mr. Ryland cordially approved, and we anxiously discussed the question of free admission, or a small charge of twopence, or threepence, or one penny at the doors, and finally decided on ONE PENNY. Mr. Ryland suggested the name, PENNY LECTURES, then a new one, as "Penny Readings" were yet unknown. He laid the project before the Council. It was adopted, and on Jan. 22, 1856, I commenced the first course of twelve penny lectures, the printed prospectus of which is now before me.

It was a general outline of natural phenomena, taking in what is now commonly taught as "Physiography." The success of the experiment was complete; the theatre was filled—at some of the lectures not only the seats were all occupied, but the platform was covered with a standing audience. The course was repeated "by special desire," and thenceforth, until I left Birmingham in 1864, I continued them every Tuesday during the session, with an occasional break, when Mr. George Dawson, or others, volunteered to occupy an evening. The subjects were various, in many cases determined by the limited stock of apparatus then within my reach, as, without ample demonstration, success was impossible. My object was to make the lectures as attractive and as *unsatisfactory* as possible, to awaken curiosity, but not to satisfy it there.

They had the desired effect. All classes of people, and of all ages attended them, the little boys in the front row being especially conspicuous. The classes steadily improved, instead of declining, as heretofore, and I am able to prove how they were fed by the statements of some of my best pupils, who told me how they were tempted to have a pen'orth on passing, then another, then to go every Tuesday, and finally to become what I then found them to be. If space permitted I could state some interesting personal details, among others, of the case of two notorious Birmingham burglars who regularly attended the Penny Lectures until the force of professional circumstances terminated their studies; of other Penny Lecture auditors who now stand well as scientific and technological experts and teachers of science; and how, through Isaac Smith, one of my most enthusiastic penny and class pupils, his uncle, Sir Josiah Mason, was induced to become the munificent founder of the "Mason College," which is doing for those who can afford to study during the day, what the Institute does for evening pupils.

Penny Readings followed the Penny Lectures, as the first initiation of the Literary Classes, and the highly successful Penny Arithmetic Lessons, by Mr. Rickard, revived the mathematical department, which, at about the date of the first Penny Lectures, had declined so seriously that an attendance of three, two, and even only one pupil was not uncommon in the Algebra Class. The present prosperity of the Institute is largely due to the remarkable energy and ability of Mr. Rickard, who proves himself a true teacher by throwing as much effort, earnestness, and enthusiasm into the simplification of simple addition as in the exposition of quaternions or the differential calculus.

When I left Birmingham the Council wisely appointed as my successor, both in the classes and the penny lectures, Mr. C. J. Woodward, who had proved the value of the Institute by first hearing a penny lecture, then entering the classes and gaining the Society of Arts gold medal while yet very young. He still works with untiring zeal and ability, and that simple-minded love of his work that is so contagious to pupils. His extended chemical laboratory and new physical laboratory are so commodious that they rival those of our great universities. A staff of able teachers has followed, and are now working admirably. In the general glorification of founders, patrons, &c., which justly occupied the recent meeting, there was a serious omission, viz., to do honour to the labours of such men as W. Matthews, George Dawson, Sam. Timmins, Dr. Badham, and others, who worked as unpaid volunteers in the pioneer labour of teaching classes when the Institute was too poor to pay professional teachers. The Institute now counts 2,688 students in the industrial department, against the 85 with whom I commenced, and 2,564 members in the general department. It has branches and affiliated institutions in every important suburb of Birmingham. Besides these, there is an "Institute Scientific Society," where original papers are read and discussed, and possessing a good lending library of scientific books; also a "Union of Institute Teachers and Students," for promoting the welfare of the Industrial department.

These, with the unrivalled Board schools, the King Edward's school and its branches, the great free library and public reading rooms and the Mason College, are effecting a wonderful transformation of the hardware metropolis, which will probably render it the most intellectual town in England.

I must not touch upon what may be termed the external history of the Institute, the visit of the great and good Prince Consort to lay the foundation stone of the building, and the many other public incidents connected with its growth; these will be duly recorded in "The Institute Book of Glory," promised by the hon. secretary; but I cannot conclude without a word or two respecting the analogy before-named between the work of the Midland Institute and that of KNOWLEDGE.

If Mr. Proctor were just emerging from Cambridge in the full-blown pride of newly-acquired Academical honours, I should despair of the success of this important effort to diffuse scientific knowledge among all classes of men, women, and children; but learning by the prospectus lately placed in my hands that he has been taking lessons from his own pupils, I have no doubt that he will successfully apply them.

Had I commenced as an Academical purist, with a cut-and-dried course of instruction framed on academical lines, the first classes of the Birmingham and Midland Institute would have failed, with serious consequences to the whole project. In like manner any attempt to popularise science by presenting to non-technical readers mere technical abstracts of technical papers or essays, must of necessity fail again, as it has failed before.

As an example, I may mention the fact that in my first Physics class not one of about forty students, mostly adults, knew what was a parallelogram or a diagonal. How, then, could I teach them the composition and resolution of forces? Such questions I had to ask and solve continually, and though I have not seen Mr. Proctor since his return from the Antipodes, I have no doubt that, during his lecture tours, similar questions have been thrust upon him, and that he has found answers for them, and will proceed accordingly.

The translation of the Latin word "Science" into plain Saxon "Knowledge" is a good beginning, and as Science, thus translated, is the natural food of the intellect of all human beings, there can be little doubt of the success of a well-conducted, non-pedantic (or, I would rather say, anti-pedantic) effort to supply the natural demand.

COLOUR OF SUNLIGHT.

PROFESSOR LANGLEY threw out, several years ago, the idea that our sun is in reality not a white sun, but a blue one, his apparent whiteness being due partly to the absorptive action of his own atmosphere, but partly to that of our own air. It should be noticed, in considering this view, that our sun is probably akin to the stars which we regard as yellowish. Capella, for instance, is decidedly yellow by comparison with Vega. The spectrum of Capella has been shown by Dr. Henry Draper to be so similar to that of the sun that the eye can detect no difference. Arcturus, which is somewhat more markedly yellow and, indeed, is regarded by some as orange-yellow in colour has a spectrum very closely resembling that of the sun, but still not exactly the same. The great value of Dr. Draper's evidence on this point, like that of his evidence respecting the presence of oxygen in the sun (as shown by the comparison of the solar spectrum with that of our own air), is that it can be studied by all who choose to examine the photographs which he has obtained of the spectra for comparison. On the same small piece of glass are shown the spectrum of Capella and the spectrum of sunlight reflected from the surface of a planet, and the identity of the lines belonging to the two spectra can be seen at once. Now, Professor Langley, pursuing the idea which he threw out several years since, has been able to show that our sun, though we regard him as a white sun, and though, seen from a greater distance, so that he appeared as a star, he would probably appear yellowish, is yet a blue, or at least a bluish sun. We see that the sun appears red, or orange, or yellow (according to the state of the air) when setting—that is, when seen through a long range of our own atmosphere. Such action as our air exerts on the sun when he is high above the horizon is similar in character, though less in degree; it serves, therefore, *pro tanto*, to render the disc of the sun yellower than it really is. Again, the edge of the sun's disc is markedly less luminous than the middle, though to ordinary eyesight the difference is generally rendered undiscernible by the sun's great lustre. Of course, in reality the same amount of light is emitted from that part of the solar surface which at any moment lies near the edge of his visible disc as from that part which lies near the centre. The apparent difference can only be due to the effect of absorption exerted by the solar atmosphere—the line of sight passing through a greater range of this atmosphere when directed toward the edge than when directed toward the middle of the disc. (In passing, I may note that the difference of absorptive action is proof, not, as Kirchoff erroneously supposed, of the depth, but of the relative shallowness of the solar atmosphere.) Now, anyone who studies a photograph of the solar disc—as, for instance, that excellent photograph by Rutherford, which forms the frontispiece of Schiller's German translation of "Le Soliel"—will have noticed that the darkening toward the edge is much more marked there than it is in the ordinary telescopic disc of the sun. It follows that those rays which produce the photographic image of the sun (chiefly the blue, indigo, and violet rays) are more affected by the absorption of the solar atmosphere than those which form the ordinary visual image of the sun. His atmosphere then acts more strongly to absorb the rays belonging to the blue end of the spectrum than the rays belonging to the red end. The sun's apparent colour, therefore, is less blue than his real colour. If his atmosphere were suddenly removed, he would change from his present white or whitish-yellow colour to bluish, or perhaps greenish, precisely as the setting sun, if the air between the eye and him were suddenly removed, would change from his apparently ruddy hue to the white colour of the overhead sun. In this way it has been, only by detailed experiments instead of by general reasoning, that Professor Langley has established the theory that our sun is really blue, his apparent colour being due to the effect of absorption exerted by his atmosphere of vapours, chiefly metallic.

THE *TRIBUNE* RIDDLE.

THE mystery of the *Tribune* Building* has long baffled the investigations of our most learned archæologists. Putting aside as unworthy of consideration certain wild theories of its purpose, there remain three leading theories, each one of which has its able advocates. By these we are told that the building was designed either for a tomb, or for religious purposes, or for an astronomical observatory.

The first of these rests on a comparatively slight foundation. The most elaborate research has failed to show that there is anything buried beneath the *Tribune* Building, except beer.

* The office of the New York *Tribune*.

The carefully [illegible] written by Prof. Veranda Brown in support of the theory that the *Tribune* Building represents the history of the Jewish and Christian religions, and contains a precious prophecy as to the future religious belief of mankind, has a certain degree of plausibility which will always commend itself to imaginative men. As is well known, the plan of the building is unlike that of any other architectural work, in the curious and arbitrary way in which it is divided into different stories. The lower part consists of four stories and a basement. According to Prof. Brown, this basement represents the patriarchal period before the time of Moses, and the four stories represent the Jewish nation under Moses, under the Prophets, under the Kings, and under the Roman Empire. The last story is smaller than the others, thus typifying the declining condition of the Jewish state, and the marble capitals of its pilasters indicate the luxury and splendour of the Roman Empire. Above the Jewish division of the building we find a second series of three stories, representing the first three centuries of the Christian era. The uppermost one of these is gorgeously ornamented with polished granite columns, with Byzantine capitals—constituting an unmistakable reference to the conversion of CONSTANTINE, the fixing of his capital at Byzantium, and the triumph of the Christian religion. Next we find three more stories, each containing five windows. These typify the fifteen centuries which have elapsed since the conversion of CONSTANTINE, and bring the record of the Jewish and Christian religions down to the present time.

Where the seventh story, or the third century of our era, begins, the façade of the *Tribune* Building is divided into two large masses, one being precisely twice the width of the other. These undoubtedly represent the division of the Christian world into the two great Churches—the Latin and the Greek—the beginning of which division really dates back to the rivalry between the Bishops of Rome and Constantinople. Between these two great divisions rises the tower—a quadrangular structure, with clear-cut angles, and standing boldly out into the light. In this tower we recognise Protestantism. Smaller in size than either of the two great divisions that represent the Latin and Greek Churches, it reaches nearer heaven, and secures a greater share of both sunlight and clouds.

The upper part of the tower is, in Prof. Veranda Brown's opinion, prophetic in its teaching. Both the tendency of Protestantism to bring all things to the bar of reason, and the refusal of any two Protestant bodies to agree in their views of what is true, are prefigured by the clock which occupies the upper part of the tower, with its four faces that so stubbornly refuse to make the same profession of faith as to the time. Still higher than the clock we find the lightning-rod. There the tower has reached its fullest development—ending in a point—signifying "nothing," and a lightning-rod, of all things the most thoroughly scientific and materialistic. Prof. Verandah Brown, who is a pure rationalist, accepts with some scientific enthusiasm this architectural prophecy of the fate of Protestantism.

The astronomical theory of the building is ably advocated by Prof. Pritchard Roctor. He maintains that had the builders intended to erect an astronomical observatory, they would have designed a building which should have been both massive and high. Both of these conditions are found in the *Tribune* building. They would also have constructed a long, narrow tube of masonry through which to observe the stars. We find precisely such a tube in the *Tribune* Building, although the ignorance of the present day has perverted it to the use of an elevator. They would also, says the Professor, have constructed a subterranean chamber as a receptacle for water. The *Tribune* builders builded even better than the Professor knew, for they made a large subterranean chamber for the reception—not of water, but of beer. From these considerations he draws the conclusion that the *Tribune* Building was designed for astronomical purposes, and incidentally remarks that the arguments of Professor Veranda Brown are fallacious, inasmuch as he forgot to include the coal-cellar in his calculations, and the coal-cellar cannot possibly be brought into any intelligible relation with the Jewish commonwealth.—*New York Times*.

ULTIMATE STRUCTURE OF BODIES.—"As matters now stand," says Mr. H. C. Sorby, "we are about as far from a knowledge (by vision) of the ultimate structure of organic bodies as we should be of a newspaper seen with the naked eye at a distance of one-third of a mile."

Our Mathematical Column.

MATHEMATICAL columns in magazines are too apt to degenerate into puzzledom corners, collections of problems of considerable difficulty, but having no real interest, and not valuable as illustrating principles. We wish ours to be of real use to the student of mathematics, but still more to those who, when studying other branches of science, find that rightly to understand their subject they require to be familiar with certain special departments of mathematics. It must be confessed that most of our treatises on mathematical subjects pay little attention to the requirements of students of this kind. They are excellently adapted to prepare students for examination; because at an examination a student of algebra must not be surprised if he be asked questions relating to infinite series, a student of trigonometry if he be asked to establish Dr. Moivre's Theorem, and so forth. But they do not meet the wants of one who requires to know the methods and principles of algebraical, trigonometrical, or other forms of calculation. There is little in such works to show the *use and value* of the processes considered in them. Many a ready student has passed a fair examination in the differential calculus, who had not the remotest idea of the practical value of its methods, or that in hundreds of simple subjects of inquiry the calculus can be employed easily and advantageously. There is no occasion for mystery respecting the use of mathematical methods; but it would really seem, to read some mathematical treatises, as though the last idea in the world the student should associate with any mathematical subject was the idea that it may actually be useful. Even the problems which are given for solution are, for the most part, either useless or absurd.

This is not the way to render mathematical subjects inviting, or to encourage the student to master the difficulties which are inherent in them.

Of course, we cannot here give mathematical essays which can render the student independent of systematic treatises. Such treatises he must have, and must carefully study. But we hope to show that a number of departments of mathematical research, supposed to be either too profound, or of too little practical value to be worth taking up, can be readily and usefully studied. In every case we wish to come as quickly as possible to the practical application of the various methods we shall successively deal with.

We shall probably begin with a simple paper on the use of logarithms, seeking to show that, instead of being regarded as a mere mysterious collection of numerals, a book of logarithms should be considered the great simplifier of all forms of calculation. Scarcely anyone who has much to deal with figures, otherwise than in mere processes of addition or subtraction, should be without such a book; and a very brief study of the subject will suffice to enable anyone to make ready and intelligent use of the tables which a book of logarithms contains.

We may then discuss the Laws of Chance, the simple applications of Trigonometry, and other such matters, avoiding, as far as possible, those parts of a subject which a student can readily study in set treatises.

We shall be glad to receive problems of interest, either for solution or discussion, preferring, however, those which illustrate general principles to those which are merely, as it were, casual.

In many cases where problems are sent to us for solution, we shall only give hints, not complete solutions, believing that, so dealt with, they are likely to be of more use to the student than if a complete solution were at once given.

We need hardly say that this is not a suitable place for the discussion of very difficult mathematical problems, albeit those of our readers who do not take interest in mathematics must not be angry with us if from time to time we devote a column, or even a page or two, to matters of no interest, or even absolutely unintelligible to them. They must remember that each of our lighter essays here may be equally interesting to proficients in the subject dealt with; that, in fact, no one can expect *all* the contents of such a journal as the present to be interesting to himself individually. We shall endeavour, however, to keep within close limits all matter likely to be "caviare to the general."

SUNLIGHT AND HEAT.—The intensity of sunlight at the sun's surface is calculated to be 190,000 times that of a candle-flame; 5,300 times that of the glowing metal in a Bessemer converter; 146 times that of the limelight; 3·4 times that of the electric arc at the brightest yet obtained. The heat emitted by the sun in a single second would suffice to melt a shell of ice covering the entire surface of the earth to a depth of 1 mile 1,457 yards, or to burn a layer of anthracite coal 17·7 yards thick over the earth's entire surface. This would be equivalent to a consumption of about 16,436 millions of millions of tons of such coal per second.

Our Chess Column.

MANY weekly and monthly periodicals in this country and in others now contain a chess column, but it may be a question how far any general knowledge of the game is encouraged in this way. Usually these chess contributions are limited to problems, games between first-class proficients, and replies to queries, with occasional items of chess news. The problems are such as only good players can readily solve, so that the learner, unable to master them without an expenditure of time which he can ill spare, is rather deterred from the study of chess than otherwise by these masterpieces of chess strategy. The games, again, even when they are sufficiently annotated, are generally too profound to have much interest for the learner. He cannot see the purpose of moves whose real significance depends generally on results five or six moves at least in advance, and almost always on moves which are not played on either side. The chess expert recognises the beautiful positions, the brilliant attacks, the complex defences, and so forth, which would result if a particular move were not met in that way in which, in the game itself, it actually is met. A volume would be required to show all such results of each move played on either side by two first-class players; and the beauty of a game between two such players can only be properly appreciated by those who, as each move is played, can follow out the various consequences, according to the way in which the move may be met.

We wish to cater for a wider circle of chess players—for those who enjoy the game and can appreciate its beauties, while they have no ambition to be able to meet a Steinitz, a Blackburne, or a Zukertort without receiving odds.

It must be admitted that nine out of ten who call themselves chess-players know very little about the game. They move their pieces without any adequate idea of the value of position, or of the manner in which the powers of the various pieces can best be brought out. Of the importance of time, again, in chess strategy, they seem to have scarcely any conception. A player of this class will move his Queen out to some square where she can be attacked by a piece which in attacking her will take up a strong position, then to some other square where she can be similarly attacked by another piece, and, perhaps, after four or five such moves, be fortunate in being able to return her to her own square. He will then complacently remark that he must try some other way of opening his attack, utterly overlooking the fact that his opponent has gained four or five moves, and that even a single move early in the game often makes all the difference between a strong attack and an unsatisfactory defence. If such a player moves his Knight from King's Bishop's third to King's Knight's fifth, and on the opponent moving Pawn to King's Rook's third (attacking the Knight), finds no better move than to return him to his former position, he would be incredulous if told that he had thus lost all chance of winning against correct play. Yet there is scarcely an imaginable case, in the early stage of a game, where, if the game had been equal before this had happened, it would not afterwards have been seriously compromised. The opponent, be it observed, has made a move of great utility (though often too slow, except as in this case when a move is given away), while it is now his turn to play instead of the first player's, who stands just where he did before he rashly moved forth his Knight. (The opening, of which the accompanying game is an illustration, shows that, even when by venturing forth the Knight to the square in question a Pawn is gained, the counter attack, after the Knight has been driven back, compensates the second player fully for the loss. It also presents at move 33, a case in which loss of time is equivalent to loss of a game which might probably have been drawn.)

It seems to us that we may do something to encourage sound chess play by giving our readers chess material of a different kind from that which has usually been presented in chess columns. We propose to explain in a series of short papers the principal openings, discussing their advantages for attack and defence, and showing how the opening moves illustrate the general principles on which sound play depends. We shall illustrate these openings by games played by good players, but not played with quite so much study (and, therefore, not needing such skill for their interpretation) as the match games commonly selected for these columns. For this purpose we have made arrangements with the proprietor of the mechanical chess-player, Mephisto, to have games played with Mephisto specially for our chess columns. By an extension of Mr. Gümpel's kindness, the guiding spirit of that mysterious player has been persuaded to make his own comments on the game. It need hardly be said that only such games will be selected as have real chess interest. The multitudinous contests in which tyros have succumbed to Mephisto (and would have succeeded at the odds of a Queen) would be quite as much out of place here as in the

columns of the *Chess Chronicle*, humbler though our aim may seem.

We shall assume on the part of our readers a knowledge of the elements of chess play—the moves, laws, and so forth; and sufficient familiarity with the notation employed by English-speaking players, according to which the King (K.), the Queen (Q.), the Bishops (K.B. and Q.B.), the Knights (K.Kn. and Q.Kn.), and the Rooks (K.R. and Q.R.) are regarded as standing severally on the first square of a file, the other squares of which are numbered in order, the 2nd, 3rd, 4th, 5th, 6th, 7th, and 8th.

For our chess readers' amusement we give an illustration of weak chess play, which occurred within the last few days over our own chess-board. The first player had the idea that, knowing little of the usual openings, he might equalise matters by playing on a line entirely unrecognised by the books:—

CHESSIKIN.

Remove Black's Q.R.

	White. Amateur.	Black. Chess Editor.
1.	P. to K.4.	P. to K.4.
2.	P. to K.R.4 (?)	Kt. to K.B.3.
3.	Kt. to Q.B.3.	B. to Q.B.4.
4.	R. to K.R.3. (?)	P. to Q.3.
5.	R. to Q.3. (?)	B. to K.Kt.5.
6.	P. to K.B.3.	Kt. to K.R.4.
7.	Q.Kt. to K.2. (a)	Q. takes P. (ch.).
8.	P. to K.Kt. 3.	Kt. takes P.
9.	Kt. takes Kt.	Q. takes Kt. (ch.).
10.	K. to K.2.	Q. to K.B.7. mate.

a The game was still defensible, by— 7. $\frac{\text{P. take B.}}{\text{Q. to K.R.5. (ch.).}}$ 8. $\frac{\text{P. to K.Kt.3.}}{\text{Q.Kt. takes P.}}$ 9. $\frac{\text{Q. to K.B.3}}{\text{B. take Kt.}}$ &c.

GAME No. 1.

Played between the Mechanical Chessplayer, "Mephisto," and an Amateur.

Two Knights' Defence.

	White. Amateur.	Black. Mephisto.
1.	P. to K.4.	P. to K.4.
2.	Kt. to K.B.3.	Kt. to Q.B.3.
3.	B. to B.4.	Kt. to B.3 (a).
4.	Kt. to Kt.5.	P. to Q.4.
5.	P. takes P.	Kt. to Q.R.4.
6.	P. to Q.3 (b).	P. to K.R.3.
7.	Kt. to K.B.3.	P. to K.5.
8.	Q. to K.2.	Kt. takes B.
9.	P. takes Kt.	B. to Q.B.4.
10.	P. to B.3 (c).	B. to K.Kt.5 (d).
11.	P. to K.R.3.	B. takes Kt. (e).
12.	P. takes B.	Castles.
13.	P. to Kt.4.	R. to K.sq. (f).
14.	P. to B.4.	B. to Q.3.
15.	B. to K.3. (g).	P. to Q.R.4.
16.	P. takes P. (h).	R. takes P.
17.	Kt. to Q.3.	R. to Q.R.6.
18.	Kt. to Kt.3.	Q. to R.sq. (i).
19.	Castles K.R.	Q. to B.sq. (j).
20.	K. to R.2.	Q. to B.4.
21.	Q. to Q.2.	Kt. to R.4.
22.	Kt. to Q.4.	B. takes P. (ch.)
23.	B. takes B.	Q. takes B. (ch.)
24.	Q. takes Q.	Kt. takes Q.
25.	Kt. to Kt.5.	R. to R.5.
26.	Kt. takes P.	R. to Q.B.sq.
27.	P. to Q.6.	R. takes B.P.
28.	K.R. to K.sq.	Kt. to Q.6.
29.	R. to K.2.	R. to Q.sq.
30.	Kt. to Kt.5.	P. to B.4.
31.	P. to B.3. (k)	P. takes P.
32.	R. to K.7.	Kt. to B.4.
33.	R. to K.Kt. sq. (l)	P. to Kt.4.
34.	R. to K.B. sq.	R. to R.5. (m)
35.	P. to R.3.	P. to Kt.5.
36.	P. takes P.	P. takes P.
37.	R. from B. sq. to K. sq.	P. to B.7.
38.	R. to K.8. (ch.) (n)	R. takes R.
39.	R. takes R. (ch.)	K. to B.2.
40.	R. to K.7. (ch.)	K. to B. sq.
41.	K. to Kt.2.	R. to K.B.5.
42.	K. to B.sq.	P. to Kt.6
	Resigns (o)	

MEPHISTO'S NOTES.

(a) This move constitutes the Two Knights' Defence.

(b) This move gives White if not a bad, at least a difficult, game to play; the continuation 6.B. to Kt.5. (ch.) is to be preferred.

(c) This move is stronger than the usual move P. to K.R.3 White threatens an attack with his Pawns on the hostile Bishop, thereby developing also his strong Queen's wing. P. to B.3. also provides a refuge for White's Knight on Q.4. in case Black should Castle, which would leave the Knight *en prise*.

(d) This certainly seems the most attacking line of play, in addition to which Black could also play P. to Q.R.4. to prevent the advance of the Queen's Pawns or P. to Q.Kt.4., or Castles.

(e) This is better than B. to R.4., which would result to the advantage of White, *e.g.* 11. $\frac{}{\text{B to R.4.}}$ 12. $\frac{\text{P. to K Kt.4.}}{\text{B. to Kt.3.}}$ 13. $\frac{\text{Kt. to K.5.}}{}$ with the better game.

(f) Threatening to win the Queen.

(g) This is the right move to stop any advance of the Black King's Pawn. Thus, for instance, 15. $\frac{\text{P. to Q B.5}}{}$ instead, would not be good; for 15. $\frac{\text{P. to Q.B.5}}{\text{P. to K.6}}$ 16. $\frac{\text{B. takes P.}}{\text{B. takes K.B.P.}}$ with the better game. White could not take the Bishop, for then Black would win his Queen by P. takes P. (ch.).

(h) Black has played P. to Q.R.4, with the intention of breaking up the Pawns on White's Queen's wing. If instead of P. takes P., as actually occurred, White should play P. to Kt.5, then P. to Kt.3 would stop White's Queen's Pawns. Black might, perhaps, also reply with P. to B.3. The variations arising out of this move are very numerous. The idea is the same as in 15. $\frac{}{\text{P. to Q R.4,}}$ namely to separate White's Pawns, and then attack them singly, *e.g.*:—

16. $\frac{\text{P. to Kt. 5}}{\text{P. to B. 3}}$ 17. $\frac{\text{P. to B.5 (or see A)}}{\text{B. to Kt. sq.}}$ 18. $\frac{\text{P. to K.6}}{\text{P. takes P.}}$

19. $\frac{\text{Q. takes P.}}{\text{Q. to Q.2}}$ 20. $\frac{\text{Q. takes Q.}}{\text{Kt. takes Q.}}$ 21. $\frac{\text{Kt. to Q.2}}{\text{B. to R.2}}$ 22. $\frac{\text{Kt. to Kt. 3}}{\text{P. to R.5}}$ with the better game.

A. It would be disadvantageous to take with the Queen's Pawn, thereby opening the file commanded by the Black Queen; therefore, 17.] $\frac{\text{Kt.P. takes P.}}{\text{P. takes P.}}$ If now White should play P. takes P., then Q. to B.2 would give Black a very good game, or if White should play, 18. $\frac{\text{P. to B.5}}{\text{B. to Kt.sq.}}$ 19. $\frac{\text{P. to Q.6}}{\text{B. to Kt.2}}$ 20. $\frac{\text{Kt. to Q 2}}{\text{Q. to B.sq.}}$ with the object of playing Q. to B.4, followed, perhaps, by Kt. to Q.2., Black should again get a good game.

(i) Threatening the capture of the Knight.

(j) By 19 Castles, White thought to evade Black's attack on the Queen's side, but only to exchange it for an attack on the King's side, the chance of which Black at once follows up by Q. to B.sq.

(k) White hopes to be compensated by his attack of R. to K.7 for this move.

(l) This is loss of time, as Black thereby advances his Pawns in support of the Bishop's Pawn. White cannot venture upon anything for fear of P. to B.7; if K. to K.3, then Black wins the White King's Pawn by Kt. to K.5 (ch.)! therefore, 33 R. to K.B.sq. at once was White's best play.

(m) This is hardly necessary; the object was to make the Rook available for support of the other Rook via R. to R. sq.

(n) This is weak again. R. to B.7 would have given White a good game; for, in reply to $\frac{}{\text{R. to B.4,}}$ White would play K. to Kt.3 with a fair chance of drawing.

(o) Black threatens to Queen his Pawn by P. to Kt.7 (ch.), which White cannot prevent.

KNOWLEDGE AND IGNORANCE.

"Ignorance is the curse of God,
Knowledge the wing on which we fly to Heaven."
—*Shakespeare.*

*** WHIST COLUMN.—Our papers on Whist will be commenced in No. 2.

KNOWLEDGE
AN ILLUSTRATED
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THE PHILOSOPHY OF ANIMAL COLOURS.

By Dr. Andrew Wilson, F.R.S.E.

THERE is a suggestive passage in Butler's "Hudibras," which maintains that—

> "Fools are known by looking wise,
> As men find woodcocks by their eyes."

And if the axiom be correct, that a poet is only great when he is true to nature, it must be admitted that Butler has been singularly felicitous in this metaphor. Whoever has seen a woodcock in its ordinary summer plumage may form a good idea of the truth of the poetic remark. As that bird moves about amongst the fallen leaves of autumn, the greys, and browns, and yellows of its feathers mingle so beautifully with the like tints of its surroundings, that the animal is absolutely concealed from any view but the practised eye of the sportsman. As has been remarked of the bird in question, even the very conspicuous and ornamental tail becomes hidden from view in a most singular fashion. Below, these tail-feathers exhibit a white colour tinted with a silver sheen and marked with a deep black. Nothing more conspicuous than such an ornament can well be imagined; yet the tail and its belongings are, nevertheless, wonderfully concealed. For, as the bird reposes, these under lines and tints are placed downwards; and above, the ashen-grey tints mingle perfectly with the bird's surroundings. As the woodcock, therefore, rests amid its background of wood and its foreground of fallen leaves, every line of its plumage is made to assimilate so closely with the objects around, that the bird's presence, even a short distance off, is not suspected.

The woodcock is by no means alone in this harmony betwixt its plumage and its surroundings. The sand-grouse of the deserts, for instance, exhibit a like harmony. These birds cannot be detected, even as they run, amidst the sand of their haunts—so closely imitated in the dull tints of their plumage is the tone of the desert wild. The well-known case of the ptarmigan is even more extraordinary still. In summer the bird shows a plumage of pearly grey, which conceals it perfectly as it lies on its bed of Scottish heather, mingled with the lichen and its kith and kin. But when the winter snows descend and coat the hillsides with a mantle of white, then a kindly nature still contrives concealment for the ptarmigan in a fresh suit of colour. The pearly greys of the summer are replaced by a plumage of snowy whiteness, and, save for its dark eye, there is little risk of the discovery of the bird by the unwary or unpractised sportsman. The grouse and common partridge are not less perfectly protected. The hues of the grouse match the tints of the heather, and the partridge is almost as difficult to discover—say, in a ploughed field—as the ptarmigan on the hill-side. The birds just mentioned are all *rasorial* birds; that is, they are allied to the type of the common fowl, and are typically ground-livers. Their tints, therefore, assimilate with those of the ground and with ground vegetation; and whatever may be the ultimate philosophy which shows the origin of such harmonies, it is very plain that the utilitarian is bound to read "protection" in every line of the story. Escape from their enemies must be favoured by the correspondence in colour to which we allude. The harmonies of colour present the safest, and therefore the best foil, to the keenness of sight of the eagle, and to the agility of the falcon and its kind. It is different, indeed, with the songsters of the wood and grove. With well-developed powers of flight, and with a close refuge amid the foliage of the wood, the appearance of bright hues and tints in these birds is by no means disadvantageous. Another law—that of the development of colour in relation to sex—has taken precedence of the regulation of colour as a means of protection. If concealment be necessary, nature will teach the art of hiding in other ways than that whereby she contrives to make the partridge face danger with a stillness that almost rivals that of the stones, trustful in the harmony of her plumage that so closely matches her heather bed.

But there are wider fields open to the naturalist's survey of colour and its meanings. Suppose that we peer for a moment into the class of fishes, we shall find the adaptation of colour to surroundings illustrated in a very apt degree. Whoever has tried to spear a sole or flounder, for example, well knows that the excitement of the sport consists in the endeavour to follow out the axiom of Mrs. Glasse, and on the principle which that worthy lady laid down about "first catching your hare," to first catch your flounder. You cautiously and softly paddle out to shallow water in your punt, and you drift over the flat, sandy beach at a depth of from two to three feet. Below, the water is as clear as crystal. Here and there you see a lazy starfish on the march, exerting himself to the utmost, as he slowly extends ray after ray, and crawls at the rate of about a mile a month or so, by aid of his hundreds of sucker-feet. The sand-eels annoy you as they burrow downwards and send up little clouds of dust on your approach; but the flounders you came to spear—where are they? and echo seems but to answer "Where?" But the practised sportsman bids you learn (as in all other sciences and arts) the first lesson—namely, how to see and observe. As your boat creeps along, he points to what seems a mere sandy lump, but in which his keener eye has detected the merest wriggle of a fin. Dash! goes the spear, and up comes a flounder, and as you watch the ground, you see dozens, it may be, of similar sandy patches swimming off in rapid alarm. The flounder's "back"—it is really the *side* of the fish—on which it lies, is white enough, as we know; but the "other side" is as close a representation of a sandy patch as you can see or as you can imagine. Small wonder, then, that in flounder-spearing you experience the difficulties which nature throws in the way of capture through likeness in colour to the animal's surroundings. It is the same with sole, turbot, and with the skates and angel-fishes. Watch the

first flounder you see resting on the sandy bed of the Aquarium tank, and you will receive ample proof of the truth of the foregoing remarks. And should you chance to see the lazy "monk," or angel-fish, as it lies prone, heavy, and indolent in the highest degree in the flow of its tank, you may again understand something of the value of colour as a means of protection to animal life.

In the case of those "queer fishes," the little sea-horses, or hippocampi, with heads like horses, and with a body which, at large, reminds one most forcibly of some figure from the Heralds' College on a crest, concealment is effected in a slightly different fashion from that prevalent among the soles. Here the body, as a rule, possesses long streamers or fringes that mimic the seaweeds; so that, as the animal reposes, its body may well enough represent a stone, to which are attached fragments of marine vegetation. The Australian sea-horses, which live among red seaweeds, have streamers of that hue attached to their bodies, and the mimicry and imitation of their surroundings are thus very complete. Even their near neighbours, the pipe-fishes, with green bodies, when they fasten themselves to some fixed object, and "loll" in the water, may closely resemble an inert piece of green weed.

Amongst even the highest animals, protective colouring is common. A lion's hue matches the sand, as a tiger's stripes, according to Mr. Wallace, imitate very closely the foliage and trees amidst which it crouches. The camel's coat is sandy like its desert; and the rabbits offer as plain examples as any of the colour harmony in question. The polar bear is white, like the arctic fox in winter dress; and the nocturnal rats and moles are dressed in shades the opposite of the ghost-like hues that become so conspicuous at night.

THE SOUTH EUROPEAN VOLCANIC SYSTEM.

THERE are reasons for believing that all the volcanoes—and, indeed, all the regions of subterranean disturbance in Southern Europe—belong to a single volcanic system.

It is, indeed, rather difficult to define the exact relation between the different parts of a widely-extended volcanic region. At a first view it seems unreasonable to assert that if eruptions or other forms of disturbance in different places are simultaneous, this must be regarded as evidence that the two places belong to the same volcanian region, while a similar conclusion should be deduced from the fact that quiescence in one spot synchronises with disturbance in another, and *vice versâ*. For instance, it was noticed that during the often interrupted eruption of Vesuvius in 1868, Etna was more or less disturbed, until at length, as if in sympathy with the Neapolitan volcano, the Sicilian mountain gave vent to enormous streams of molten matter; and this of itself might be considered to afford satisfactory evidence of the existence of some sort of connection between Etna and Vesuvius. But on the other hand, we are told that when the great earthquake of Calabria took place early in 1783, the inhabitants of Pizzo remarked that the volcano of Stromboli, which is in full view of Pizzo at a distance of about fifty miles, smoked less and threw up a less quantity of heated matter than it had done for several preceding years. Then, again, on the same occasion the great crater of Etna gave out enormous quantities of vapour as the Calabrian earthquake began, while Stromboli seemed exceptionally active as the commotion of the earth in Calabria began to diminish; yet no eruption occurred from either of those great vents during the whole progress of the Calabrian earthquakes. Are we to infer from this, as in one place Sir C. Lyell seems to do, that the volcanic fires of Etna and Stromboli are "very independent of each other," or that the same mutual relation exists between them as between Vesuvius and the volcanoes of the Phlegræan Fields and Ischia, "a violent disturbance in one district serving as a safety-valve to the other?" Lyell completes the latter sentence, by the way, by adding, "both never being in full activity at once." But here it seems to us he loses the full value of the evidence which the various disturbances of the South European volcanic regions have afforded. Rightly viewed, both forms of evidence equally tend to show the inter-dependence, neither showing the independence, of different parts of this great region. If an eruption of Vesuvius begins precisely when an eruption of Etna seems for some cause or other to be checked, or diminished in activity, the circumstance seems in itself to suggest, if it does not prove, that the two vents serve to relieve one and the same volcanic region. If, again, Vesuvius and Etna both burst suddenly into violent eruption at one and the same time, surely that also is evidence that they belong to the same volcanian system.

There is no reason whatever for assuming that because a disturbance in one region may serve as a safety-valve for another, therefore both can never be in full activity at once. Experience has shown repeatedly that two regions of volcanic disturbance, the movements of which sometimes alternate, may at other times be simultaneously active.

But it may perhaps be asked, if this really is so, how can we possibly decide whether any two volcanian regions whatever are connected or distinct? For instance, when Chimborazo is active, Vesuvius is either active or at rest. If Vesuvius is active, the two vents may be said to be working simultaneously; and so the preposterous idea may be adopted that these two volcanoes, though separated by so many thousands of miles, relieve the same region of subterranean disturbance. If, on the other hand, when Chimborazo is active, Vesuvius is at rest, then the same absurd conclusion is derived from the consideration that the action of Chimborazo alternates with that of Vesuvius. In reality, however, plausible though this objection seems, it has no weight. The distinction between the simultaneity or the reverse of the action of Vesuvius and that of Chimborazo, and the associated or alternated efforts by which Vesuvius and Etna, or Etna and Stromboli, or Vesuvius and the Phlegræan Fields, relieve one and the same region of subterranean disturbance, is sufficiently well marked. Compare the dates of the eruptions of Chimborazo and Vesuvius with the actual range of time over which the observations extend, and no connection whatever can be observed between them. Vesuvius must, indeed, be either quiescent or at rest when Chimborazo is in eruption, but Vesuvius never (or only by the merest accident) begins to be disturbed when an eruption of Chimborazo commences; nor has an eruption of Chimborazo ever synchronised, except by the merest accident, with the cessation of an eruption of Vesuvius, or *vice versâ*. But whether we find that an eruption of Vesuvius ceases very soon after an eruption of Etna has begun, or that both volcanoes burst at once, or nearly at once, into eruption, the coincidence of contrasted conditions implies equally with the coincidence of similar conditions, that the two volcanoes are outlets of the same volcanic region.

In other large regions of subterranean activity we notice similar relations—namely, at times periods of well-marked oscillatory disturbance, and at other times periods of simultaneous action. We also find volcanic regions where the outlets are nearly always in action together at

times of disturbance, and other regions where alternations of activity are the only evidence of connection. Thus the Peruvian and Chilian volcanic regions have seldom, if ever, been simultaneously disturbed. A long period of disturbance in the Peruvian region, culminating in the terrible earthquake of Riobamba, was followed by a series of disturbances in the Chilian region, resulting in a permanent elevation of the whole line of coast. Then came disturbances in the Peruvian Andes, including the tremendous earthquake of the year 1868, which so shook the shores of Peru, that the sea wave then generated swept right athwart the Pacific to the shores of New Zealand, Australia, and the Asiatic continent. It is noteworthy that between the Chilian and Peruvian regions of disturbance there is a space in which no volcanic action has ever been observed; precisely as between the oscillating ends of a balance there is a region of comparative quiescence.

It certainly does not seem probable that these signs of disturbance in different parts of the South European volcanic region which have followed or accompanied the eruption of Vesuvius have been free from all connection with the activity of the great Sicilian vent. Certainly earthquakes in Sicily have been associated with the cessation of the outflow of lava from Etna. Nor can there be much doubt that signs of activity shown by Vesuvius after Etna has been in eruption, and *vice versâ*, indicate sympathy between the Neapolitan and Sicilian volcanic regions. It may seem extravagant to associate earthquakes in France or in the British Isles with the activity of the Italian volcanoes; but there are many reasons for believing that such slight earthquakes as do from time to time occur in these more northerly regions depend indirectly on the condition of the South European region of subterranean activity. It is certain that shortly before the great outburst of Vesuvius in 1868, an earthquake more marked in character than usual in this country shook the western parts of England. It is well known, too, that when another part of the great southern region of disturbance was affected, and Lisbon was laid in ruins, the lakes, rivers, and springs of England were disturbed in a remarkable manner; the water of Loch Lomond, for instance, suddenly rose, without apparent cause, against its banks, and then quickly subsided to its usual level.

THE RELATION OF FOOD TO MUSCULAR WORK.

By Dr. W. B. Carpenter, F.R.S.

PART II.

It has been shown that whilst Liebig regarded the production of Muscular Energy as an expenditure of the "vital force" of the muscle-substance itself—involving its death and chemical transformation, and requiring *nitrogenous* or tissue-food for its regeneration—Mayer attributed the production of that energy to the oxidation of a portion of the *non-nitrogenous* or respiratory food, regarding the muscular apparatus as the mere instrument by which that oxidation is made to produce Motion in place of Heat.

I shall now give a general account of the experimental inquiries by which Liebig's doctrine has been disproved and that of Mayer established. These have reference to the relation of the *amount of work* done by the body as a whole,—(1) to the relative consumption of the nitrogenous and the non-nitrogenous components of the food; (2) to the amount of Carbonic acid exhaled; and (3) to the amount of Urea passed off:—while (4) another set of experiments upon isolated muscles has demonstrated the dependence of the production of energy upon a change in the substance of the muscle itself, rather than (as Mayer supposed) in the blood passing through it.

1. If a man be carefully "dieted" for a time long enough to determine the amount of *nitrogenous* aliment adequate to repair the ordinary daily "waste" of his tissues, and the amount of *non-nitrogenous* aliment needed to maintain the heat of his body at its normal standard and keep him in healthful exercise, so that his weight remains the same at the end of the experiment as at the beginning—and he is then set to make a great addition to his daily exercise in the shape of walking, pumping, turning a machine, or the like—it is found that his body can be kept up to this extra work by an increase in the supply of *non-nitrogenous* aliment, with such a small addition to the nitrogenous as may suffice to make good the loss produced by the increased "wear and tear" of the machine itself.

2. All observations concur in showing an *immediate* increase in the exhalation of Carbonic acid, capable of being determined with rigorous exactness, when the body, previously at rest, is put into motion. The late Dr. Edward Smith made himself the subject of a long-continued and diversified series of researches upon this point, by the use of a portable gas-meter, through which he could breathe when walking or working at a tread-wheel, as well as when standing still, sitting, or lying: and he found not only that when continually "getting up stairs" on the tread-wheel he exhaled more carbonic acid than when walking, that when walking fast up-hill he exhaled more than when walking slowly on level ground, and that in the latter case he exhaled more than when standing still; but that he exhaled more when standing than when sitting, and more when sitting upright (without support to the back) than when lying so fully supported as not to put forth any muscular effort. Further, he found that when, in walking, he carried a weight even of a few pounds, the exhalation of carbonic acid was sensibly augmented; the increase being still greater when the weight had to be *raised* (as in walking up hill), as well as transported. These results have been confirmed by numerous other experimenters. They are in remarkable harmony with those long previously obtained by Mr. Newport, upon the relative amounts of carbonic acid exhaled by a bee at rest, and a bee "buzzing" under a glass.

3. The employment of more exact methods for the quantitative determination of Urea than that used by Liebig, has shown that he was altogether wrong in asserting that a corresponding increase is produced by muscular exertion in the quantity of that substance eliminated by the kidneys. An experiment which has now become "classical" was performed upon themselves by Professors Fick and Weslicenus in 1866; namely the determination of the respective quantities of urea eliminated by each of them for twelve hours *before*, for eight hours *during*, and for six hours *after* the ascent of the Faulhorn, whose height is about 6,500 feet. They took no nitrogenous food either for seventeen hours before the ascent, during the eight hours of the ascent, or for six hours after the ascent; but then took a good ordinary meal. The mean of the two determinations (between which there was a very close correspondence) gave for the twelve hours before the ascent, 0·62 gramme, being at the rate of 0·052 gramme per hour; for the eight hours of the ascent 0·40 gramme, or at the rate of 0·05 gramme per hour; and the same amount for the six hours following the ascent, being at the rate of 0·066 gramme per hour; while for the twelve hours after the subsequent meal, the mean amount was

0·48 gramme, or at the rate of 0·01 gramme per hour. There was thus a positive reduction in the amount of urea eliminated, which was probably attributable to the temporary abstinence from nitrogenous aliment; since the results of subsequent observations carried on for a much longer period upon men going through severe exertion upon an ordinary diet (as those made by Dr. Austin Flint, of New York, upon Weston, the pedestrian, during a five days' walk of 310 miles), show a slight total increase in the elimination, which is fairly attributable to the general "wear and tear" produced by the excessive strain put upon the machine. There is, then, no foundation whatever for the assumption of Liebig, that every exertion of muscular energy involves the death and disintegration of an equivalent amount of muscle-substance.

4. It now appears certain that the chemical change which is the source of Muscular energy occurs in the Muscle itself, not in the stream of Blood that courses through it. For the muscular tissue nourishes itself at the expense, not merely of the *protoplasmic* constituents of the food brought to it by the blood-current, but also of the *saccharine*; and, in addition, takes in oxygen which the red corpuscles of arterial blood bring to it from the lungs. A sort of explosive mixture is thus formed, which is "fired" (so to speak) by the nerve-discharge; a certain quantity of the saccharoid being thus caused to unite with the oxygen contained in the tissue, producing at the same time the *heat* which raises the temperature of the muscle, and the *motor force* exerted in its contraction; while the carbonic acid, which is the product of this oxidation, together with the residual water,* is conveyed away in the return-current of venous blood. But that this "explosion" (the term is only used figuratively, to indicate the *suddenness* of the chemical change, and its excitement by nerve-discharge) does not involve the destruction of the tissues, is evident from the consideration stated by Mayer, that the amount of work done by the heart (which is capable of very exact measurement) would require, if it involved the disintegration of the muscular tissue, a renewal of it at the rate of *a heart per week*. But muscle-substance constitutes no exception to the general rule that every tissue in the body has a term of life of its own: as we see in the rapid waste it undergoes when entirely thrown out of use. And it is the need of renovation thence arising, that gives rise to a demand for *nitrogenous* aliment; this being used to keep the machine, so to speak, in working order, not to serve as its fuel.

Again, it was formerly supposed by physiologists that the conversion of arterial into venous blood (which chiefly consists in the replacement of a portion of its oxygen by carbonic acid) takes place in the capillary network of the system generally; but we now know that it goes on at very diverse rates in different parts, and varies in the same part according to its functional activity. Now, this variation especially shows itself in the blood that passes through the Muscular substance; for when a muscle is at rest, the blood returned by its veins retains almost completely the character of that brought by its arteries; whilst, if the muscle be thrown into contraction by nervous or electric stimulation, the returning blood at once assumes the ordinary venous character—thus showing the dependence of its conversion upon the action of the muscle. We get a corresponding result by experimenting on a separated muscle; that of a cold-blooded animal being most suitable, as longest retaining its vitality when removed from the body. If the muscle of a Frog, placed in a closed chamber exhausted of all save watery vapour, be repeatedly called into contraction by electric stimulation, the chamber is found, after a time, to contain carbonic acid in a quantity proportionate to the number of such contractions, showing that some component of the muscle-substance has undergone oxidation; and since no other product of chemical action is discoverable, it may fairly be concluded that what has been given off from the muscle is part of the saccharoid matter which chemical analysis proves to have been previously stored up in its substance. The union of the saccharoid with the oxygen also stored up in the muscle, produces an amount of energy that can be determined (in the form of units of heat) by the quantity of carbonic acid found in the vessel. This energy, however, may express itself in motion as well as in heat; and the amounts of both may be determined with considerable precision—the former as units of *work* done, the latter as units of *heat* by which the temperature of the muscle is raised. Now, if the units of work be turned into their equivalent units of heat, and the two amounts be added together, they give a total so closely accordant with that deduced from the quantity of carbonic acid produced, as to afford the most striking confirmation of Mayer's prediction:—"Convert into heat," he said, "the mechanical product yielded by an animal in a given time, add thereto the heat directly produced in the body during the same period, and you will have the total quantity of heat* which corresponds to the chemical processes."

To sum up:—The *mechanical working* of the body of a living animal is as directly dependent as its *heating* upon the oxidation of the hydro-carbons of its food; and these may be most economically supplied by non-nitrogenous substances. On the other hand, the mechanism can only be kept in working order by the continual renovation of its substance (its very existence as a living whole involving the continual death and decay of its component parts); and for this renovation a supply of *proteids* is essential, with a certain admixture of *fat* to serve as material for protoplasm.

I have thought it worth while to enter somewhat fully into the particulars of this inquiry, since it affords an excellent example of the truly scientific methods on which Physiology is now being studied, and of the value of the results that are being obtained by their use. It is by such methods alone that the Physical and Chemical actions taking place within the living body can be so determined as to give to Physiology that place among the exact sciences which its earlier cultivators could scarcely venture to hope it would ever attain.

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* The *saccharoids* may be regarded as consisting of carbon *plus* the components of water, so that the amount of energy (whether manifested in Heat or in Work) produced by their oxidation, is proportional simply to the quantity of carbonic acid generated. In *oleaginous* compounds, on the other hand, the number of oxygen atoms is not nearly equal to that of the hydrogen atoms; so that their oxidation generates not only carbonic acid, but also water. It is pretty certain that this metamorphosis, like that of saccharoid, takes place in the substance of the muscle, since it must be the chief source of energy in carnivorous animals, whose food contains no saccharoid. But we have no means of distinguishing the water thus generated from that which is otherwise present.

* It would appear from the experiments of Fick upon frog's muscles, that of the whole energy generated by the oxidation process, about five-sevenths show themselves as heat, whilst the other two-sevenths do the mechanical work. In the bodies of Man and other warm-blooded animals, however, the proportion of heat to work is ordinarily much larger; the former constituting about five-sixths of the total energy generated by the oxidation of the food, while the latter is only about one-sixth. Still, considered simply as a contrivance for doing mechanical work, the human body compares favourably with a steam-engine; the very best form of that machine only exerting about one-eighth of the power which is generated by the combustion of the coal it consumes, the remaining seven-eighths being wasted.

BRAIN TROUBLES.

IN these days, when the energies of the mind have become more important than those of the body, and when even the health of the body is chiefly of value because of its direct association with the health of the mind, it is well that all who have much brain-work to do should know and understand the symptoms indicating derangement or overwork of mental powers. Of course, in all cases where, through whatever cause, any specific mental malady is in question, the assistance of physicians who have given special attention to cerebral diseases must be obtained. But fortunately with most, even of those who work the brain hardest, no more real occasion arises (whatever some doctors would assure us) for medical advice respecting mind troubles, than commonly arises in the case of corporal troubles among men who pass their days in hard but healthy bodily toils. The saying that every man is either a fool or a physician at forty (thirty would, perhaps, be nearer the mark) may be applied at least as well to the case of the mind as to that of the body. It is as easy for one who is not the fool of the proverb to understand the signs which indicate mind-mischief, and to minister to the mind when out of sorts (not actually diseased), as it is for him to note the signs of bodily ill-health, and apply the remedies which experience has shown him to be appropriate. And here we would note generally, what it is one object of this article to indicate specifically, that the analogy may be carried somewhat further. There are few greater mistakes, so far as the body is concerned, than to imagine every little ailment to be a sign of actual disease, and to have recourse for such slight troubles either to medical advice, or (which may prove more mischievous still) to active medicines or other strong remedies. The physician of the proverb, that is, the man who, not being a fool, has learned to understand his own constitution under ordinary conditions, may be watchful, if he so pleases, of even the slightest indications of ill-health, general or local, so long as such watchfulness does not degenerate into hypochondria. But most of these indications should suggest to him only such changes of diet, exercise, hours of resting, and so forth, as his experience has found to be suitable, and should in the greater number of cases suggest negative rather than positive remedies even of this kind. Many signs of illness, indeed, which obtrude themselves on the attention even of those who watch themselves least in such matters, may far better be dealt with by the patient himself than by the physician. For instance, the present writer has learned to regard severe headaches of a certain type simply as affording evidence that certain articles of food (milk, butter, cheese, and the like) must either be given up altogether for several days, or taken in much-reduced quantity. When this course is followed, he is freed from all such attacks, until after the lapse, perhaps, of two or three months, a headache of this particular kind shows him that he has taken such articles of food in greater quantity than is desirable for one of his constitution. A doctor might prescribe with advantage for the cure of the attack itself, and there can be no reason why a person troubled by some severe attack of headache, muscular rheumatism, or the like, should not obtain from a doctor some active medicine by which to diminish the pain from which he suffers; but it is a far more important matter to ascertain the regimen by which such attacks may be prevented from occurring, and this is a matter which a man (not being the "fool" of the proverb) should manage for himself. Now what is true of bodily troubles is true of mental mischief, short of actual disease, though doctors who have learned, rather late, to leave men a good deal to themselves, so far as the former are concerned, are by no means ready to admit that mental troubles can also for the most part be remedied without calling in the physician. Writers like Forbes Winslow, and others who have dealt with obscure diseases of the mind, have done service in calling attention to certain signs of cerebral mischief which laymen might be apt to overlook; but they insist rather too strongly on these as indicative of actual disease, whereas it is within the experience of thousands that such signs, in the majority of cases, are no more to be regarded as necessarily indicating disease, than a passing feeling of nausea necessarily indicates an approaching fever, or than a pain in the bowels necessarily indicates an approaching attack of Asiatic cholera.

It should also be noted, that much mischief may be caused by suggesting that tricks and failings of the mind, which are quite common, are signs of serious cerebral mischief. Not long after the first edition of Forbes Winslow's treatise on "Obscure Diseases of the Mind" appeared, a friend of the writer's, who had begun to read the book only because of his interest in matters scientific, found that it possessed for him a strange fascination, because nearly all the phenomena mentioned by Winslow as indicative of approaching insanity were such as he had frequently noticed in his own case. Thereafter regarding these symptoms in the light in which they were thus presented, this unfortunate student of cerebral science found himself presently possessed by a strange terror lest the state which Winslow seemed to indicate as a necessary sequel of these familiar signs should be close at hand in his own case. The evil progressed until his mind was really endangered by these mistaken fears; but, fortunately for him (if madness is rightly regarded as the greatest of all evils), a series of misfortunes befell him which for a time altogether withdrew his attention from the mental phenomena which had so excited his fears. For two or three years he had to contend against great pecuniary difficulties, and to endure a series of domestic calamities of no ordinary order. Compelled to withdraw his attention from his own mind, he forgot that, according to the teachings of mental physiologists, he had been fairly on the way towards either mania or idiocy. Four or five years later, chancing to take down Forbes Winslow's book from his library shelves, he read with amusement the passages which had formerly excited his fears. He knew that the mental symptoms graphically described by Winslow still presented themselves from time to time—when, for instance, he was tired or unwell bodily—but he had learned in a very practical way that they are not quite so ominous as the mind-doctors assert. It is indeed possible (perhaps probable, or even certain) that no cases of acute mania may be noticed which have not been preceded by such symptoms; but assuredly these symptoms are not in every case—probably not in one case out of hundreds of thousands—the signs of actual mental disease, nor in one case out of millions followed either by acute mania or by apoplectic seizure, as in the exceptional cases dealt with by Dr. Forbes Winslow.

We propose hereafter briefly to consider some of the signs which show that the mind is temporarily out of order, requiring rest, relaxation, or change of employment. We may in some cases have to enforce the lesson we wish to inculcate by citing cases in which such symptoms have been followed by serious mental disturbance; but we wish at the outset to persuade our readers that, in far the greater number of cases, these signs suggest only the necessity for ordinary precautions, not for medical advice or active remedial measures.

(To be continued.)

COMETS.

Part II.

Most persons know that the name "comet" is derived from the word *coma*, or *hair*, and is applied to celestial objects which appear to have a hairy appendage. Modern astronomers do not, indeed, use the word *coma* in this sense, but draw a distinction between the *coma* and the tail. There can be no doubt, however, that the part now called the comet's tail was that from which these objects derived their name. The word *cometa* or *cometes* is not a lately-formed one; but was used by Cicero, Tibullus, and other ancient writers, and it is worthy of notice that all the names applied to comets by the Romans had a reference to *hairiness*—*stellæ comantes, crinitæ, concinnatæ*, they are called by Ovid, Pliny, and Cicero. The last term —signifying stars which show a *curled* or *crisped* hairiness —would not be very applicable to the great comets that have appeared in modern times. The Chinese applied to comets the name *sui*, or "broom."

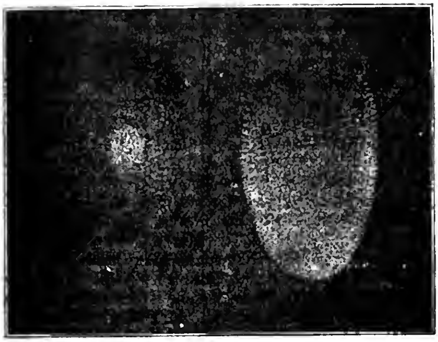


Fig. 1.—Changes of a Comet when first seen.

It might be supposed that the hairy, broom-like, or tail-like appendage, so commonly seen in comets, is really a distinctive feature of these comets. This, however, is far from being the case. A very large number of comets have no visible tails. We refer, of course, principally to telescopic comets; for very few comets which have been conspicuous to the naked eye have wanted this appendage.

The *coma* in the modern astronomical sense is never wanting. This term is applied to a misty, hazy light, surrounding on every side a small bright spot, which is termed the *nucleus* of the comet.

When first seen in the telescope, a comet usually presents a small round disc of hazy light, somewhat brighter near the centre. As the comet approaches the sun, the disc lengthens, and, if the comet is to be a tailed one, traces begin to be seen of a streakiness in the comet's light. Gradually a tail is formed, which is turned always *from* the sun (Fig. 1). The tail grows brighter and longer, and the head becomes developed into a coma surrounding a distinctly marked nucleus. Presently the comet is lost to view through its near approach to the sun. But after awhile it is again seen, sometimes wonderfully changed in aspect through the effects of solar heat. Some comets are brighter and more striking after passing their point of nearest approach to the sun (or *perihelion*) than before; others are quite shorn of their splendour when they re-

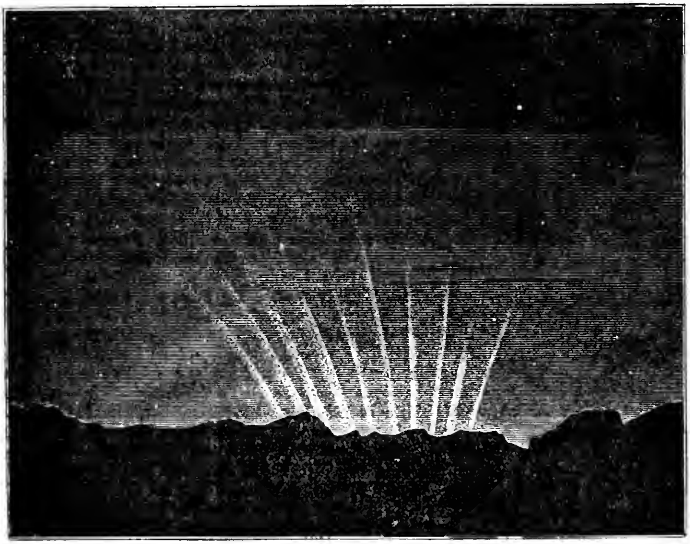


Fig. 2.—The Comet of 1744 (*Cheseaux*).

Fig. 3.—Comet of 1861 (*[illegible]*).

appear. The latter was the case with the comet of 1835-36, as we have already seen. On the other hand, the comet of 1861 burst upon us in its full splendour *after perihelion*-passage.

Some comets have more than one tail. One appeared in 1744 which had no less than six tails, symmetrically disposed (if one can trust the pictures handed down to us) in the figure of a half-opened fan (Fig. 2). Others have presented a yet more peculiar appearance, having, besides a tail in the usual position, a second "unconformable" tail, at right-angles to the first, or inclined to it at some incongruous, out-of-the-way angle—for instance, in one case, one hundred and sixty degrees. Sometimes the peculiarity is presented of a perfectly dark gap separating the tail from the head. More commonly a dark space is seen behind the head, but on each side of this space the light from the head is continued so as to form a bright border on each side of the tail.

away *from* the sun. The same sun which attracts the nucleus seems to repulse the emitted matter with inconceivable energy. Consider for a moment what took place with Newton's comet in 1680-81 (Fig. 3). When this comet was about as far off from the sun as our earth (ninety million miles) it began to throw out a tail. But the comet was going far nearer to the sun than this. Onwards it rushed under the powerful influence of the sun's attraction, until it had crossed the whole space of ninety million miles, making—almost in a straight line—for a point only one hundred and thirty thousand miles from the sun's surface. In four weeks it traversed that vast distance, and then, suddenly (in a few hours) sweeping half round the sun, started on its return journey. But note this: as it approached the sun, the comet had thrown out a tail continually increasing in length, and pointing *back* almost along the orbit; then the comet is lost to sight for a few days, and when it is next seen returning

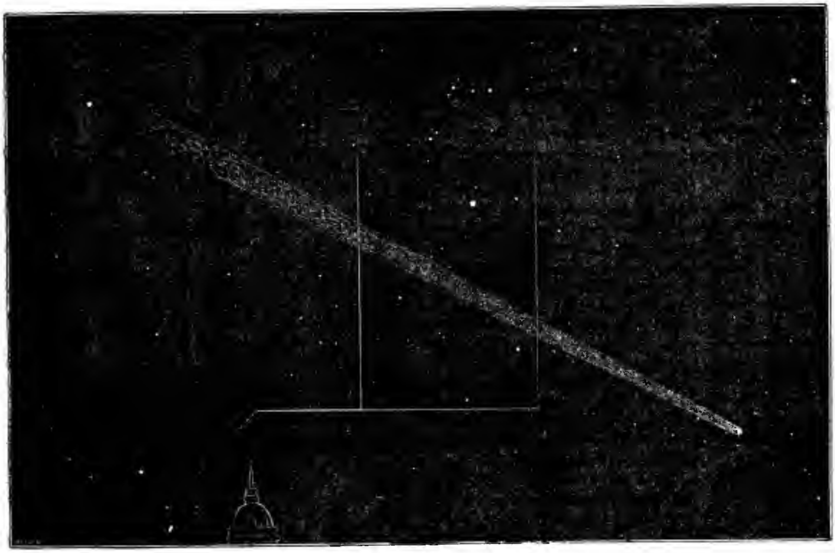


Fig. 4.—Comet of 1843.

As a comet approaches the sun, we have seen that a change takes place in the appearance of the coma and nucleus, and that in some instances a tail is generated. The process actually observed is generally this: in the forward part of the nucleus a turbulent action is seen to be in progress, leading to the propulsion towards the sun of jets or streams of misty-looking matter. Sometimes a regular cap or envelope is seen to be projected in this manner towards the sun, or even a set of envelopes one within the other. The matter thus thrown off is not suffered to pass very far from the nucleus towards the sun, but is swept away, as fast as formed, in the contrary direction. If the funnel of a steam-engine were directed forwards, instead of upwards, then the appearance presented by the emitted steam, as the engine rushed on (against a hurricane, suppose, to make the illustration more perfect) would exemplify the process which seems to be taking place around the front of the nucleus, and far behind it, as the matter formed is continually swept

rapidly from the sun, it has a tail pointing *forwards* (a tail which *must* be a different one, since—as Herschel says—"we cannot conceive a comet's tail to be brandished round like a stick"), and ninety million miles in length. So that, whereas the comet, already moving with a tremendous acquired velocity, had taken four weeks in traversing a distance of ninety millions of miles under the sun's *attraction*, the matter composing the tail had been thrown to the same enormous distance by the sun's *repulsion* in scarcely one-tenth part of the time, possibly (for the tail was formed when first seen) in a few hours!

The comet of 1843 (Fig. 4) was yet more remarkable for the dimensions of its tail and for its close approach to the sun. The tail of this comet stretched half-way across the sky in March, 1843. Its real length was two hundred million miles *at least*, for the end of the tail was lost to view through the excessive faintness of its light. So near did this comet pass to the sun, that many astronomers did not expect ever to see the comet again.

But after all but grazing the sun—sweeping round him at a distance of less than one-tenth of his diameter, the comet escaped and passed back again into space.

When we see the tail of a comet occupying a volume thousands of times greater than that of the sun itself, the question naturally suggests itself, "how does it happen that so vast a body can sweep through the solar system without deranging the motion of every planet?" Conceding even an extreme tenuity to the substance composing so vast a volume, one would still expect its mass to be tremendous. For instance, if we supposed the whole mass of the tail of the comet of 1843 to consist of hydrogen gas (the lightest substance known to us), yet even then the mass of the tail would have largely exceeded that of the sun. Every planet would have been dragged from its orbit by so vast a mass passing so near. We know, on the contrary, that no such effects were produced. The length of our year did not change by a single second, showing that our earth had been neither hastened nor retarded in its steady motion round the sun. Thus we are forced to admit that the actual substance of the comet was inconceivably rare. A jar-full of air would probably have outweighed hundreds of cubic miles of that vast appendage which blazed across our skies, to the terror of the ignorant and superstitious.

The dread of the possible evils which might accrue if the earth encountered a comet will possibly be diminished by the consideration of the extreme tenuity of these objects. But the feeling may still remain, that influences, other than those due to mere weight or mass, might be exerted upon terrestrial races in the course of such an encounter. The subtle breath of some mephitic vapour might penetrate our atmosphere, and, if it did not bring immediate destruction, might leave dire forms of plague and pestilence to work their evil will upon the human race. This fear is not, perhaps, wholly unreasonable, though—as will presently appear—the positive information we now have does *not* favour the supposition that the tail, at any rate, of a comet is likely to exercise such destructive effects. And it is only the tails of comets that we have much chance of meeting. On account of their enormous volumes, it is not so utterly improbable that we should encounter them as that we should meet the comparatively minute nuclei. In fact, there is reason for supposing that the earth actually did pass through the tail of the comet of 1861. At about the hour when it was calculated that the encounter should have taken place, a strange *auroral glare* was seen in the atmosphere, but beyond this, no effect was perceptible.

INTELLIGENCE IN ANIMALS.

Few of the questions raised in Darwin's "Descent of Man" are at once more difficult to deal with satisfactorily, or more important in their bearing on the subject of that volume, than the question how far animals possess mental powers akin to those of man. It is somewhat singular, we may remark in passing, that Darwin and Huxley, whose views in some respects are so similar, and who are regarded by the general public as standing side by side in their advocacy of the theory of the relationship of man to the lower animals, should seem to uphold almost exactly opposite opinions respecting the cerebral qualities of animals—one maintaining that in some cases animals reason, while the other (if we rightly apprehend what Huxley has said about animal automatism) will scarcely allow that animals even possess consciousness.

We propose here to consider some cases in which animals have seemed to reason. The importance of the subject will be recognised if we remember Darwin's admission that, had no organic being except man possessed any mental power, or if man's powers had been of a wholly different nature from those of the lower animals, we should never have been able to convince ourselves that our high faculties had been gradually developed. Darwin expresses his belief that there is no fundamental difference of this kind. "We must also admit," he says, "that there is a much wider interval in mental power between one of the lowest fishes, as a lamprey or a lancelet, and one of the higher apes, than between an ape and a man; yet this immense interval is filled up by numberless gradations." But this has not been so generally admitted, despite the evidence advanced by Darwin, as might have been expected. The feeling is still commonly entertained that a distinction exists between the mental qualities of the cleverest ape and the dullest and stupidest savage, which is utterly unlike any that exists among animals. In this essay we shall have to consider cases in which rats, cats, dogs, &c.,—animals all inferior in mental faculties, though not all in equal degree, to the more intelligent apes—have acted in ways which seem to imply reasoning. We shall treat these cases rather from the point of view of an opponent of Darwin's thesis above quoted than of a supporter, endeavouring in every case to find explanations not involving the exercise of reasoning faculties. But we must admit at the outset, that we find ourselves led to precisely the conclusion which he has indicated.

In the first place, we must recall to our reader's recollection those instances which have been selected by Darwin as so satisfactory, that in his opinion any one not convinced by them would not be convinced by anything that he could add.

Rengger states, says Darwin, "that when he first gave eggs to his monkeys, they smashed them, and thus lost much of their contents; afterwards they gently hit one end against some hard body, and picked off the bits of shell with their fingers. After cutting themselves only once with any sharp tool, they would not touch it again, or would handle it with the greatest care. Lumps of sugar were often given them wrapped up in paper, and Rengger sometimes put a live wasp in the paper, so that in hastily unfolding it they got stung" (the tenderness of some of these students of science towards animals is quite touching). "After this had once happened, they always first held the packet to their ears, to detect any movement within." These were not monkeys of the higher orders, but American monkeys, none of which are so near man in cerebral development as the orang, the chimpanzee, the gibbon, or the gorilla. The next cases relate to the dog, and are important, first, because two independent observers give evidence in the same direction; and secondly, because the action of the dogs can hardly be explained as resulting from the modification of an instinct. "Mr. Colquhoun winged two wild ducks, which fell on the opposite sides of a stream; his retriever tried to bring over both at once, but could not succeed; she then, though never before known to ruffle a feather, deliberately killed one, brought over the other, and returned for the dead bird. Colonel Hutchinson relates that two partridges were shot at once, one being killed, the other wounded; the latter ran away, and was caught by the retriever, who on his return came across the dead bird. 'She stopped, evidently greatly puzzled, and after one or two trials, finding she could not take it up without permitting the escape of the winged bird, she considered a moment, then deliberately murdered it' (the winged bird), 'by giving it a severe crunch, and afterwards brought away both together. This was the only known instance of her having wilfully injured any game.'

"Here," proceeds Darwin, "we have reasoning, though not quite perfect, for the retriever might have brought the wounded bird first, and then returned for the dead one, as in the case of the two wild ducks." If the dog had followed the wiser course, it would not have been quite so clear as in the actual case that he had reasoned, though the pause for consideration after an attempt to take both together, would have gone far to suggest that explanation. But the action of the dog in killing the bird seems quite decisive, because such an act was entirely opposed to the instincts of the breed and to the training which retrievers receive.

To these cases Darwin adds the statement that "the mulateers in South America say, 'I will not give you the mule whose step is easiest,' but *la mas racional*—the one that reasons best": on which Humboldt has remarked, "this popular expression, dictated by long experience, combats the system of animated machines better, perhaps, than all the arguments of speculative philosophy." Here, although Darwin only quotes Humboldt, he manifestly expresses his own view, and we find him opposed in a very definite manner to the theory of Kepler, afterwards supported by Descartes, and recently advocated by Huxley and others, that animals are automata, not possessing consciousness (or at anyrate that this theory is admissible).

The next case to be considered is one which was described a year or two since in *Nature*. It was not one which in reality demonstrated, or even strongly suggested, the exercise of reasoning faculties by animals. We quote it, however, because it illustrates well the mistakes into which want of care may lead the student of our subject. During the cold weather of last January, the writer of the letter in question put bread on the window-sills of his drawing-room for the benefit of the birds. These, finding food there, were constantly fluttering about the windows. "One day a large water-rat was seen on the window-sill, helping himself to the bread. In order to reach the window he had to climb to a height of about 13 ft.; this he did by the help of a shrub trained against the wall. Neither instinct nor experience," proceeds the correspondent of *Nature*, "will easily account for his conduct, since he never found food there before. If neither experience nor instinct, what, save reason, led him? His action seems to have been the result of no small observation and reasoning. He seems to have said to himself: I observe the birds are thronging the window all day; they would not be there for naught: it may be they find there something to eat; if so, perhaps I, too, might find there something I should like. I shall try." The way in which this story is told singularly illustrates the difficulty which we, as speaking animals, find in understanding how a process of reasoning can be carried on without the imagined use of words. Probably few men whose mental powers have been well trained carry on a process of pure ratiocination, without clothing with words the thoughts successively suggested to their minds. It almost seems to a mind thus accustomed to reason with a verbal accompaniment (audible to the mind's ear only) that any mental process not thus accompanied must be to some degree instinctive, and any actions resulting from such a process automatic. But it is certain that even the most intellectual sometimes act in a manner which, if noticed in an animal, would suggest the exercise of reasoning power, not only without putting their thoughts into mental language, but without, in reality, noting what they are doing. However, the point to be specially noticed about the above story is that the narrator overlooks the most obvious, and probably the true, explanation of the rat's behaviour. The rat could not see the food, but most probably he could smell it. If so, his adventuring up the wall to get it was not the result of reasoning, or, at least, not necessarily so, for that was the shortest path to the much-needed food. Possibly the birds themselves may have been an attraction to him. Certainly the case is not one which compels us to believe that water-rats reason.

This objection was so well urged, in company with other points necessary to be considered in such inquiries, by a German writer, Herr H. T. Finck, that we quote his remarks almost in full. "Before we ascribe to a rat such complicated reasoning powers," Herr Finck remarks, "it is necessary to ask if there is no other simpler way of accounting for the phenomena. I think there is. It is well-known that different species of animals vary greatly in the acuteness of their senses. To man, sight is the most important sense, and the same is true of many other animals and most birds. The cat is a representative of another smaller class of animals, whose most perfect organ of sense is the ear; while the dog lives in a field of sensatives, the most important of which are contributed by the sense of smell." This point, as dogs afford many of the most striking illustrations of reasoning, or of what looks like reasoning, in animals, must be carefully remembered. Few are aware, we believe, how imperfect a sense is sight with all dogs, as compared with our own sense of sight. We believe that there could not be cited a single instance tending to show that a dog has been able to see as well as a very short-sighted man would, while in the great majority of cases, it can be shown by a few easily-tried experiments that dogs scarcely see at all in the true sense of the word. Our sense of smell is probably not more completely inferior to the same sense with dogs, than is their sense of sight to ours. To return, however, to Herr Finck. After pointing out that the rat belongs to the class of animals who are guided by the sense of smell, he says, "It is evident, therefore, that the water-rat in question was led to the window-sill by his nose, which in his case was a more trustworthy guide than his eyes would have been. I do not wish to deny, by any means, that animals have reasoning powers. On the contrary, I am convinced that human and brute intellect differ only in degree, not in kind. But what we have to guard against is not to ascribe [he obviously means the reverse, that we are to guard against ascribing] to animals reasoning powers of a higher type than is consistent with the development of their brain, especially when the actions which seem to postulate such powers can be readily accounted for by simply bearing in mind the extraordinary acuteness of one or more of their senses. We are altogether too prone to judge the intellectual life of animals by the human standard—to imagine that the eye is everywhere, as with us, the leading source of knowledge. The neglect of the important *rôle* which the sense of smell plays in animal life has been particularly fruitful of errors in philosophical speculation. It has, among other things, helped to give a longer basis of life to the old theory of instinct, regarded as a mysterious power of nature." In passing, we may remark that at the very beginning of our own life the sense of smell is stronger and more useful than the sense of sight; as though during those first few days, before the eyes acquire power to recognise objects or to do much more than to distinguish light from darkness, we belonged for the time being to that inferior class of animals with whom the predominant sense is that of smell. In that part, also, of their lives, human beings seem so far to resemble the lower animals that their actions appear to be governed by instinct solely. In reality, probably, a sense of smell much keener then than during the subsequent years which alone we can remember, governs the actions in the same way, though not so obviously, as sight governs them in most of the actions of later years.

BIRDS WITH TEETH.

In the year 1861 a feather was found in a slab of lithographic stone from Solenhofen, which Hermann von Meyer assigned to an animal as yet not otherwise known, which he called *Archæopteryx lithographica*. Later in the same year, a large portion of the skeleton of Archæopteryx was discovered in the same formation. There were impressions of feathers radiating fanwise from each of the forelimbs. But Prof. Andreas Wagner, in a report to the Royal Academy of Sciences in Munich, expressed the opinion that the creature was not a bird, but a reptile, whose natural covering presented a deceptive resemblance to feathers. He called it the *Griphosaurus*, which (considering he had not seen the fossil remains) was very obliging on his part. Von Meyer, however, regarded the impressions as representing real feathers, belonging to the same animal as the feather he had already discovered. The fossil was secured for the British Museum in 1862. It is contained in two slabs of Solenhofen limestone; one representing the surface of tidal mud on which the bird lay at the time of its death, the other the layer deposited over the dead body. The lower slab shows the impressions of the tail, wings, and parts of the skeleton. The right shoulder-blade and upper arm (wing), as well as both the forearms, are well preserved. The head, the neck, and the backbone are wanting. Two of the digits of the wing (wing fingers we may call them) are free, and armed with sharp claws or recurved spurs. The right lower limb is well preserved, consisting of the thigh-bone, the tibia or larger lower leg-bone, and the tarso-metatarsal bones, or bones of the upper foot. To the metatarsus, four toes are articulated, one hind-toe and three fore-toes, which are jointed as with birds, and armed with strong recurved claws. "The foot," says Mr. Woodward, from whose description the above has in the main been taken, "agrees well with that of a true perching bird, but from the fanwise and rounded arrangement of the wing-feathers, it would appear to have been a bird of feeble flight."

Without entering further into the peculiarities of this creature, we note that while a few naturalists were doubtful as to its being really a bird, the majority were very confident that it was so. Professor Owen, in particular, pointed out that in one respect in which it differed most from modern birds it resembles the embryonic bird. Its tail-bones diminished gradually to the last, whereas in modern birds, the last vertebra of the tail is almost always the largest. But, said Owen, "All birds in their embryonic state exhibit the caudal vertebra distinct, and in part of the series [of embryonic changes] gradually decreasing in size to the pointed end one." The two-fingered and free condition of the wing-hand, that is of what corresponds to the hand in the bird's fore-limbs (which Owen pleasantly described as the biunguiculate and less confluent condition of the manus), he did not account for in the same way as a feature of an embryonic bird; but in some modern species the forward wing finger supports a claw, and the Screamer has two claws. All who at that time examined the fossil agreed that in all probability the creature had a beak like a bird.

But Mr. John Evans noticed somewhat later (besides a rounded mass which he took for part of the brain-pan, with a cast of the brain) what he regarded as a fossil jaw, on the slab on which lies the fossil body of the bird. It had been supposed to be the beak of Archæopteryx, but "great was my surprise," writes Mr. Evans, "when I detected along its right-hand margin, towards the apex, the distinct impression in the slab of four teeth still attached to it. The teeth themselves remain adhering to the counterpart, and are easily recognised by the lustre of their enamel." The teeth are thus described by Mr. Woodward. "The three which remain in a vertical position with regard to the jaw are about one-tenth of an inch long, and at intervals of about one-fifth of an inch. They consist of a slightly tapering flattened enamelled crown, about a twenty-fifth of an inch in width, and obtusely pointed, set upon what is apparently a more bony base, which widens out suddenly into a semi-elliptical form, so that at the line of attachment to the jaw the base of one tooth comes in contact with that of the next. So sudden and extensive is this widening of the base, that at first it gave me the impression that the teeth were tricuspidate, with the middle cusp far longer than the others. The front tooth of the four, which slopes forward from the rest, and is rather smaller than the others, shows little, if any, similar enlargement of its base. Of the fifth, which lies across the base of the foremost of the four, only a part is visible. There appears to be a well-defined line at the base of the teeth along their junction with the jaw, but I can offer no opinion as to the method of their attachment."

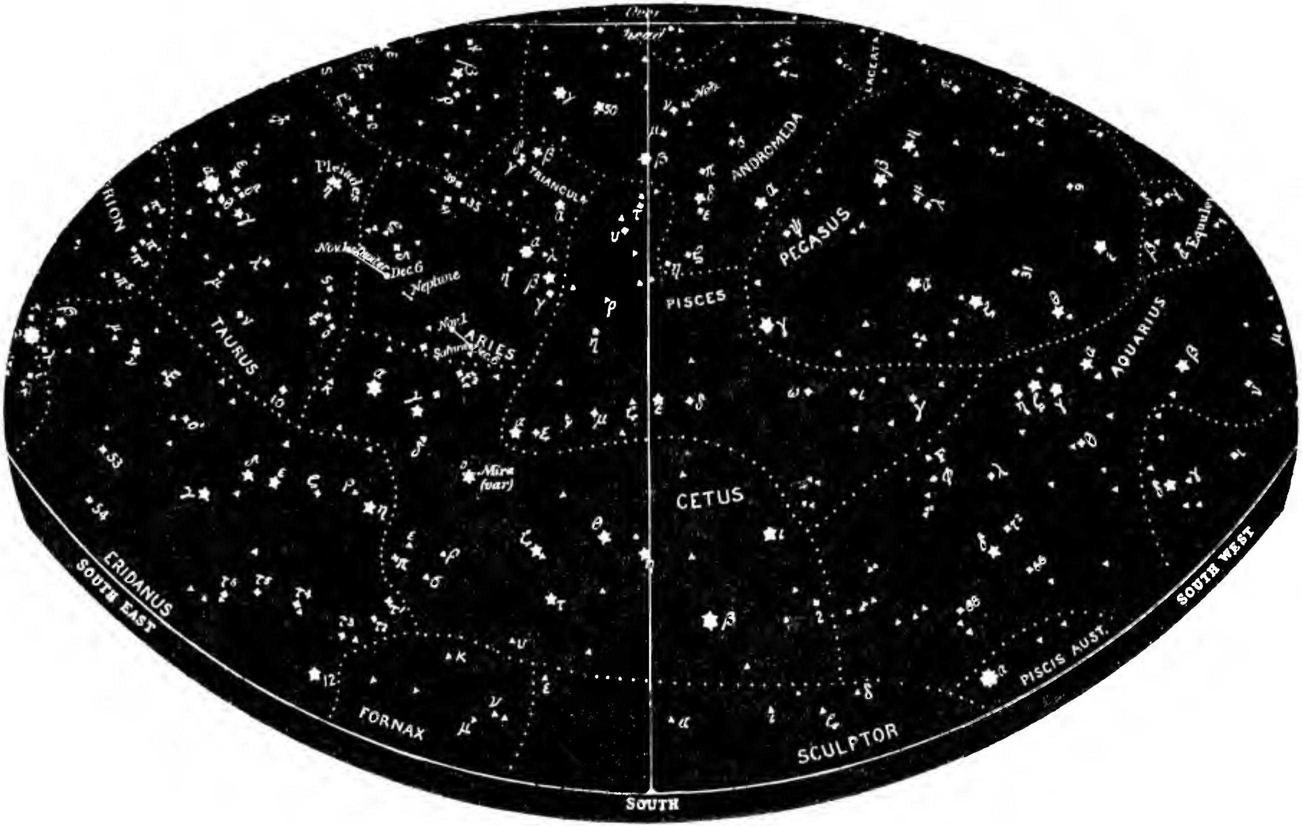
It seemed so unlikely when the above description was written that a jaw armed with teeth could belong to a creature manifestly bird-like, that many supposed the jaw belonged to some fish, though the jaws and teeth of fossil fishes from the same bed were found to be unlike this. Hermann von Meyer, referring to the drawings sent to him by Mr. Woodward, said that he knew of no tooth of the kind in the lithographic stone; nor were the teeth like those of Pterodactyles (the great reptiles with bat-like wings). "An arming of the jaw with teeth would contradict the view of the Archæopteryx being a bird or an embryonic* form of bird. But, after all," he proceeds, "I do not believe that God formed his creatures after the systems devised by our philosophical wisdom. Of the classes of birds and reptiles, as we define them, the Creator knows nothing, and just as little of a prototype or of a constant embryonic condition of the bird which might be recognised in the Archæopteryx. The Archæopteryx is, of its kind, just as perfect as other creatures, and if we are not able to include this fossil animal in our system, our shortsightedness is alone to blame."

Probably the theory that the Archæopteryx had teeth would still be regarded as little better than an assumption, had not other and more complete evidence been obtained, Professor Marsh discovered two fossil birds in the cretaceous shale of Kansas, which had well-developed teeth in both jaws. Of one of these birds—the *Ichthyornis Dispar*—"the teeth were quite numerous," Marsh wrote in *Silliman's Journal* for October, 1872, "and implanted in distinct sockets. They were small, compressed, and pointed, and all of those preserved are similar. Those in the lower jaw number about twenty in each ramus" (that is, on each side), "and all more or less inclined backwards. The series extend over the entire margin of the dentary" (or tooth-bearing) "bone, the front teeth being very near the extremity. The maxillary teeth" (those in the upper jaw, that is) "appear to have been equally numerous, and essentially the same as those in the mandible. The skull was of moderate size, and the eyes were placed well forward. The lower jaws are long and slender. . . . . The jaws were apparently not encased in a horny sheath."

* The word embryonic is here used with reference to the species, not to the individual. It signifies a form which creatures of the species presented before the type of the species had become, as it were, distinct and established. Traces of such past forms of a species are recognisable in the embryonic development of later representatives of the species.

[*Continued on page* 33.

THE SOUTHERN SKIES IN NOVEMBER.



This Map shows the position of the stars in the Southern Skies —

On October 30, at 11½ o'clock.	On November 7, at 11 o'clock.	On November 14, at 10¾ o'clock.	On November 22, at 10 o'clock.	On November 29, at 9½ o'clock.
On November 3, at 11¼ o'clock.	On November 10, at 10½ o'clock.	On November 18, at 10¼ o'clock.	On November 25, at 9¾ o'clock.	On December 3, at 9¼ o'clock.

In this map, as in the map for last week, the movements of some of the planets are illustrated. Saturn and Neptune have already passed opposition, or their place where they are due south at midnight. Jupiter comes to opposition on Sunday Nov. 13th. He is now nearly at his brightest.

*** Stars of the first magnitude are shown with eight points, stars of the second with six, of the third with five, of the fourth with four, of the fifth with three.

Continued from page 30.]
The shoulder-blades and the bones of the wings and legs were all of the true bird-like type. The breast-bone had a prominent keel. The wings were large in proportion to the legs. The wing-bones corresponding to the hand in man, were united as in ordinary birds. The bones of the hinder extremities resembled those in swimming birds. The bird was about as large as a pigeon. The species was carnivorous, and probably aquatic. Professor Marsh called the other form discovered by him *Apatornis celer*.

Later, Professor Marsh announced that having re-examined another fossil bird—a large diving bird nearly six feet high, found in the same cretaceous formation as the *Ichthyornis*, he found that it also had *teeth in both jaws, not in sockets*, like the Ichthyornis, *but in grooves*, as in *Ichthyosaurus*, the great lizard-formed marine reptile.

The skeleton of this toothed bird is pictured in our illustration. Prof. Marsh called it the *Hesperornis Regalis*. Before the discovery of teeth, Prof. Marsh had unhesitatingly classed the Hesperornis as a gigantic diver, though recognising peculiarities of structure. But recently, in a Monograph on the Extinct Toothed Birds of North America, he called attention to its resemblance in certain respects to the Ostrich. He says that if these characters are to be "regarded as evidence of real affinity, the *Hesperornis* would be essentially a gigantic swimming ostrich." Professor Huxley, on the other hand, says that the bird is "in a great many respects astonishingly like an existing diver or grebe—so like it, indeed, that had this skeleton been found in a museum, I suppose—if the head had not been known—it would have been placed in the same general group as the divers and grebes of the present day."

The teeth seem to have been admirably adapted to aid a diving bird (like a grebe) in catching its slippery prey. In the *Odontopteryx toliapicus* of Owen, the bony denticles were inclined at a considerable angle, but with the points forward, yet Professor Owen concluded that even such projections (they could not properly be called teeth) must have greatly assisted the bird in holding captured fish. In the existing bird, the *Merganser serrator*, the tooth-like serrations are inclined with the points backwards. These serrations, however, were not teeth, but merely tooth-like extensions of the horny covering of the beak. The teeth of the *Ichthyornis* and *Hesperornis*, as is shown by the smaller figure (showing a tooth, and, within it, a tooth forming to take its place) were unmistakably teeth. It does not take away from their dental character that they were set in a groove in *Hesperornis* and *Archæopteryx*, instead of in separate sockets, as in higher-toothed races and in *Ichthyornis*.

It should be added that Professor Marsh has examined the specimen of *Archæopteryx* in the British Museum, and fully satisfied himself that it belongs to the class of toothed birds. "The teeth seen on the same slab with this specimen agree so closely with the teeth of *Hesperornis*, that" he "identified them at once as those of birds, and not fishes."

He describes the leading characters of the ancestral bird in the following terms:—"In the generalised form to which we must look for the ancestral type of the class of birds, we should expect to find the following characters: Teeth in grooves; vertebræ biconcave" (that is, the bones of the backbone shaped somewhat as we see them in fish); "breastbone without a keel; tail longer than the body; bones of the hand and wrist, as also those of the foot, free; the bones of the pelvis separate; the sacrum" (or hind bone of the pelvis) "formed of two vertebræ; four or more toes directed forward; feathers rudimentary or imperfect."

If we consider the circumstances under which, according to the theory of evolution, the race of birds came into existence, we can understand that the ancestral creatures whence birds are descended presented many features in which they were not only unlike the birds of our time, but unlike any other race of existing animals. Were they not also, in all probability, very unlike each other? Probably there were much wider differences among the various orders of animals, which included all the ancestry of the modern bird, at the time when first *any* of the characteristics now regarded as avian first existed, than there are now among all the orders of existing birds. This certainly appears from the evidence obtained, not only respecting toothed birds, but also respecting those bird-like animals of which Huxley and others have shown that they were closely akin to reptiles—were, in fact, biped reptiles. We believe that the same holds with every species now existing, even with man—that, for instance, if we could have brought before us in rapid review all those creatures from which the human race of our time has descended (taking only those which belonged to one particular epoch, before man, specialised as we now find him, existed), we should not only find a far wider range of difference among these creatures than among the human races of the present day, but a wider range of difference than even exists between men and apes. There are *à priori* reasons for this view as regards the human race; but, apart from these, the evidence collected by Mivart in his work, "Men and Apes," while not, we think, available to show that there is no kinship between the Simian and Human races, seems only explicable on the assumption that the Simian ancestors of man differed widely *inter se*.

THE FIJI ISLANDS.*

THE ideas generally entertained respecting the Fiji Islands and their inhabitants are not such as to encourage the idea that life to white men would be very pleasant there. Probably most persons, who have not followed the changes which have recently taken place in this important group of islands, suppose that the Fijians are still, as they used to be considered, the most barbarous of all the Polynesians, addicted frightfully to cannibalism, and little changed from those who, as Herbert Spencer puts it, possessed such "extreme loyalty," that if the king willed it, a Fijian cheerfully stood unbound to be knocked on the head. The days are passed, however, when a Fijian king could register by a row of many hundred stones the number of human victims he had eaten. The Conservative Fijian sighs in vain for the good old times when the king's will reigned supreme. A visitor has now only to take with him, as Mr. Horne did, a circular letter of introduction written in Fijian to all the chiefs, to find himself a welcome guest at (instead of upon) their hospitable tables. "In each village some one, generally the schoolmaster, 'teacher,' or native clergyman was found, who could read and explain the letter to the people, who were at all times attentive listeners." The Sunday schools are well attended, and most of the rising generation of Fijians can do something in the way of reading, writing, and ciphering. In fact, with a few guides and an interpreter, a little sugar, tea, coffee, and biscuits, mats for sleeping upon, a rug or so, and a mosquito net, the visitor can enjoy himself immensely in the Fiji Islands, as Mr. Horne's work shows in almost every page.

While the cool weather lasts, Europeans in Fiji can wear with comfort clothing adapted to an English summer; "indeed, at this season, the weather is delightful, finer than the best summer weather in England." In the hot weather, it is true, the heat is oppressive, while storms of thunder and heavy rain are more frequent than pleasant. With reference to the rainfall, by the way, which even for a tropical country is very heavy, Mr. Horne notes a circumstance of considerable interest. "Previous to and during 1861-2 the low hills around Levuka were thickly wooded. Since that time the woods have been cut down, and the number of days on which rain falls has been reduced from 256, the average for 1861-2, to 149, the average for 1865-6 and 1876-7. It would seem that the number of showers diminished simultaneously with the cutting of the trees. The average rainfall has not been much diminished, however, and with an annual rainfall of 118 inches the Fijians may be well satisfied, especially as the rain falls most abundantly during the warm or summer season, when vegetation most requires it. It was absolutely necessary, moreover, to clear the forest region, for the thick woods afforded shelter to the mountaineers, who, on several occasions, appeared in large numbers, and threatened to sack the town and murder the white settlers. "These marauders came from Lasoni, in the centre of Ovalan, just across the mountains from Levuka; stole down upon the town, plundered the goods of the settlers, and then made off into the woods, where it was useless and dangerous to follow them." Unfortunately, since the woods were cleared the rain falls more torrentially than before, and carrying away the loose soil on the surface, where the ground is steep, does great damage to both soil and vegetation.

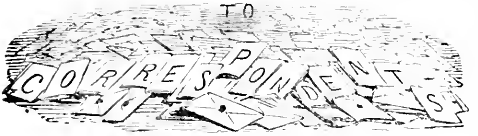
The Fiji Islands number in all 255, having an entire area of about 7,403 square miles, or about 738,350 acres. The largest island of the group, Vili Levu, has an area of 4,112 square miles, while the next in size, Vau na Levu, has an area of 2,432½ square miles. The others are all much smaller. About eighty of the islands are inhabited, the white population being about 2,000 (in 1871), the natives numbering about 140,500. As regards communication with the outside world, Fiji is not badly off. Twenty-four hours after the arrival of the mail from San Francisco at Sydney, a fine steamer of 1,000 to 1,500 tons leaves for Levuka, the voyage occupying seven or eight days. The steamer remains at Levuka nearly a week, and leaves with the mails for England in time for them to be transhipped to one of the Peninsular and Oriental steamers at Sydney. From Melbourne there is direct steam communication with Suva and Levuka once every five weeks. There is also regular steam communication between Levuka and Auckland (New Zealand), and between Levuka and the Friendly Islands. A visit to the Fiji Islands during the cool season would be pleasant for any one who enjoys change of scene; but it is clear from Mr. Horne's book that the naturalist (especially the botanist) would find such a visit at once interesting and profitable.

The natives are hospitable, as also (which is of more importance, perhaps) are the white settlers. The Fijians are daring sailors, and good customers to the boat-builders, who have taken the place of the native canoe-builders. The natives play a number of athletic games, among which may be mentioned throwing the *tinika*, or reed, wrestling, and a game which is something like tennis, a little like cricket, and a great deal like skittles. They throw the tinika (an oval-shaped piece of wood about four inches long and two in diameter at the thickest part) a distance of about 300 yards, or thrice as far as our best cricketers can throw a cricket-ball. The natives are subject to elephantiasis, and consider their children neither strong nor healthy till they have experienced an ulcerous disease, which they call *coko*. A kind of ophthalmia is not uncommon, but it lasts only a few days, both natives and settlers being subject to it. The natives have succumbed in great numbers to epidemics of measles, and many consider that the population has, in consequence, become greatly decreased. But Mr. Horne considers that the many abandoned "patches" may indicate rather a change in the habits of the people than a diminished population.

The animals indigenous to Fiji are bats, flying foxes, and a small rat. The ten species of snakes found there are all harmless. Pigs, introduced from Tonga (where Cook left them), run wild in the forests. There are wild ducks, snipe, sand-pipers, wild pigeons, and beautiful golden orange doves, as well as parrots or parroquets. Whales and porpoises abound in the seas round the group, which swarm with many kinds of fish, edible and otherwise. Sharks abound in the seas, and travel long distances from the sea to the deep pools in the rivers, which must render them less pleasant to bathers than they otherwise would be.

Mr. Horne's book is full of interesting facts, and though it has been specially written in response to an official invitation, it will be found very pleasant reading. There is a copious index—indeed, the index is a little too copious, a passing word in the text being, in many cases, all that is found to bear on a carefully-paged index heading. The facts gathered together here are the fruits of a year of faithful and laborious research.

* "A Year in Fiji: an inquiry into the Botanical, Agricultural, and Economical resources of the Colony." By J. Horne, F.L.S., &c., Director of Woods and Forests, and Botanical Gardens, Mauritius.



Letters to the Editor.

[The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with their writers. He requests that all communications should be as short as possible, consistently with full and clear statements of the writer's meaning.]

All Editorial communications should be addressed to the Editor of KNOWLEDGE; *all Business communications to the Publishers, at the Office, 74, Great Queen-street, W.C.*

All Remittances, Cheques, and Post-Office Orders should be made payable to Messrs. Wyman & Sons.

⁂ All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.

All Letters or Queries to the Editor which require attention in the current issue of KNOWLEDGE, *should reach the Publishing Office not later than the Saturday preceding the day of publication.*

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

Our Correspondence Columns.

THE 1 INCH MAP OF THE ORDNANCE SURVEY.

[4]—From time to time we hear expressions of admiration at the beauty and fidelity of those maps of Great Britain and Ireland, based upon that great Trigonometrical Survey, which really began in 1784. In fact, an "Ordnance Map" is with many people a kind of synonym for all that is topographically accurate in the shape of cartography. I venture, however, to express my belief that praise is bestowed upon these maps—be it observed that I am now speaking only of those on the scale of 1 inch to a mile—I say, I believe it will be found that praise is bestowed upon them in the inverse ratio of their employment for purposes where minute accuracy is essential. I have said that the survey began in 1784, because it was in that year that the Triangulation for correcting the Observatories of Greenwich and Paris was commenced by the measurement by General Roy of his famous base of 27,404 feet on Hounslow Heath. I believe, however, that what is now known as the Ordnance Survey had its beginning in 1791, when proceedings were initiated by the remeasurement of the base of which I have just spoken, by Colonel Williams, Captain Mudge, and Mr. Dalby. As an illustration of the manner in which, up to a pretty recent period, the maps on which I am commenting were produced, I may here say that one purchased within a comparatively few years lies before me as I write, and bears upon the upper part of it the legend, "Published 1st February, 1813, by Lieut.-Colonel Mudge, Tower." Now, it is abundantly evident from this, that an old plate must have been worked upon and patched up year after year, alterations being engraved somewhere about the place in which they had occurred; but, as I shall immediately show, they seem to have been put in merely by eye, so erroneous are a large proportion of the minor details when tested with minute precision. To give a single illustration: the mark + stands for a church upon these Maps; but a query as to whether the intersection of the arms of the cross gives the site of the spire (the conspicuous part from which it might well be supposed that all measurements would be made) is always met at the head office at Southampton by the reply that the cross is merely a conventional sign, and does not represent any part of the church in particular! Another stock answer, too, to complainants at head-quarters is that distortion arises from shrinkage in drying the maps: such shrinkage being unequal, and dependent upon the manner in which the maps are hung up to dry after printing. Whence it would appear that after between 79 and 80 years' practice and experience, the combined science of the entire staff at Southampton is unequal to provide for the equable shrinking of a sheet of damp paper. I have been led into these remarks by a recent piece of personal experience, in the shape of the attempted identification of all the chief objects visible from a height, by the aid of a 1-in. Ordnance Map and a 6-in. transit theodolite. One observation will suffice to illustrate the ground of my complaint. It is that of a large and conspicuous church-tower, 8½ miles from the place of observation; the angle between which and the meridian differed 51′, as measured by the instrument and on the map; 51′, 8½ miles off, representing, it is almost needless to add, about 706·7 feet or 235½ yards. The fact seems to be that, while the greater triangles have been surveyed with all the refinements of Geodetical science, the filling in of the details has been done "anyhow." I have, I verily believe, seen quite as accurate plans made by pacing, and with an ordinary prismatic compass. Such of the 25-in. maps as I have examined really do seem to afford remarkable examples of painstaking correctness; and I am informed that those on the 6-in. scale are equally praiseworthy. Moreover, I learn that a new series of 1-in. maps, reduced from this 6-in. survey, are in the course of publication. If this be so, we may live in hope that we shall some day be in possession of a series of portable maps, vieing in accuracy with those issued by the German and other Continental Governments. Meanwhile, let no one go into a shop to purchase one of the present 1-in. Ordnance Maps under the impression that he will receive a rigidly correct chart of the region professed to be represented: because he will not.

A FELLOW OF THE ROYAL ASTRONOMICAL SOCIETY.

IS THE SUN HOT?

[5]—Will you permit me to remark that "Anti-Guebre's" letter (No. 1., p. 15) is in some respects a repetition of an article of mine entitled "The Astronomy of the Future," which appeared in *Fraser's Magazine*, Nov., 1876, since published in a volume, "Pith," in which I endeavoured to maintain that, in spite of the revelations of the spectroscope, the theory that the sun is incandescent is wholly untenable and improbable. My reasons for thinking so are given at length in the paper mentioned; but perhaps it may be as well to repeat here that if we start with the supposition that the sun is a gigantic galvanic battery, there can be no more reason for believing it to be red-hot, than there is for assuming that the battery we use in our laboratory is a sort of furnace.

Light and heat are surely phenomenal products, caused by magnetic and electrical forces, in a state of intense activity, acting upon atmospheric conditions; so that we are at perfect liberty to maintain that Mercury need not be any warmer or more illuminated than is our earth or Jupiter. The sun may possess the power of producing the phenomena of incandescence, without itself being incandescent.

The inflammatory action apparent on the face of the sun may be merely the chemical conversion of substance into force; and if we could see the working of a dilute acid on the surface of the metallic plates in a galvanic battery, we should probably discover on a minute scale a corresponding commotion to that which is so conspicuous on the sun.

As stars differ from each other in their material composition, it is only natural that the revelations they make of themselves in the spectroscope should be also constitutionally different.—Yours, &c.,

NEWTON CROSLAND.

[6]—You wish for more reasons in favour of the sun's being a cold body. Why is it that, although we have reflected sunlight from the moon, we do not see that light on its way to that body? Surely there would be a broad flood of effulgence along the heavens.

TYRO.

INFLUENCE OF SEX ON MIND.

[7]—Permit me to take exception to the title of the article "Are Women Inferior to Men?" To hold that woman's mind is, like her body, naturally weaker than man's, does not imply absolute inferiority. Overwhelming evidence to prove woman intellectually weaker than man can be classed under five heads:—1. *Anatomical.*—Head-forms, shape of skull, size of brain. 2. *Physiological.*—Woman is always, more or less, an invalid. Hence, if supposed equal to man in intelligence, she is heavily handicapped by her physical organisation. Sex must influence mind. 3. *Historical.*—If the sexes are equal in mind, why has man's intellectual work so far surpassed that of woman? Why was not the alleged sexual mental equality asserted and proved long ago? 4. *Daily Experience* that women cannot argue, and never see more than one side of any question. 5. Woman's *Superior Instinct;* a decisive proof that she has *less reason* than man.

The subject is highly interesting and important, as determining woman's proper sphere and education. If permitted, I would gladly place my views concisely before your readers.

Nov. 5. Yours, &c., J. MCGRIGOR ALLAN.

[Certainly.—ED.]

THE NOVEMBER METEORS.

[8]—In your first number you invite correspondence, and I therefore make no apology for writing and suggesting that, as your publication is intended for beginners in science, it might be an advantage

if in your next number, it were pointed out at what hour, and in what direction, intending observers should look out for the November meteors—that is, as proposed in the current number, use "plain untechnical language", and may I further suggest that (making use of your own two typical words), if you write about small-faced or long-headed men, you would *combine with them* the technical word—e.g., "the race was dolichocephalic, i.e., long-headed." By this means your readers will understand the technical terms, without referring to their lexicons, when they come across them in more advanced books.

London, Nov. 9, 1881. A. T. C.

[The meteors of November 13-14 will hardly be seen this year. If seen at all, it will be after midnight, and they may equally well be looked for in all parts of the sky.—Ed.]

THE EARTH'S INCLINATION.

[9]—It may seem a ridiculous question to ask you to answer, but I can find no satisfaction in scientific primers. Will you kindly tell me, simply and clearly, the reason of this:—The earth's axis is at an inclination of 23° (?) to the sun. Very good. Now suppose the northern hemisphere be nearer that body, if the earth should spin around itself and the sun, why, when it gets on the opposite side, will the northern hemisphere be turned from it? Does the bottom of the body move out [? move nearer], while the top recedes in its annual revolution? TYRO.

SPEED OF AMERICAN ICE-YACHTS.

[10]—With reference to your reply to my query at p. 16, I must confess that while I see the reasoning I had supposed conclusive is not so in reality, yet I fail to understand how there can be any driving force on a vessel driven by the wind when her velocity is equal to that of the wind. It seems to me the observed fact that an ice-yacht travels faster than the wind can only be explained by supposing that the wind comes occasionally in blasts much stronger than the average force of the wind. A fierce blast might give an ice-yacht a velocity much greater than the average velocity of the wind, and owing to the slight frictional resistance, she might retain that velocity with very little loss until the next blast came.

UPSILON.

[11]—An explanation of the great velocity attained by ice-yachts is given in the August number of *Scribner's Magazine*. It runs as follows:—An ice-yacht makes "the greatest speed on that course in which she meets with the least air friction, receives the strongest push of the wind in a forward direction, and yet does not lose the wind too much by her own speed. This course is running free, with the wind on the quarter, or about one hundred and thirty-five degrees off her course. Suppose the boat heads north-west, while the wind blows from the west. Now, her speed diagonally across the wind causes her to receive the wind on the beam, as if it blew from the north-west. She practically has a wind on the beam; this offers but little air friction against her forward motion. The running-friction is so slight that the boat keeps her way, the direction of the sail-push is sufficiently forward to be advantageous, and, lastly, her diagonal course, partly with and partly across the wind, saves her from losing too much of the wind's force by her own speed. Suppose that a twenty-knot breeze blows from B to C, and that she heads towards D, while the wind, represented by the arrow A, blows in a given time to C, it carries the boat with it in nearly the same time; but as she heads diagonally across the wind, she is obliged to run the long distance from B to D, while the wind blows only from B to C; she therefore beats the wind. Her speed is limited only by the loss or change of the wind through the effects of her own velocity. The greatest velocity of an ice-yacht is not recorded, because her finest runs occur either at unexpected moments, or when she sails over unmeasured distances. But the time over short and long courses has often been taken. The distance from New Hamburg to Poughkeepsie is over seven miles. The *Sn[illegible]* ran this course in seven minutes. This is the quickest time on record; but many winters the trip has been made in from nine to ten minutes. This speed is attained with a stiff breeze on the beam or on the quarter, and when the ice is tolerably smooth and clear of impassable cracks. But an ice-yacht very seldom runs a straight course for even a mile. Various obstructions have to be avoided; the wind changes direction very often, and also comes and goes in fitful puffs over the hills. The consequence is that she makes a very crooked course at very uneven speed; she goes more than seven miles, and sails at her full speed during much less than seven minutes. Probably she flies at times from eighty to one hundred miles an hour. The speed of an ice-yacht, in working to windward, which is her poorest course, is from ten to fifteen miles an hour, against an eight or ten-knot breeze."

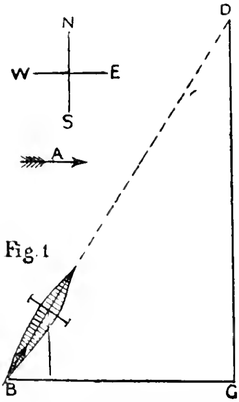


Fig. 1

YACHTSMAN.

[Thanks to "Yachtsman" for his interesting extract. I do not think the explanation, however, is strictly satisfactory. What we want to have explained is just what the explanation takes for granted, namely, *why* the ship runs the longer distance BD, while the wind has gone over the shorter distance BC. Any account which simply says that she does, or that she is obliged to, can hardly be regarded as an explanation. When we consider, too, that with every change of her velocity there is a change in the direction in which the sail-push acts, of which the above so-called explanation takes no account, it must be admitted that it is open, as an explanation, to objection. The explanation referred to by "Upsilon," which I gave in the *Newcastle Weekly Chronicle*, ran somewhat on this wise:—

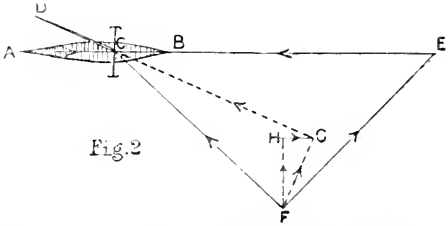


Fig. 2

Let AB be the centre line of the yacht (bow at B), CD her sail. Let CE represent the yacht's velocity at any moment, the effect of which is, of course, equivalent to the existence of a wind blowing in direction EC, and having its velocity represented by EC. Let the actual wind be in direction FE (which would be, if the yacht were at rest, a wind on her quarter), and represented in velocity by FE. Then, by the principle known as the "triangle of forces," we know that the effective wind comes in direction FC, and with a velocity represented by FC. Now, we must resolve this into the two velocities FG, perpendicular to the sail, and GC parallel to it; of course the resolved part GC corresponds to a wind which could have no effect whatever, and therefore may be neglected. What remains is a wind blowing with velocity FG square to the sail. This wind we resolve in turn unto two, one HG, parallel to the yacht's course, the other FH, perpendicular to it. The last has no effect in urging on the yacht, only inproducing lee way. The effective driving wind with velocity HG remains, and unless the accelerating force of this wind is counterbalanced by frictional resistance, it will increase the yacht's velocity, which already exceeds that of the wind.—Ed.]

ARE MEN'S HEADS SMALLER THAN OF YORE?

[12]—In a letter to your contemporary *Nature*, Mr. W. B. Kesteven states that in the hat trade the opinion is prevalent that within the last thirty or forty years the size of men's heads has diminished. "The following statement," he says, "has been given to me by a hatter, whose name has attained a pre-eminence lasting more than a generation." "Five-and-thirty years ago, when I was a young man, we used to purchase hats for retail trade in the following ratio:—

Sizes	21—21½—22—22½—23—23½ inches
Relative number	0— 1 — 2— 4 — 3— 1

At the present time," he adds, "I am selling hats in this ratio —

Sizes	21—21½—22—22½—23—23½ inches
Relative number	3— 4 — 3— 1 — 1— 0"

A manufacturer writes:—"I should say that heads generally are two sizes less than at the time you refer to. A head of more than twenty-four inches' circumference is now quite a rarity, whilst we

make thousands of hats for heads with a circumference of about twenty-one inches." "I have received similar statements," Mr. Kesteven says, "from other members of the trade, both wholesale and retail. The statement comes to me not only from men of experience in the trade, but from men of intelligence and observation exercised beyond the limits of the shop or the factory. It is, I am informed, extensively believed among hatters; it may, nevertheless, be merely a general impression. The diminution, it is said, is observed mostly among grooms and men of that class in the social scale. If this be really the case, the change should be noticeable also among soldiers. The diminution is possibly more apparent than real, and may be traceable to alteration in the style of hair-cutting, or of wearing the hat. It has been suggested to me that men of the present generation have from birth smaller heads, dependent upon an alteration in the dimensions of the female pelvis, in consequence of modern fashion in dress. Of this opinion, however, I obtain no confirmation from eminent obstetricians of whom I have made inquiries. The statement, then, as it stands, is wanting in explanation, and calls for further investigation." Mr. Kesteven quotes the reply sent him by Professor Flower to his question as to the statement made by the hatters, "that men's heads were smaller than they were twenty years ago":—

"Before drawing any important conclusion from such a statement, it would be necessary to know much about the authority upon which it is made. Who, for instance, are the hatters that make it? Do all hatters concur in the same statement? Is it a mere general impression, or is it founded upon actual arithmetical data? Does it refer to any particular class of men, and does it refer to the same class of men? If it should be true, may it not arise from some change of fashion, if only founded upon the size of the hat, and not of the head) other even than the one you suggest of hair being worn shorter—such as hats being worn more on the top of the head than formerly (in old-fashioned prints one sees the hat well down over the ears, which is certainly not the case now), or, perhaps, hats of the kind specified being now worn by a different (perhaps lower) class of the community, or by younger people? All these questions must be considered, and, perhaps, other sources of error eliminated which may not occur at first, before the statement can be accepted. If the evidence of the statement appears to bear investigation, it would be well worth while following it up, as, if true, it would be one of the most remarkable facts with which I am acquainted, that in the space of twenty years a material diminution in the average size of the heads of the same population had taken place—a fact so contrary to all theory and to all experience."

Professor Flower's opinion seems to me very much to the point. I may note, in addition, that the different material of which hats were made thirty years ago may have something to do with the supposed change. Those who remember the heavy beavers of that period will hardly doubt, I think, that they must have been worn more loosely-fitting than the lighter hats of the present time.

Can any readers of KNOWLEDGE throw light on this subject? Considering that the hope of the future lies much in our growing men with larger heads than now, it would be a serious matter were the hatters right.

Are grooms and men of that kind drawn now from the same classes as of old? May not the best of those classes now seek better employment? Or may not emigration have had something to do with the supposed change?—Yours, &c.,

CEREBRUM.

[The question raised by Mr. Kesteven seems to us of considerable interest, though it is utterly unlikely that within so short a time, any change, such as hatters suppose, can really have taken place in the size of men's heads—even if, which is almost as unlikely, any change in the direction suggested is going on at all. We may mention one circumstance, which, however, would hardly affect grooms. Wigs were certainly more commonly worn thirty years ago than now, and wigs in those days were wigs indeed. The average size of hats must have been quite appreciably greater in those times on that account alone, we should imagine. It is, however, really true that hats of 23½ inches are no longer kept in stock? We should have supposed, from our own observation, that in any good hat-shop a hat of 24 inches could generally be obtained. This leads us to consider another point. Possibly hatters measured heads differently in former times than at present. If they measured round the head then, instead of taking, as now, the two diameters of the cranial oval, they would certainly have had a higher average for the circuit of the head. Any one who has examined the head-shapes in American hat-shops will know that nine heads out of ten are quite irregularly shaped. We have seen some having an outline more like a long oblong than the oval which a well-shaped head should have. But taking the case of a regular oval (or egg-shape), or even a truly elliptic head, the true circumference would be somewhat greater than that inferred from the hatter's reckoning. Take, for instance, a head section having diameters 6 and 7: then, if I remember rightly, the hatter would call the circumference 3 times 6½ inches + 1 inch (*i.e.*, an inch more than three times the mean between the two diameters), or 20½ inches. Now the actual circumference would be in the case of an ellipse—

$$7\pi\left[1 - \tfrac{1}{4}\cdot\frac{13}{49} - \frac{1}{64}\,\frac{(13)^2}{(49)^2}\,\&c.\right]$$

$$=\frac{3\cdot1416}{21952}\times 113303$$

or 20·58 inches; that is, nearly a tenth of an inch longer. In the case of an oval shape the difference would be about a tenth and a half, while, in the case of an irregular head, it would be fully a quarter of an inch. Where the section of the head is long (dolichocephalic), the difference between the estimated and the measured circumference would be much greater.—ED.]

THE FIFTEEN PUZZLE.

[13]—I am told that in a magazine article which appeared some time since, you have attempted to show that there are positions in the Fifteen Puzzle from which the won position can never be obtained. As I believe that the won position can be obtained from any position whatever, including that in which the numbers 13, 15, 14 appear in that order on the last line, I should like to know how the reverse has, in your opinion, been demonstrated.—BOSS.

[I thought the Fifteen Puzzle was dead, and hoped I had had some share in killing the time-absorbing monster. (It is an excellent puzzle, by-the-way, except when the puzzled ones try to do impossible things with it.) I have no doubt "Boss" has succeeded in obtaining, from the losing position he names, or others of the same class, what he regards as a won position. For instance, he may have obtained the arrangement—

	1	2	3
4	5	6	7
8	9	10	11
12	13	14	15

or some other, which seems as satisfactory to him as the true won position. But he cannot have obtained this last. The proof would occupy too much space to be given here. But "Boss" may try this. Taking any positions at random, let him take the fifteen numbers one after another as they occur, and for each let him count how many come after it which ought to precede it (running along the lines in the way in which we read the lines of a book, or as the numbers run in the won position). Let him add together all the numbers he thus obtains, and call the result the "total displacement." This number will be either odd or even. The vacant square will be either on an odd line (the first or third) or on an even line (the second or fourth), or, for convenience of expression, say the vacant square will be either odd or even. Now, he will find that if the "total displacement" and vacant square are both odd or both even, so they will remain after any change he may make by sliding a block, after two such changes, after three, in fine, after any number of legitimate sliding changes. If the "total displacement" is odd and the vacant square even, or *vice versâ*, so will they be after any number of legitimate sliding changes. ("Boss" will readily see the *raison d'être* of this, after examining a few cases.) No amount of changes, then, will cause the "total displacement" and the vacant square to be both even or both odd, unless they were so at the outset. As they have to be both even in the won position (for which the total displacement is 0, an even number, and the vacant square on the fourth, an even line, whereas, when the last line runs 13, 15, 14, the total displacement is odd and the vacant square even) no amount of changing will bring the losing position, mentioned by "Boss," to the true won position.

In the article referred to I showed that what, as I have above said, will be found on trial in any given cases, must be universally true. I also showed, in a part of the article which most readers found rather tough reading (there were several misprints, too, the article having been written when I was in Australia), that from any position any other of the same class, either losing or winning, can be attained. As there are more than ten millions of each kind, it is not wonderful that the proof of this general proposition was not altogether simple.

It is singular to think that though probably not fewer than twenty millions of persons tried the Boss Puzzle, probably not a

hundredth of the multitudinous positions of which the pencil-ledicats were occupied among all the positions (set up, attained, or passed through) in the thousands of millions of trials those millions made.

If "Boss" is not content, let him try the easier task of bringing eight blocks from the position shown in Fig. 2 to that shown in Fig. 3.

1	2	3
4	5	6
8	7	

Fig. 2.

1	2	3
4	5	6
7	8	

Fig. 3.

This is a less complicated task than that of the original "Boss" puzzle, for instead of more than 10,000,000, there are only 181,440 losing positions in a nine-square puzzle. Or he might try to change from

1	2	3
5	4	

to

1	2	3
4	5	

There are only 360 losing positions in a six-square puzzle. Or finally from

1	
3	2

to

1	2
3	

, where he has only twelve losing positions to trouble him. He will see at once that the last task is an impossible one; but he may rest assured it is not more completely so than the others, and it wastes much less time.—Ed.]

Queries.

[1] Ultima Thule.—One often finds the expression *Ultima Thule* used as if indicating the remotest known regions of the earth. Where in classical authors is this expression used, and in what sense?—Alpha Sigma.

[2] A Fifteen Puzzle.—Most of your readers are no doubt familiar with the puzzle how to send out fifteen school-girls walking, three and three, so that during seven successive walks no two of them shall be more than once in the same set of three. Is there any mathematical treatment of this puzzle corresponding to the mathematical treatment of permutations and combinations? It seems to me that there should be. For instance, one might begin by showing that nine persons could walk out three and three (with the same restriction) for four successive days, and then apply the method of demonstrative induction to show that if $3\,(2n-1)$ persons can be arranged to walk out in the required manner on $(3n-2)$ different days, then also $3\,(2n+1)$ persons may be arranged in the required way on $(3n+1)$ different days. It will be obvious that the number of persons must be of the form $3\,(2n\pm1)$; that is, three times some odd number. The successive numbers to be dealt with are, therefore, 3, 9, 15, 21, &c., and the number of days in which the three can go out in the required way are respectively 1, 4, 7, 10, &c.—Rusticus.

[3] Flight of Birds.—Is it the case or not, that the flight of birds is greatly aided by the presence of warm, and, therefore, light air in the air passages of the bones?—Aerial.

[4] The Earth's Inclination.—In diagrams explaining the season's differences, the figure of the earth is shown to rotate on an oblique axis. Were it to rotate on an upright axis, would not perpetual summer reign at the poles, with greater heat at the equator?—Moonstruck.

[5] Hot Winds, Cause of?—The difficulty is as to the force from front or back which will overbalance the tendency to *rise* over the hot area. Take the case of a Mediterranean sirocco, why is there not rather an *in*rush to the hot sandy plains?—L. S.

[6] Lying To.—Wanted, diagram of forces when a ship lies to, 1° under sale, 2° under steam.—L.S.

[7]—Flying Bridge.—Wanted, diagram of forces, arrangement of rubber, position of ropes, when the vessel has passed the middle of the river.—L. S.

[8] Volume of Sphere.—Is there any simple way of showing that the volume of a sphere is two-thirds that of the enclosing cylinder?—Archimedean.

[9] Sun-dial.—Can any of the readers of Knowledge describe any method of constructing a sun-dial by which the error arising from the shadow not being sharp may be got rid of?—Solar Time.

[10] The Zoetrope.—In the zoetrope we get a series of pictures of a moving body, showing it in a certain number of positions from among the infinite number that it passes through in accomplishing the movement illustrated. Can any one explain how it is that the impression conveyed to the mind by this incomplete series of views is that of continuous motion? It seems to me that we ought to recognise a certain jerkiness in the apparent movement. But in well-arranged zoetropic illustrations no jerkiness can be noticed.—Zulu.

A Cargo of Human Bones.—New York, Sept. 11. A special from London says:—A great sensation was caused at Bristol by the discovery of a cargo of three hundred tons of human bones being discharged there to the order of a local firm engaged in manufacturing manure. The bones were shipped from Rodosto, at Constantinople, and are supposed to be the remains principally of the defenders of Plevna. There are complete limbs among the horrible cargo, and in some cases the hair still adheres to the skulls. Peter Cooper says it is a common thing among the British to buy human bones. "In fact, they will take all they can get at any time and from any part of the world. They use them for manuring their lands. I have often heard it said that England was manured with bones taken from the battle-field of Waterloo. There is no finer to be had."—*New York Herald.*

Science in the Police Court.*—At Bow-street last week, Mr. Waddy, Q.C., attended before Sir James Ingham, with Mr. Besley and Mr. Bernard Coleridge, to apply for a summons under the Vivisection Act, against an eminent professor of science. Special reference was made to the subsections providing for a licence to be granted to any one practising vivisection, and for the administration of some anæsthetic of sufficient power to prevent the animal feeling any pain during the experiment. If pain was likely to continue after the effect of the anæsthetic had ceased, or any serious injury was inflicted on the animal after the experiment, it was to be killed before it had recovered from the effects of the anæsthetic. When it was desired to extend the experiments for a lengthened period, it was necessary to procure a certificate authorising the extension of the experiments, and the summons was asked on the ground that this provision in the Act had not been complied with. In support of the application the learned counsel read extracts from a report in the *Lancet* on cerebral localisation, the subject having been brought under the notice of the recent International Medical Congress. One of the speakers was Professor Goltz, who had experimented upon two dogs by exposing the surface of the animals' brains, and washing away large portions of the substance by subjecting it to the action of a powerful stream of water. With reference to this experiment, Professor Ferrier has expressed his views upon the subject and gave details of some experiments he had made upon two monkeys. These animals had been operated upon some months previously, definite motor paralysis being procured in one, and in the other absolute and perfect deafness. The animals subjected to these experiments were produced by each of the professors, Professor Goltz asserting that he had removed the greater part of both hemispheres, including all the supposed motor and sensory areas. That the operative procedures to which the animals had been subjected had been extensive was quite obvious upon examination of the skulls, large gaps in the continuity of the upper and external walls of which were felt. Saving some clumsiness in its movements, one of the dogs showed but little signs of injury. It appeared possessed of considerable intelligence, and certainly did not suggest to the onlookers that it was a dog demented. In startling contrast were the two monkeys exhibited by Professor Ferrier. One of these had been operated upon in the middle of January, the left motor area having been destroyed. There had resulted from the operation right-sided hemiplegia, with conjugate deviation of eyes and head. Facial paralysis was at first well marked, but ceased after a fortnight. From the first there had been paralysis of the right leg, though the animal was able to lift it up. Its arms it had never been able to use. Lately rigidity of the muscles of the paralysed limbs had been coming on. The other monkey, as a consequence of paralysis of its auditory centres, was apparently entirely unaffected by loud noises, as by the firing of percussion-caps in close proximity to its head. From the *Times*.

* The report would be somewhat clearer if it were not taken throughout for granted that the readers must necessarily be medical students. Why should not paralysis of the right side be written, instead of right-sided hemiplegia? wryneck and squinting for "conjugate deviation of eyes and head?" The "motor and sensory areas" are, of course, those parts of the brain which are supposed to regulate respectively the movements and the sensation.

UNHEALTHY HOUSES.*

In an interesting lecture at the London Institution, Prof. F. de Chaumont mentions the following causes of disease:—

(1) Want of renewal of air in our rooms.

(2) The pollution of the house air by admixture with sewer emanations.

(3) The contamination of our water supply.

He says that could arrangements be effectually carried out in our dwellings for removing these causes of disease, certain much-dreaded maladies might disappear altogether. "But we must further remember," he proceeds, "that it is not death alone we have to dread, terrible as its effects often are in a household. For every case, we have to bear in mind, argues a considerable number of cases of illness, which, even if recovered from, may leave consequences behind them sufficient to affect the health of a lifetime, and to diminish the power of the sufferer for the work he has to do. The duration of illness alone is often a serious break in the life of a professional man, artisan, or labourer—a break which in some cases may mean the difference between comfort and penury, or between a successful career and a struggle for existence." "I think I shall be understating the case," he says, "when I say that each case of death argues about a dozen cases of illness, although the number is somewhat less in the severer diseases, such as enteric fever, which is fatal in one out of six cases, and diphtheria, which kills one out of three." Taking even these, however, Prof. de Chaumont shows that in London alone from 50,000 to 60,000 weeks of productive labour are totally lost to the community from illness due almost entirely to the unhealthiness of our houses.

The lecture was delivered for the purpose of bringing before the public the subject of sanitary assurance, and to advocate the cause of the Sanitary Assurance Association, founded in November, 1880. The objects of the Association are as follow:—

"The practical application of Sanitary Science generally, and especially the encouragement and development of proper systems of house-drainage, water supply, and ventilation.

"The examination, inspection of, and reporting on houses and buildings, or plans of houses and buildings, either erected, in course of erection, or to be erected, as regards their sanitary arrangements; the supervision by the officers of the Association of any work done, or to be done, by or on behalf of the members of or subscribers to the Association in connection with the sanitary condition of such houses or buildings; and the granting of certificates relative thereto.

"The publishing of reports on matters connected with the progress of sanitary science in the United Kingdom and abroad, and the distribution at the end of each year of any surplus funds, or part of any surplus funds, to such institutions as are devoted to the advancement of sanitary science."

If there is one application of science in which all should be interested, it is the endeavour to diminish disease and suffering. Were there no nobler reason, sheer selfishness might well cause all men who claim to be reasoning beings to join, each to the best of his abilities, in helping the cause of sanitary reform. But this is in truth a case where the good of each is the good of all.

That the objects of the Association may be the better effected, it has been formed of two classes—(1) Members who are responsible to a certain amount for the necessary expenses of the Association, and who have a voice in the management of its affairs; (2) Subscribers who incur no liability, and who will take no part in the management of the Association.

The Association not being formed for executing works, leaves the members and subscribers to employ any person they may select to carry out the recommendations of its officers.

Members and subscribers alike contribute an entrance-fee of half a guinea and an annual subscription of half a guinea, or a life-subscription of five guineas, and on payment in accordance with the graduated scale, they are entitled to have one house in London placed on the Assurance Register. The fees for houses outside the metropolitan district will be increased according to distance. The secretary of the Association is Joseph Hadley, Esq., 5, Argyll-place, Regent-street, W.

PLAIN WORDS IN SCIENCE.†

In making use of language to express our thoughts, we ought to be sure—(1) That the words used really express the idea which it is wished to convey; (2) that they are the shortest; and (3) that they are the most familiar words which are available. Words must be as objective as possible, *i.e.*, they should bring the subject with the utmost vividness before the mind's eye; and, therefore, those words to which the eye, and the ear, and the mind had been accustomed for the longest time (vernacular terms used from infancy) were the best; and, other things being equal, the shortest words were the best. If the advantages of expressing themselves simply were so obvious, why, it would be asked, do men continue to use the polysyllabic gibberish which passes current as the language of science, but which proves that they have not yet come to a right comprehension of the scientific use of language? By using a language "not understanded of the people" for the expressing of scientific facts, they undoubtedly seriously curtailed the area from which they drew their scientific recruits; and he took it that one explanation of the scientific fervour which pervaded the whole of Germany was to be found in the fact that scientific terms were in that country very largely derived from the German vernacular, and that he who only knows the German language was not necessarily confronted in a German scientific book with words which compelled him to close the volume almost as soon as opened with a sigh of helplessness and hopelessness. It must be admitted that our long words had not hitherto been of much use as a means of international communication. For international communication they must make themselves familiar with each other's languages. That was certain. And it was manifestly of importance that each nation should try to keep its language pure, in order that it might be the more easily learned. The practice of concubinage with the dead languages merely had the effect of producing a mongrel language (as unproductive as are all other mules), of huge bulk and monstrous form, which has to be learnt as an additional study. It seems to be the pitiable ambition of some writers to seize upon a trifling fact, and to give it the longest name they can invent with the aid of a lexicon. Some, possibly, are under the impression that their dictionary-made expressions may gain for them a reputation for classical learning. They cannot afford, as did John Hunter, to rely for their reputation upon the facts which they discover, who, when he was twitted with his want of knowledge of Greek and Latin, wrote thus characteristically to a friend: "Jesse Foote accuses me of not understanding the dead languages; but I could teach him that on the dead body which he never knew in any language dead or living." The defence has lately been put forward for scientific jargon that every trade or profession must have its own technical terms. He confessed he could not see the necessity. The tailor, as far as he knew, derived no advantage from calling his smoothing-iron a "goose"; and seamanship is not advanced because a sailor's "companion" is one thing at sea and another thing on shore. It seemed to him that technical terms ought, as far as possible, to be discouraged, because the coining of new words when they are not wanted, and the giving of strange and conventional meanings to common words, must increase the difficulty of acquiring any art or handicraft. Among unworthy motives which had induced them to have long words, must be reckoned the desire to appear more learned than they were. There was in human nature a tendency which was expressed by the words, *Omne ignotum pro magnifico*—a tendency to put an undue value upon the unknown. It was this natural tendency which led the hero of Warren's famous novel, "Ten Thousand a Year," to make the fatal experiment of applying to his hair the pomade called "Cyanochaitanthropopoion," and it was the same tendency which led the public to buy anything, no matter how common or how worthless, to which the vendor had given a name which was utterly incomprehensible to them. By pandering to this tendency he doubted not that medical terms had been in reality an unspeakable, though delusive, comfort to the public; and that the lady who was told by the physician "that there was still in her husband's lung a perceptible amount of 'whispering pectoriloquy,' although the 'ægophony' had happily completely disappeared," derived from the information the same kind of consolation as did the old woman who, listening to a deep and learned sermon by her rector, found solace in "that blessed word Mesopotamia."—*Times.*

* "Sanitary Assurance," a Lecture by Prof. de Chaumont. (J. & A. Churchill, London.)

† From an Address by Dr. George Vivian Poore, F.R.C.P., Prof. of Medical Jurisprudence, University College.

TRUSTING TO LUCK.

It is worthy of notice how little those who trust most to chance understand of the laws of chance. This is shown in hundreds of different ways, but by none, perhaps, more than by the strange selections made by the venturesome among the various methods in which they may risk their money: their preference for this or that form of risk, rather than for some other, is scarcely ever based on any real advantage which one form has over the other. Ask a gambler, for instance, to pay £1 for a ticket in a lottery where there are a thousand equal chances and but one prize of £1,000, and he

will gladly pay the money. He would most probably not refuse even if there were two thousand equal chances, and the prize were still but £1,000, though the real value of the ticket would be but 10s. If, however, you asked him to pay £1 for the chance of getting £1,000 if a tossed coin comes up head eight times running, he will reject (probably with ridicule) the idea of accepting it. Yet in reality the offer is a far better one than the other. He ought to pay very nearly £2 for the chance offered him for £1 (the exact value of the chance is £1. 19s. 0¼d.). Or the "eight times running" might be changed to "nine times running" if the £1,000 prize were increased to £1,024, and the second offer would then be as fair as the other. But the same gambler who thinks he is quite likely, owing to his luck, to draw the right ticket out of a thousand, would utterly despair of tossing head nine times running.

"Increased knowledge confers an increased feeling of duty, and increased power to perform it."—*Maudsley.*

The Cedars of Lebanon.—The Vienna *Politische Correspondenz* says:—The once famous cedar forest of Lebanon, formerly so extensive, has dwindled down to the dimensions of a mere thicket, numbering about 400 trees. To save it from complete destruction and preserve it at least in its present extent, Rustem Pasha, the Governor-General of the Lebanon, has issued a special ordinance, containing a series of stringent regulations calculated to check, if not quite put a stop to, the vandalism and carelessness of most travellers. It is expressly forbidden to put up tents or other kinds of shelter within the district of the trees, or to light fires or to cook any provisions in their vicinity. No one is allowed to break off a bough or even a twig from the trees. It is forbidden to bring any beasts of burden, be they horses, mules, asses, or any other kind of animal, within the district. Should oxen, sheep, goat, or other pasturage cattle be found within the prescribed limits, they will be irredeemably confiscated.

Professional Astronomers and Popular Astronomy.—We are inclined to doubt whether the official chiefs of great observatories are, as a rule, the persons best fitted to write treatises on popular astronomy. It is not that they are unwilling to deal with astronomy in a popular manner. On the contrary, they are apt to adopt too familiar and condescending a tone, as if writing for children. But in reality, their astronomical labours, whether in the observatory or in the calculating-room, are not adapted to give them that knowledge of the general science of astronomy, without which no man can present astronomical truths at once simply and effectively. Their work bears the same relation to the real living astronomy of men like the Herschels that land-surveying bears to geology as dealt with by a Hutton, a Playfair, or a Lyell. They find more interest, as a rule, in the correction of a star's place by the tenth of a second of arc than in the inquiry into the star's attributes as a sun. A perturbation affecting the moon's position by the hundredth part of her diameter is more important to them than telescopic evidence of the most tremendous changes in the moon's surface would be. In fine, their way of viewing the heavenly bodies somewhat resembles the way in which a certain Senior Wrangler is said to have viewed Snowdon, who, when asked if he had climbed that mountain, replied that he had not, because a neighbouring hill was equally suitable for trigonometrical purposes. The astronomy thus taught differs as widely from the astronomy of the Herschels as a series of anatomical plates differs from the Venus of Milo or the Theseus and Ilyssus of the Parthenon.

The Induction Balance applied to Surgery.—It was stated in the *Times* of Aug. 5 that the place of the bullet in President Garfield's body had been localised by the use of the induction balance. The *post-mortem* examination seems to have shown that the diagnosis was incorrect. The *Times* Correspondent at the Paris Electrical Exhibition makes the following remarks on the subject, which are interesting as including a concise and sufficiently simple account of the method in question. "I am convinced," he says, "that if the experiments were conscientiously carried out there could be no error. A remarkable confirmation of the utility of the apparatus as a means of diagnosis has just come to my notice, but I will first give a short account of the action of the instrument. A current of electricity is passed through two coils of insulated wire, which are kept apart. The current is rapidly made and broken by clockwork. Above each coil is a second similar coil of insulated wire, and every time that a current is made or broken in the first pair of coils a second current is induced in the secondary coils. These secondary coils are connected together and to a telephone, and the currents induced in either coil tend to produce a noise in the telephone. But these coils are so connected as to induce currents in opposite directions in the telephone, so that under ordinary circumstances they destroy each other, and no noise is heard in the telephone. If, however, a piece of metal be placed inside one of the coils, the character of the induction is changed in that coil, and the balance is destroyed, so that a noise is heard in the telephone. The amount of noise depends upon the form, mass, and conductivity of the metal placed inside. By choosing pieces of metal all of the same size, but differing in their composition, it is possible to compare the conducting powers of the metals. A long scale of metal, wedge-shaped, is moved along above the coil which does not contain the metal to be tested, until the thickness of the wedge over one coil is sufficient to balance the metal inside the other coil. The reading of the scale on the wedge then gives a measure of the conductivity. Some most astonishing results have been thus obtained. It is found that pure copper electrically deposited has a conducting power far greater than the copper of commerce, the difference being far greater than was generally supposed. I will now describe the experiment which has just been completed. Mr. Elisha Gray, of America, whose name is so well known in connection with the telephone and the harmonic telegraph, was a disbeliever in the utility of the induction balance as a surgical appliance. He said to Professor Hughes, 'Thirty years ago, when working at some metal-work, a filing of iron entered my finger; the more I tried to extract it the deeper it went in. I believe it is still there, and if your instrument is of any value, you ought to be able to tell me in which finger it is.' The presence of bone or flesh in the coil of the balance would produce no effect; a metal or other conductor is necessary. Professor Hughes tested Mr. Gray's fingers; none of them gave any sound until he came to the forefinger of the right hand, when the balance of the coils was quite destroyed, and a noise was given out. This was the very finger in which the filing was buried thirty years ago. I need hardly say that Mr. Gray was completely convinced."

Weather Forecasts.—That the daily forecasts issued from the Meteorological Office are often wrong I need not say. Why they are so is simply because general disturbances in the atmosphere only are taken into consideration, local disturbances being ignored. And so long as the forecasts are based solely upon the distribution of barometrical pressure this cannot be otherwise. When there is no distinctly-developed area of high or low pressure lying over the country or approaching our shores, the forecasts must be made at random, for the observer has nothing whatever to guide him in his prediction. He may say "wind light and variable," or "calm," or "weather unsettled"; but such forecasts are liable to falsification in many districts through the operation of local influences. A little hurricane may start up in one spot, rain may pour in another, and a thunderstorm may burst over a third, all unexpectedly, and the observers in the Meteorological Office could not possibly have been expected to warn the afflicted districts. I think, then, the importance of local meteorology cannot be exaggerated. The distribution of rainfall over the British Isles sufficiently shows the potency of physical conditions, but a more striking example is found in the fact that within a radius of twelve miles around Somersham Railway Station (Huntingdonshire) hailstorms are so frequent and destructive that all the insurance companies charge double the ordinary rates per acre on crops growing within that district. Violent winds, too, are often due to local causes. An instance of this came under my notice some years ago in the Isle of Man, when a violent gale blew over Ramsey from the westward, the air a couple of miles to the north being almost still all the time. The storm rushed along the base of North Barrule, lashing the bay into foam, and apparently becoming dissipated out at sea. The afternoon on which this occurred was bright and almost cloudless, and the storm maintained its fury for several hours. Such local disturbances may be quite as destructive as more general ones, and forecasts, to be of any practical use, ought certainly to take cognisance of the causes which produce them. To render forecasts as reliable as our present knowledge can make them, I would suggest that a local observer be appointed in each district to act in concert with the Meteorological Office, and to base the forecasts upon the information and charts supplied by that office, supplemented by his own knowledge of local conditions and influences. Every district has its system of natural weather signs dependent upon its physical features, and experienced farmers, and others who have given attention to them, can predict the weather with far greater certainty than the functionaries in London. There was a time when natural signs were our only aid to a foreknowledge of the weather, but since the invention of telegraphy and the consequent discovery of cyclonic movements in the atmosphere, the cat has been discarded and the barometer put in her place. Why not use both? Theoretical meteorology, so to call it, determines the general distribution of wind and weather over the country; local meteorology, the particular kind of weather in a district. The one is the complement of the other; and in order to obtain the best result they should be used accordingly, the one supplying what the other lacks.—*J. A. Westwood Oliver in the "Times."*

Our Chess Column.

WE propose to consider the chess openings in the following general order:—

First we shall examine the best methods of attack and defence following from
1. $\frac{\text{P. to K.4.}}{\text{P. to K.4.}}$ 2. $\frac{\text{Kt. to K.B.3.}}{\quad}$ or the *King's Knight's Opening.*

Next we shall examine those following from
1. $\frac{\text{P. to K.4.}}{\text{P. to K.4.}}$ 2. $\frac{\text{B. to Q B.4.}}{\quad}$ or the *King's Bishop's Opening.*

We shall then take the King's Gambit, examining first the various forms of the opening which follow from
1. $\frac{\text{P. to K.4}}{\text{P. to K.4}}$ 2. $\frac{\text{P. to K.B.4}}{\text{P. takes P.}}$ 3. $\frac{\text{Kt. to K.B.3}}{\quad}$
the King's Knight's Gambit. Then those following when White plays $\frac{\text{3. B. to Q.B.4}}{\quad}$ the *King's Bishop's Gambit.*

We shall then consider other openings following from 1. $\frac{\text{P. to K.4}}{\text{P. to K.4}}$ as 2. $\frac{\text{Kt.Q.B.3}}{\quad}$ the Vienna opening; 2. $\frac{\text{P.Q.B.3}}{\quad}$ the *Q's Bishop's Pawn Opening;* 2. $\frac{\text{P. to Q.4}}{\quad}$ the *Centre Gambit*, and so forth.

Next we shall examine the chief openings resulting from
1. $\frac{\text{P. to Q.4}}{\text{P. to Q.4}}$ 2. $\frac{\text{P. to Q.B.4}}{\quad}$ the *Queen's Gambit.*

And lastly, we shall examine the various openings called *Irregular*, which include all openings beginning otherwise than 1. $\frac{\text{P. to K.4}}{\text{P. to Q.4}}$ or 1. $\frac{\text{P. to Q.4}}{\text{P. to Q.4}}$ We may note in passing that some of these so-called irregular openings deserve in reality to be regarded as more regular, because steadier and surer, than many of the openings classed as regular.

We do not propose to give a deep analysis of any of these openings, because our wish is to be of use to the many, not to cater for those who are already first-class players. We wish, in fact, to give just so much of what is called book-knowledge of chess as may enable amateur chess-players to start each game on sound chess principles. We wish also to show how, when they meet opponents of superior book-knowledge, they may avoid those openings in which book-knowledge alone can save them from disastrous defeat.

We shall occasionally deviate from this course to discuss the various points illustrating general principles of play, as such points may be suggested by communications received from our correspondents.

King's Knight's Opening. (Two Knights' Defence.)

There are only three sound replies to the move 2. $\frac{\text{Kt. to K.B.3}}{\quad}$; viz., first, 2. $\frac{\quad}{\text{Q.Kt. to Q.B.3.}}$; secondly, 2. $\frac{\quad}{\text{P. to Q.3}}$; and thirdly, 2. $\frac{\quad}{\text{Kt. to K.B.3.}}$ of which the third is rather a counter-attack than a defence. There is a fourth way of meeting the move, by 2. $\frac{\quad}{\text{P. to K.B.4.}}$ (called Greco's counter-gambit), which, though not strictly speaking sound, is often used with great effect by an experienced player against one unfamiliar with the opening. We shall have therefore to consider it, though briefly, further on.

We take first the line of play which we should advise the learner nearly always to follow till he has become tolerably familiar with its various sequels, viz.:—
1. $\frac{\text{P. to K.4}}{\text{P. to K.4}}$ 2. $\frac{\text{Kt. to K.B.3}}{\text{Kt. to Q B.3}}$
The continuation is now either 3. $\frac{\text{B. to Q.B.4.}}{\quad}$ leading to a number of the most interesting openings known in chess or 3. $\frac{\text{P. to Q 4.}}{\quad}$ the Scotch Gambit, or 3. $\frac{\text{B. to Q.Kt 5.}}{\quad}$ the strong Ruy Lopez attack, or 3. $\frac{\text{P. to Q.B.3.}}{\quad}$ a quiet continuation. If the move 3. $\frac{\text{B. to Q.B.4.}}{\quad}$ is met by the same move on Black's side (Black is for convenience regarded always, in these analyses as second player), we have an opening which may be resolved into the *Giuoco Piano*, or Steady Game, by 4. $\frac{\text{P. to Q.B.3.}}{\quad}$ or 4. P. to Q.3, or into the brilliant Evans Gambit by 4. $\frac{\text{P. to Q Kt 4.}}{\quad}$

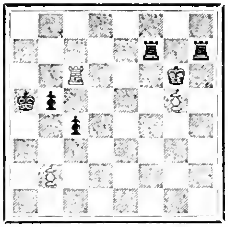
Against a player well acquainted with the openings, the safest reply to 3. $\frac{\text{B. to Q.B 4}}{\quad}$ is, perhaps, 3. $\frac{\quad}{\text{Kt. to K.B.3}}$, which constitutes the Two Knights' Defence; for this move saves the second player from the Evans Gambit, to meet which requires thorough knowledge of a great number of varied lines of play, while declining the proffered Queen's Knight's pawn subjects the second player to a cramped defensive game. We do not ourselves advise 3. $\frac{\quad}{\text{Kt. to K.B. 3}}$ for general adoption, for the games to which it leads are seldom so interesting as those following from 4. $\frac{\quad}{\text{B. to Q.B.4.}}$ But, properly played, it is a safe defence. And as the variations following from it are not nearly so numerous as those from 4. $\frac{\quad}{\text{B. to Q.B.4,}}$ it will be well to dispose of it before entering on the latter.

We shall begin the inquiry into the Two Knights' Defence next week.

In the meantime we give for consideration two positions.

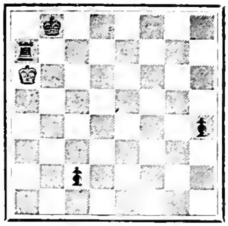
No. 1, which occurred a few nights ago in a game in which we had given the odds of a Queen to a lady.

No. 1. Actual End-Game.
Black. (Lady).



White. (Chess Editor.)
White to play and draw.

No. 2. By Mr. F. Healey.
Black.



White.
White to play and draw.

Black's last move was R. to K.R. 2, imprisoning the King, while leaving the two Black Rooks free to move *ad lib.* along Black's second row. This move, however, enabled White to draw the game. The young player will readily see how.

This, of course, as occurring in ordinary play is not to be regarded as a Chess study. It simply illustrates one of the dangers which a weak player, who, after a game at odds, finds himself with a winning superiority of force, should carefully avoid. Position No. 2, by the celebrated problem-maker, Healey (which appeared in the *Westminster Papers*, as nearly as we can recollect, in 1871) illustrates the same danger very prettily.

[1.] Chess Query.—In an old number of the *Illustrated London News* I find the following problem given by the Chess Editor (then Mr. Staunton, I believe), as a puzzle, by the Anonymous Modenese. White K. at Q.Kt sq.; Q. at K.B. 4; R. at K.Kt.5; B. at Q Kt 2; Ps. at K.B 5, Q Kt.3, and Q.R 2. Black K. at K.R sq., Q. at Q.4; R. at Q.sq.; Kt. at Q.R.5; Ps. at K.R.2 and K.Kt.2. White to play and Mate in four moves. Can any one give me the correct position? There seems to me to be no solution with the position as given. Queen's Knight.

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Our Whist Column.

By "Five of Clubs."

Introduction.

Whist, properly played, is the finest of all card games, perhaps not even excepting chess—the finest of all sedentary games. But Whist, as it is often played, without any knowledge or appreciation of the real nature of the game, seems to have nothing making it better worth playing than Pope Joan or Casino, and to be decidedly inferior to Euchre. There is an intermediate kind of whist, the game played by persons who have a keen perception of the strategy of the game, but no knowledge of its language, which may be full of interest or full of annoyance, as the cards may happen to lie. To watch a proficient in this kind of whist, playing a good hand, and ably supported by a steady-going partner who understands his ways, one would say Whist was the most delightful of all games; but to see him playing an average hand, and to note his wrath when his partner, considering his own hand, fails to play precisely as he wishes, one would say Whist was a rather severe form of punishment.

The present series of papers on Whist, and the problems, games, &c., which will accompany and follow it, are intended to indicate the nature of the only game of Whist which is worth playing—Whist as a game between two forces, two pairs of partners, each pair having between them twenty-six cards. The game thus played, that is with constant reference by each player to the fact that he has a partner, may be regarded as a really scientific game. It is often called the book game, theoretical play, and by other names, implying that a fine player need care very little about it. But it is, in truth, the only common-sense, practically-sound form of the game, and no one can be regarded as a really good, still less as a fine, player who does not play it. It has, moreover, the additional advantage of being readily learned by those who have not the capacity for really great play; and when it has been learned, such players, though never brilliant, become good and safe partners. Moreover, by learning the rules of scientific Whist, which seem at first an extra trouble to the memory, the learner finds that his power of remembering the fall of the cards is greatly increased. It is, indeed, the purposeless nature of ordinary unscientific Whist play which makes it so difficult for the bad player to remember what cards have been played, and by whom. So soon as he has adopted just principles of play, each hand is played according to a plan, the development of which is full of interest, so that the stages are easily remembered. Each card is played with a purpose, and whether the purpose succeeds or fails, the result is noted and remembered, whereas when there is no purpose, the memory has no such aid.

The first great principle of the scientific game of Whist is to give your partner (always at the beginning, and almost always throughout the play of the hand), all the information in your power within the rules of the game. (There arise cases occasionally towards the end of a hand where it becomes clear that the partner can do nothing, and nothing can be lost by misleading him; then, and then only, false cards, deceiving him, but deceiving the adversaries also, may be usefully played.) To this the objection is repeatedly made—especially by brilliant one-handed players—"a player has but one partner while he has two adversaries, and by playing so as to give information to one friend, he gives information to two enemies, or the harm exceeds the good two-fold." The true answer to this objection does not seem to me to have been recognised by Pole, Cavendish, Clay, and other great masters of the game, who have yet, of course, known perfectly well from practice that it is advantageous to give to your partner all the information in your power. Cavendish says the objection would have considerable force if you were compelled to expose the whole of your hand, but you possess the power of selecting what facts shall be announced and what concealed. Pole says the objection "involves a confusion in reasoning: for if the opponents are equally good players, they will adopt the same system, and the positions must be equal; and if they are not good players, they will be incapable of profiting by the indications you give, and the whole advantage will rest with you: adding that "even good players seldom pay so much heed to their opponents' as to their partners' indications." Pole and Drayson agree in saying that by not giving your partner information, you run the risk of having to fight three opponents single-handed. Clay does not specifically consider the objection.

The true answer seems to me to be different from any of these. The reply of "Cavendish" implies that there is a limit to the principle that it is more important to inform your partner than to deceive your adversaries. Pole's reply takes the system for granted, by assuming that good opponents will follow it; and certainly he does not reason soundly in suggesting that even good opponents pay less attention to their opponents' than to their partner's indications. It is also an exaggeration to speak of a partner as becoming a third opponent if not duly informed as to your cards; he may take one or two of your winning cards, but cannot play as an opponent throughout the hand, as they seem to imply. (The mischief is bad enough, without exaggeration.) The real reason why information to your partner is so important as to outweigh the knowledge given to the adversary, is that it is only by giving him information that your cards can be combined with his in the strategy of the hand. You tell him points about your hand which he can utilise, let the opponents do what they will, although, of course, you may also give him information which he cannot utilise, whether because the adversaries have also learned it, or not. Cases of the latter kind count neither one way nor the other; if you had not suggested such and such a plan, he would not have tried it, and when you have told him he has not succeeded, so that you are none the worse; all the cases of the former kind are so much clear gain.

Take a familiar instance. I lead ace, and follow with queen of my best suit. My partner *knows* that I have the knave and a small card left. Suppose he has the king in his own hand and a small one left after the first round. Now according to the state of the score and of his own hand, it may be better to let the trick fall to my queen, or to take it with his king, leaving me still the command of the suit with my knave. By my play, showing that I have the knave, I have left it open to him to do whichsoever of these two things may be best for both of us, and this choice he has, let the opponents act as they please. But suppose that, instead of following the recognised line of play for such cards, I lead the second round with my small card. My partner plays his king, and, let us suppose, wins the trick. He cannot now play as he would (as it might be absolutely essential to success that he should) if he knew that I had the command of the suit. On the contrary, so far as he can understand me at all, he thinks I have three small cards of the suit left, and that the queen lies with one of the adversaries. His consequent play in this case spoils our common game, whereas in the other case his play advances our common game. In either case it is *his* play, not the opponents', which affects our combined game for good or for ill.

In fine, instead of the maxim, "It is more useful to inform your partner than to deceive your adversary," I would substitute this—"Your single partner can do more good than both your adversaries can do harm by utilising information you may give by your play." (Good here includes the avoidance of harm: we might supplement the rule by saying that your partner is likely to do much more mischief through ignorance of your hand, than could be counterpoised by any good which the adversaries might chance to do you.)

It is the recognition by good players of this first rule, as resulting from the general principle that partners should play in harmony and with a common purpose, which has led to the system of modern whist strategy. There are commonly more ways than one in which, if the partner's cards were seen, the qualities of the combined hands might be used; but there is only one system by which, in the actual method of play, your partner can work in harmony with you. That system being adopted, the principles guiding us in the opening of a hand, and determining the play of first, second, third, and fourth player, are deduced at once. Our books of whist seem, indeed (and it has always seemed to me a fault in them), to require that the learner should know multitudinous rules for leading, and for playing second, third, and fourth; but in reality all these rules depend on one general principle. I do not say that the player ought at once to know, from his knowledge of this principle, his proper course as leader, second, third, or fourth player. He has not time to go through all the considerations involved in applying it to particular cases. He must be content, therefore, to retain a number of rules for such cases in his memory. But his memory will be greatly helped, and the number of rules will be greatly diminished, when he recognises the general principle on which modern whist-play proceeds.

In my next I shall show what this general principle is, why it has been selected in preference to others, which, at first view, seem to have great, if not equal advantages. Afterwards we shall consider how this principle suggests the various leads, the play of second, third, and fourth hand, &c., endeavouring so to treat the matter that the memory may be as much as possible helped to retain the resulting rules, by recognising the string on which these seemingly scattered beads of Whist wisdom are in reality strung.

I may in the next number present a game actually played, mentioning the inferences which a player of the systematic game would make at once. Those who follow our explanation of the system will presently see that these inferences are not, as they might suppose, recondite, but perfectly obvious, even after a very moderate study of the modern system of Whist play.

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CONTENTS.

SOLIDS, LIQUIDS, AND GASES.

By W. Mattieu Williams.

PART I.

THE growth of accurate knowledge is continually narrowing, and often obliterating the broad lines of distinction that have been drawn between different classes of things. I well remember when our best naturalists regarded their "species" of plants and animals as fundamental and inviolable institutions, separated by well-defined boundaries that could not be crossed. Darwin has upset all this, and now we cannot even draw a clear, sharp line between the animal and vegetable kingdoms. The chemist is even crossing the boundary between these and the mineral kingdom, by refuting the once positive dictum that organic substances (*i.e.*, the compounds ordinarily formed in the course of vegetable or animal growth) could not be produced directly from dead matter by any chemical device. Many of such organic compounds have now been made in the laboratory from mineral materials.

We all know broadly what are the differences between solids, liquids, and gases, and, until lately, they have been very positively described as the three distinct states or modes of existence of matter. Mr. Crookes suggests a fourth. I will not discuss this at present, but merely consider the three old-established claimants to distinctive existence.

A solid is usually defined as a body made up of particles which hold together rigidly or immovably, in contradistinction to a fluid, of which the particles move freely over each other. "Fluid" is the general term including both gases and liquids, both being alike as regards the mobility of their particles. At present, let us confine our attention to liquids and solids.

The theoretical or perfect fluid which is imagined by the mathematician as the basis of certain abstract reasonings has no actual existence. He assumes (and the assumption is legitimate and desirable, provided its imaginary character is always remembered) that the supposed particles move upon each other with perfect freedom, without any friction or other impediment; but, as a matter of fact, all liquids exert some amount of resistance to their own flowing; they are more or less *viscous*, have more or less of that sluggishness in their obedience to the law of finding their own level which we see so plainly displayed by treacle or castor oil.

This viscosity, added to the friction of the liquid against the solid on which it rests, or in which it is enclosed, may become, even in the case of water, a formidable obstacle to its flow. Thus, if we make a hole in the side of a tank at a depth of 16 feet below the surface, the water will spout from that hole at the rate of 32 feet per second, but if we connect with this hole a long horizontal pipe of the same internal diameter as the hole, and then observe the flow from the outlet of the pipe, we shall find its velocity measurably diminished, and we shall be greatly deceived if we make arrangements for carrying swift-flowing water thus to any great distances.

Three or four years ago an attempt was made to supersede the water-carts of London by laying down on each side of the road a horizontal pipe, perforated with a row of holes opening towards the horse-way. The water was to be turned on, and from these holes it was to jet out to the middle of the road from each side, and thus water it all. I watched the experiment made near the Bank of England. Instead of spouting across the road from all these holes, as it would have done from any *one* of them, it merely dribbled, the reason being that in order to supply them all, the water must run through the whole of the long pipe with considerable velocity, and the viscosity and friction to be overcome in doing this nearly exhausted the whole force of water-head pressure. Many other similar blunders have been made by those who have sought to convey water power to a distance by means of pipes of such diameter as should demand a rapid flow through a long pipe.

The resistance which water offers to the stroke of the swimmer or the pull of the rower is partly due to its viscosity, and partly to the uplifting or displacement of some of the water. If it were perfectly fluid, our movements within it, and those of fishes, &c., would be curiously different, and, in fact, the whole face of this globe would be strangely altered in many respects. I will not now follow up this idea, but leave it as a suggestion for the reader to work out for himself, by considering what would remain undone upon the earth if water flowed perfectly, without any internal resistance.

The degrees of approach to perfect fluidity vary greatly with different liquids.

Is there any such a thing as an absolute solid, or a body that has no degree of fluidity, the particles or parts of which will admit of no change of their relative positions, no movement upon each other without fracture of the mass? This would constitute perfect *rigidity*, or the opposite to *fluidity*.

Take a piece of copper or soft iron-wire, about one-eighth of an inch in diameter, or thereabouts, and bend it backwards and forwards a few times as rapidly as possible, but without breaking it; then, without loss of time, feel the portion that has been bent. It is hot—painfully so—if the experiment is smartly made. How may this be explained?

It is evident that in the act of bending there must have been a displacement of the relative positions of the particles of the metal, and the force demanded for the bending indicated their resistance to this movement upon each other; or, in other words, that there was friction between them, or something equivalent to such internal friction, and thus the mechanical force exerted in the bending was converted into heat-force.

Here, then, was fluidity, according to [illegible] tion: not perfect fluidity, but [illegible] the above defini- [illegible] fluidity attended with

resistance to flow, or what we have agreed to call viscosity. But water also offers such resistance to flow or viscosity, therefore the difference between iron or copper-wire and liquid water as regards their fluidity is only a difference of degree, and not of kind: the demarcation between solids and liquids is not a broad, clearly defined line, but a band of blending shade, the depths of tint representing varying degrees of viscosity.

THE PHILOSOPHY OF ANIMAL COLOURS.

By Dr. Andrew Wilson, F.R.S.E.

PART II.

But, descending to still lower grades of life, we may discover examples of this "mimicry," not only of surroundings, but also of lifeless or inorganic objects, and of, it may be, plant structures as well, on the part of animals. The so-called "stick insects," or "walking twigs," as they are often called—the *Phasmidæ* of the naturalist—present us with the most perfect reproductions of bits of dried twigs. A figure of one of these insects is before me as I write. It is represented climbing on the delicate branch of a shrub, and but for the expectation of what one is looking for, there would be considerable difficulty in determining which is insect and which plant. The bodies of these "twig insects"—which belong, by the way, to the *Orthoptera*, or that order which harbours the familiar crickets and grasshoppers—are represented by mere lines. The wings have disappeared, and it has been remarked that in their gait these insects exhibit a peculiar habit of using their legs in a singularly awkward fashion, and thus apparently aid the illusion of the spectator that he is regarding a dried twig, moved erratically by the wind.

More extraordinary still are the "Leaf insects;" near allies, indeed, of the walking-sticks. Here "mimicry" of the plant proceeds so far as to fully justify the eminent naturalist's remarks, that it is strange to find the animal assuming a mimetic disguise and apeing the actor's art. The wings in the "leaf insects" exactly imitate leaves. The venation, or arrangement of the veins in the leaf, is clearly seen, and in one form (*Phyllium*) even the chest and legs of the animal assume leaf-like characters. When such an insect rests amid foliage, the value of such a close resemblance to its plant surroundings as a means of protection can be readily understood. In some "leaf insects"—all of which are tropical species—the wings resemble leaves that are dried and withered. In others, the minute fungi that attack leaves are imitated. Mr. A. R. Wallace tells us that one of the "walking-sticks" obtained by him in Borneo, "was covered over with foliaceous excrescences of a clear olive green colour, so as exactly to resemble a stick grown over by a creeping moss or Jungermannia. The Dyak who brought it me assured me it was grown over with moss, though alive, and it was only after a most minute examination that I could convince myself it was not so."

Lastly, there may be noticed in connection with these curious traits of animal life, the fact that certain animals, themselves harmless and inoffensive, may assume the exact appearance of offensive neighbours. In this respect, certain butterflies are *facile principe*. Certain South American butterflies, known collectively under their family name of *Heliconidæ*, exhibit a brilliant colouration, but like[illegible]ise possess a very strong odour; and, it may be presumed from [illegible] sequel, a highly disagreeable taste as well. They are highly [illegible]onspicuous insects, and the under sides of their wings are as brilliantly coloured as the upper surfaces; so that, even in repose, and when resting with the wings apposed over the back, they are readily enough seen. Their colours are prominent, not to say gaudy. Yellows, reds, and whites, commingle with blacks, blues, and other tints in a striking fashion. They are, further, by no means rapid flyers, and, putting the foregoing circumstances of their gaudy colour and their slow movements together, no group of animals would seem more liable to the attacks of bird enemies than these Helicon butterflies. Yet the reverse is the case. So far from being decimated, their race flourishes apace, and this result is clearly due to the strong odour and nauseous taste they possess. The mere touch of a Helicon is in itself a pungent matter, which reminds one of nothing so much as the persistence of the musk-rat's secretion, or the still more awful effluvium of the American skunk. Their neighbour butterflies may fall victims by the score to the rapacity of their feathered enemies, but the Helicons are spared from even the semblance of attack.

So far there seems nothing unusual or striking in a group of butterfles being protected, through strong odour and worse taste, from their natural enemies, the birds. But now comes the most curious phase of this history. Another and distinct family of butterflies, known as the *Leptalidæ*, allied to the common white cabbage butterfly, and removed from the Helicons, also possesses representatives in South America. There are no points of agreement between the Leptalides and the Helicons, save, indeed, that both are butterflies. Furthermore, the Leptalides are entirely destitute of the nauseous odour and of the strong taste of the Helicons, and in respect of their more agreeable presence, should become a prominent article—as do other butterflies—in the bill of fare of the birds. Yet, strangely enough, the Leptalides escape persecution; and the reason is not far to seek or difficult to find. When they are carefully examined, certain species of the Leptalides are seen to be exact *facsimiles*, in colour and appearance, of the stinking Helicons! Naturalists at first classed both as Helicons, until a closer examination showed the difference between these butterflies, and likewise proved that the Leptalides had thus "mimicked" in the plainest possible manner the colours of their strong-smelling neighbours. Nor are the colours alone imitated. The very shape of the Helicon's wings is reproduced in those of the Leptalides, and the "feelers" likewise mimic those of the former group. Again, special forms of Leptalides "mimic" special forms of Helicons. The flight has become of similar character in both species, and the habits have been also slavishly copied.

Such instances as these certainly present "food for thought" to the reflective mind. It is the business of philosophy to account for facts by placing the facts in scientific juxtaposition—philosophy, in this light, is the thread upon which the pearls of knowledge are strung. What, then, it may be asked, is the philosophy which can explain the curious resemblances seen in the animal world, ranging from, say, a mere likeness in tint to the surroundings (as in the flounder or woodcock), through more intensified likenesses, to the exact "mimicry" and to the slavish copy of colour and form, as in the butterflies?

A first and highly important feature in the consideration of the case is found in the fact that there is a gradation in the degree of "mimicry." From the mere sand or ground tinting of the flounder to the exact colouring of the butterflies is, of course, a wide step, but it is one which is bridged over by intermediate examples and stages. Then, secondly, we discover a purpose or use in the disguises: that purpose, apart from any considerations of its origin, being the protection of the animal from its enemies, and the con-

sequent good and increase of its race. Thirdly, it appears possible to account for these curious transformations and disguises, by finding an *initial* step. It is the old story of *le premier pas qui coûte*, applied to natural history research: and this first step is found in the solid axiom, that every living species is liable to *variation* and change. Next succeeds the consideration that such varieties as are produced have to "struggle for existence." Suppose a number of white varieties produced in a cold, snowy region, along with varieties of more conspicuous colours. It is evident that, whilst the white varieties would escape from their enemies, the darker-coloured individuals would succumb. Thus the white race comes to the front, and holds its own, and its perpetuation and increase becomes a matter of surety. Summing up the argument, we find that two factors are at work in bringing about these wonderful colour likenesses in the animal world. The one is variation, producing the colour-varieties: the other is the circumstances of life, which weed out the weak and give the battle to the strong, which latter are those whose colours best suit their surroundings. This is the philosophy which natural history to-day lays down for our acceptance. Nay more, it is a philosophy which explains far more important facts of life than mere mimicry. It is "evolution and development" reduced to their plainest and fundamental terms—in a word, Darwinism in a nutshell, as illustrated by the variation and change that all life knows, and by the warring of that life bringing the best of its units to the front of the battle.

BRAIN TROUBLES.

Impaired Memory.

The first mind trouble we propose to consider is the apparent temporary impairment of the memory. The gradual progressive decay of the power of memory with advancing years is, of course, a change which all may expect who attain great length of years; though, as Cicero long since pointed out, and as has been repeatedly exemplified by modern instances, the change is to some degree under control, and those matters in which an aged person takes special interest may be well remembered, when others about which he cares little are easily forgotten. "I never heard," says Cicero, "of any old man that had forgotten where he had hid his treasure; things which they regard, old men remember—the securities they have out, and who are indebted to them, as well as to whom they are indebted." And so forth. The points to be noticed here are, first, that memory is seen to be in large degree a question of attention as well as of retention; and, secondly, that decay of memory implies a change in the mind analogous to that which makes the old incapable of great bodily efforts. So that when the memory of a person who is not old becomes impaired, we may infer that unless there is actual disease, the symptom indicates overwork of the mind, precisely as bodily weariness indicates that the body has been overwrought. We may, perhaps, be led to inquire here whether a distinction should be drawn between loss of memory, as shown by a weakening of the power of committing to mind new matter (of whatever kind) which we may wish to remember, and the passing away from the mind of matter which had been already committed to it, and retained so long and so recently that its being forgotten can be explained only as due to some marked and recent change in the state of the mind. Suppose, for instance, that after carefully noting a number of facts, which under ordinary conditions we should remember thenceforward for weeks, we find that they have left no sufficient impression on the mind: here we obviously have evidence that the power of attention on which, in the first instance, memory depends, is for the time being enfeebled. Can we, however, infer that it is weakened in the same way and in the same degree as we should judge it to be if we found that numbers, dates, names, or words which we had had occasion to refer to daily for years, were suddenly clean forgotten? Making use, as we conveniently may (though we must not place too much stress on the method), of the analogy between bodily and mental relations, we may compare a change of the former kind to a diminution of the power of acquiring some new feat; a change of the latter kind, to the sudden loss of a feat already acquired and long practised. It can hardly be doubted that an athlete who should find himself unable to perform some new gymnastic trick, which he had supposed well within his powers, would not be so much struck by the circumstance, as he would be if he should suddenly find himself unable to achieve a feat in which he had hitherto found no difficulty.

Let us inquire, however, whether known cases of loss of memory of either kind afford any means of answering the question which has thus arisen. Of course, those cases in which the trouble has been only temporary, though far more numerous than those in which loss of memory has been symptomatic of actual disease, stand far less chance of being kept on record, so that we may have to consider cases of the latter kind to discover the relative importance of the two forms in which loss of memory may be noticed. The reader must not judge from cases thus cited that either class of symptoms is necessarily, or even probably, indicative of serious brain mischief.

We will begin, however, with a case in which the trouble was only temporary, and, moreover, its cause obviously indicated.

Sir Henry Holland gives the following suggestive account of a transient loss of memory due to fatigue. It will be seen that the failure of memory belongs to the second class above referred to, that, viz., in which what had been long and well known is suddenly forgotten. "I descended," he says, "on the same day, two very deep mines in the Harz Mountains, remaining some hours underground in each. While in the second mine, and exhausted both from fatigue and inanition, I felt the utter impossibility of talking longer with the German Inspector who accompanied me. Every German word and phrase deserted my recollection, and it was not until I had taken food and wine, and been some time at rest, that I regained them." This case would seem to show that transient loss of memory, even of this kind, need not be regarded as necessarily an alarming symptom.

The following case points in the same direction even more decisively. "A gentleman," says Dr. Winslow, "well known for his intense passion for field sports (living, it may be said, upon the saddle during the greater part of the year), frequently complained of transient attacks of loss of memory after a hard day's run with the hounds. His remedy for this affection was half-a-pint to a pint of port wine *at a draught!* The effect of this heroic dose of vinous stimulant upon the depressed energy of the brain was evidenced by the memory immediately recovering its vigorous activity." It would, however, be unwise to infer that this sportsman, in thus prescribing for these attacks, showed himself the "physician" of the proverb relating to fools and physicians. The remedy was a dangerous one. His was specially a case where prevention was better than cure. The transient attacks of loss of memory showed that the hard day's run with the hounds overtaxed his strength. He would have done wisely to have limited his exertions

in the field (not giving up hunting, but restraining his zeal on those occasions when the day's run promised to be harder than usual).

The effect of wine used regularly, not in pint draughts, is in many cases undoubtedly good where the memory is apt to fail. We have an illustration of this in the following case, belonging to the first of the two classes above considered. A gentleman whose mental and physical powers had been severely taxed, lost all power of recollecting recent events. "Whilst engaged in active conversation, he was able, by a strong effort of the will, to retain possession of the ideas suggested by others to his mind; but if there were the slightest interruption, even to the extent of a minute, in the conversation, he lost all recollection of what he had been previously saying. This gentleman had been living for some weeks below par, with the view of enabling him to perform an amount of urgent mental work, requiring for its execution the lengthened concentration of a clear and vigorous intellect. He had been in the habit of drinking a fair portion of wine, but had unwisely abandoned the use of stimulants, fancying that by so doing he would be better fitted for clear-headed mental occupation." Under Dr. Forbes Winslow's advice, the patient "lived generously, took iron tonics, quinine, and valerianate of zinc, and resumed his daily quantity of wine. This treatment eventually restored his memory to a state of health." Dr. Forbes Winslow adds that he has known other instances of temporary loss of memory cured within a short time by the free use of tonics and stimulants. "In these cases," he says, "the brain is generally in a starved and impoverished condition, arising from a deficient supply of blood; it is in a state of enervation and inanition." On the other hand, the excessive use of stimulants produces unmistakably mischievous effects. Temporary attacks of loss of memory have been caused by intemperance. "By an old Spanish law," Dr. Winslow mentions, "no person was admitted into the witness-box to give evidence in a disputed case who was proved to indulge in habits of intemperance, as an excessive use of stimulants was considered to weaken and destroy the memory."

(*To be continued.*)

INTELLIGENCE IN ANIMALS.

PART II.

The next case cited also relates to the apparent exercise of reasoning faculties by rats, and is interesting, because probably their action was guided by the sense of hearing, rather than by that of smell. "Some years ago," says the narrator, "a plumber told me that he had, on several occasions, been called in to examine into the cause of leakage of water-pipes under the flooring of houses, and had found that the rats had gnawed a hole in the leaden pipe to obtain water, and that great numbers of them had made it a common drinking place, as evidenced by the quantity of dung lying about. The plumber brought me a piece of leaden pipe, about three quarters of an inch in diameter, and one eighth of an inch in thickness, penetrated in two places, taken by himself from a house on Haverstock-hill. There are the marks of the incisors on the lead as clear as an engraving; and a few hairs and two or three of the rat's whiskers have been pinched into the metal in the act of gnawing it. This crucial proof of brute intelligence for a rat will not drink foul water—interested me so much that I ventured to send an account of it to Dr. Charles Darwin, asking his opinion on the means by which the rats ascertained the presence of water in the pipe. To this he replied: 'I cannot doubt about animals reasoning in a practical fashion. The case of the rats is very curious. Do they not hear the water trickling?'" This explanation would go far, it would seem, to do away with the idea that the rats in this case had reasoned, seeing that if they recognised the presence of water by the sense of hearing, their action in biting their way through to what they wanted would correspond precisely with what we have been taught (erroneously, in all probability, but that is a detail) to regard as instinctive. The narrator, however, did not read Dr. Darwin's reply in this sense. "It may be conceded," he says, "that this explanation is the most probable, and if it be the true one, we have an example of an animal using his senses to obtain the data for a process of reasoning leading to conclusions about which he is so certain that he will go to the trouble of cutting through a considerable thickness of lead. Obviously man could do *no more* under the same conditions." If the rats had shown in their boring operations some special aptitude for securing most conveniently, with the least possible overflow, the water they required, this would be a just inference. But as we know no more than that, having found, probably by the sense of hearing, that water was present in the pipe, they bored their way through to reach it, we have in reality no more proof of reasoning power than is afforded by the familiar action of mice in biting their way through the wooden or card casings of boxes of edibles they like, of whose presence within such boxes the sense of smell has convinced them.

This objection is well put by Mr. Henslow in a letter discussing this particular case, and Dr. Darwin's comments thereon, only, as it seems to us, he carries the objection rather farther than it will fairly go, extending it to cases to which we think it can hardly be applied. "It has always seemed to me," he says, "that brute reasoning is always *practical*, but never *abstract*" (but he tries to show that there is very little reasoning at all in the matter). "They do wonderful things, suggested by the objective fact before them; but, I think, never go beyond it. Thus, a dog left in a room alone, rang the bell to fetch the servant. Had not the dog been taught to ring the bell (which, on inquiry, proved to have been the case), it would have been abstract reasoning; but it was only practical. The Arctic fox—too wary to be shot, like the first, who took a bait tied to a string, which was attached to the trigger of a gun—would dive under the snow and so pull the bait down below the line of fire. This is purely practical reasoning: but had the fox pulled the string first out of the line of fire, *in order* to discharge the gun, and *then* to get the bait, that would have been abstract reasoning, which he could not attain to." This, however, is assuming more than can be proved: the fox in the case referred to did not act in the way which would have implied abstract reasoning; we do not know that no fox has ever done so, still less that, failing a simpler way of attaining his object, no fox could so reason. Albeit, we believe there are very few cases in which a line of reason involving so many steps as that suggested by Mr. Henslow has been followed by an animal. Mr. Henslow makes a good point in noting how like the practical reasoning of animals is the reasoning of young folk. "A boy the other day," he says, "found the straps of his skates frozen. The fact only suggested *cutting* them. Not one of his schoolfellows reflected upon the abstract fact that the ice would melt if he sat upon his foot a few minutes. Hence brutes and boys are exactly alike in that nothing occurs to either beyond what the immediate fact before them may suggest. The one kind I call purely *practical* reasoning, which both have; the

other abstract, which brutes *never* acquire; but the boy *will* as his intelligence develops."

Certainly the next case cited in the correspondence suggests practical rather than abstract reasoning. "In Central Park, one very hot day, my attention, writes Mr. James J. Furniss, of New York, "was drawn to the conduct of an elephant which had been placed in an inclosure in the open air. On the ground was a large heap of newly-mown grass, which the sagacious animal was taking up by the trunkfull, and laying carefully upon his sun-heated back. He continued the operation until his back was *completely thatched*, when he remained quiet, apparently enjoying the result of his ingenuity. It seems to me that *instinct* should have prompted the elephant to eat the grass, and that it was reason which caused him to use it for the purpose of diminishing the effect of the sun's rays." Undoubtedly, had hunger been the prevailing sensation at the time, instinct would have caused the elephant to eat the grass. But, as he was probably much troubled by the heat, it was not more wonderful that he should throw grass on to his back, than it would have been if, had there been any shadow, he should have withdrawn under it. Doubtless, however, the true explanation is that the elephant reasoned in a practical way. The effect of the grass as a protection from the heat was obvious to his senses, so he continued to add more and more grass to his covering until he felt comfortable. If the use of the grass for food occurred to him at all, it would have appeared obvious enough that even if all the grass were used for shelter, it would be none the less suitable for food when hunger began to be troublesome.

ARE WOMEN INFERIOR TO MEN?

M. DELAUNAY next proceeds to compare the brain capacity of men and women. Huschke estimates the mean capacity of the cranium for Europeans to be 1,446 cubic centimètres (about $88\frac{1}{4}$ cubic inches) for men, and 1,226 (about $74\frac{4}{5}$ cubic inches) for women, or the masculine brain exceeds the feminine, on the average, nearly 18 per cent. in capacity. However, before we too hastily assume that this implies inferiority, we may as well consider the relative dimensions of men and women in other respects. We have to take into account rather the relative than the actual dimensions of the brain. Now, the average height of men in European countries exceeds that of women by about one-seventeenth part, that is, men are taller than women in about the ratio of 18 to 17. Men, therefore, exceed women in bulk in about the ratio of 18 times 18 times 18 to 17 times 17 times 17 (for the volumes of bodies vary, not as their linear dimensions, but as the cubes of these dimensions), or rather more than the ratio of 118 to 100. Hence, so far as Huschke's estimate can be trusted, the cerebral capacity of women is relatively greater than that of men.

We learn from Broca, though one would like to be assured that the statistical evidence is trustworthy, that the brain of man is heavier than that of women as 111 to 100. This, like Huschke's evidence, but in even greater degree, would be unfavourable to M. Delaunay's position. But Broca also notes that the female cranium is longer and less high than the male. The broader-headed among men will probably regard this as evidence of inferiority, while the dolichocephalic, or long-headed, will regard it as proof positive that at any rate women have equal, or probably superior, brain fitness, so far as shape is concerned.

"The graphic curves of feminine brains of various races," says M. le Bon, "shows that even in the most intelligent societies, as the Parisians of to-day, there is a notable proportion of the female population whose brains approach more to the volume of those of certain gorillas than the least-developed crania of the masculine sex." This sounds unflattering, but in reality it means very little. For the worst developed crania which are here compared include, both among male and female specimens, abnormal cases, from which it is not easy to infer the true relations of brains of that class. Moreover, the men of worst-developed brains are more apt to get eliminated from society, so that the very worst specimens of masculine crania being removed, the comparison between the bad male and the bad female heads is affected unfavourably for the latter.

The frontal lobes, "the seat of the highest intellectual faculties, are less developed in woman than in man, while, on the other hand, the occipital lobes, which especially preside over the life of sentiment, are more voluminous in the woman than in the man." In its totality, according to Professor Wagner, "the brain of the woman is always in a state more or less embryonic." This must, of necessity, relate to averages, not to individual brains. If the brains of those women who have been distinguished for genius were compared with the brains of ordinary men, there would not be found any evidence of a state more or less embryonic. This, of course, would by no means dispose of the argument, but it indicates a circumstance to which in all statistical inquiries attention should be carefully drawn, the effect namely of selection, conscious or unconscious, in affecting the result. Is it certain that the female crania which have been at the disposal of anatomists for examination have come from the same classes (or in the same degree from those classes) as the male crania? Are there not reasons for thinking that, on the whole (and in considering averages this would be a sufficient objection), the women whose crania have been examined belonged to lower classes than the men?

We note these points, not that we would confidently deny the asserted superiority of male crania, on the whole, in capacity and shape. Such superiority may exist, but may indicate no original difference of capacity. Broun Sequard has shown how the brain grows with use; and it is certain that existent systems of education give, on the whole, far less exercise to feminine than to masculine brains. In America, where women are more fairly treated, the customary tests of capacity show by no means that degree of masculine superiority which might be inferred from M. Delaunay's reasoning. On the contrary, the feminine brain holds its own so well against the masculine as to suggest the thought that had equal chances been given for as many centuries as years, the superiority might be quite the other way. Nor has this been the case only during the earlier stages of education; but often up to the final examinations.

In passing, we may note that at present it can hardly be said that the frontal lobes of the brain have been proved to preside definitely over the intellectual, while the occipital lobes preside over the life of sentiment. The animals whose brains have been experimented on do not exhibit so clearly the respective action of intellectual and sentimental ideas, that the different functions of parts of the brain can be thus localised. *Post-mortem* examinations are confessedly unsatisfactory. The relation assumed by M. Delaunay without any doubt or scruple most probably exists, but it has not yet been demonstrated.

To the objection that the intellectual inferiority of women, assuming it to exist, is due to the fact that for centuries she has not received the same education as man, M. Delaunay replies that it is unsound. "In past centuries

the mass of the people were sunk in ignorance; neither the one sex nor the other received any education." He therefore maintains, with Professor Bischoff, of Munich, "that women have not had to the exercise and evolution of their brains any other hindrances than those proceeding from their constitution and their capacity for development." On this point he uses Cyril's argument:

> "They hunt old trails," said Cyril, "very well;
> But when did woman ever yet invent?"

"Female musicians receive," he says, "the same education as males, and yet it is well known that, though there have been some excellent female performers, there is no instance of a great female composer. It is the same with painters and with the culinary art; among the thousands of women who have exercised the latter, there have been few, if any, *cordons bleus*. If we prepare a list of the men and another of women most distinguished in poetry, painting, sculpture, science, and philosophy, each containing a dozen names, the two lists would not bear any comparison." He cites the opinions of manufacturers and commercial men who employ individuals of both sexes. "They all agree that women are more assiduous, but less intelligent, than men. In printers' establishments, for example, women work with minute care, mechanically, without knowing very well what they are doing. Thus they make good compositors in the case of reprints, a work not demanding much intelligence, but set up manuscripts badly, not understanding them so well as men." To which it may be replied that as yet the capacity of women for such work has not been fairly tested. Miss Emily Faithfull asserts, however, that well-trained female compositors show as much readiness to deal with manuscripts as the best male compositors.

M. Delaunay touches, indeed, on the moral qualities of men and women, referring to the many authors who have maintained that women are more prone to every kind of wickedness than men, though he admits they commit proportionately fewer crimes. Poisoning is more favoured by women—that is, the poisoning of others—than by male criminals. "Moralists have noted that woman is more playful, more changeable, more capricious than man. She is also more destructive and less circumspect. The number of women run over in the streets is greater than that of men." *Quorsum hæc tam putida tendunt?* What has all this to do with M. Delaunay's subject? The argument which Mr. Delaunay uses to clinch his case, supplies the best answer to this part of his reasoning. "All known legislators take for granted the intellectual inferiority of the feminine sex as compared with the masculine. Everywhere woman is regarded as a minor, incapable of taking care of herself, and requiring a guide and tutor." The laws, in fact, having been made by man, assert his superiority, and so far as they can, ensure it. Woman is carefully placed in an inferior position, and then assured that she is an inferior being.

COMETS' TAILS.

By the Editor.

FROM what we have already seen, it will be manifest that the formation of comets' tails is a process of a very marvellous nature, as apparently involving forces other than those with which we are acquainted. The tail, ninety millions of miles in length, which was seen stretching from the head of Newton's comet nearly along the path which the retreating comet had to traverse (the comet thus passing away with its tail in front, instead of behind, as when it approached the sun), must, it would seem, have been formed by some force far more active than the force of gravity. The distance traversed by the comet in the last four weeks of its approach to the sun under gravity was no greater than that over which the matter of the tail, seen after the comet had circled around the sun, had been carried in a few hours. Yet we have no other evidence of any repulsive force at all being exerted by the sun—at least, no evidence which can be regarded as demonstrative—and still less have we any evidence of a repulsive force exceeding in energy the sun's attracting power.

Fig. 1.—Donati's Comet, September 24, 1858.

This difficulty, and the circumstance that a comet's tail lies in the direction opposite to the sun, or in the position which the shadow of the head would occupy, has led many, unfamiliar with the laws of optics, to suppose that the comet's tail may be simply the track of the luminous rays which have passed through the comet's head. They seem to think that the head may act in some way to send a beam of condensed light along the region opposite to the sun. It should hardly be necessary, however, to explain that no such beam of light could ever be seen where we see the comet's tail. The cases supposed to correspond with the formation in this way of the tail-like appendage are, in reality, of an entirely different kind. Thus, when we see a long beam extending from a bright light, we find that first the light has been caused to pass in that direction only (as when light is admitted into an otherwise darkened room through a hole); and secondly, there is matter along

the course of the light to be illuminated. The beam is simply that long array of material particles which the light illuminates while leaving the particles in neighbouring space in darkness. So understood, such a beam is seen to be utterly unlike a comet's tail; for, in the first place, we know of no matter behind the head to be illuminated; and, in the second, we know that light is falling on the regions all around the apparent array of illuminated particles, so that these surrounding regions should be as brightly lit up, which is not the case.

If any further doubt could remain as to this theory, it would be removed by first, the circumstance that the tail of a comet is generally curved; and, secondly, the existence of several tails extending from the head of one and the same comet.

Professor Tyndall started a theory based on physical experiments, and otherwise in better accordance with scientific possibilities. Having found that certain gases, even in an exceedingly attenuated form, form a luminous cloud under the action of the electric light, he suggested that a comet's tail may be a luminous cloud of this sort, formed in the æther of space by those rays of sunlight which have passed through the comet's head. The rays which, without passing through the head, fall on the æther of space, would not call into existence this visible cloud, because their heating action would destroy what their chemical or actinic action *by itself* would produce. And as fast as, by the comet's motion, the cloud formed behind the head came under full solar action, it would be destroyed. So the tail would always be behind the head.

It appeared to Professor Tyndall that the curvature of a comet's tail, or the existence of more tails than one, as in Donati's Comet (Figs. 1 and 2), was not inconsistent with this interpretation. For he noticed that, according to the gas dealt with, the luminous cloud would take a longer or shorter time in becoming visible. And he suggested that when the cloud formed slowly, the tail would be curved, the part near the head being behind the position which the head had recently passed through, while the part near the end of the tail would be behind the regions through which the comet had passed much earlier. Such luminous trails as were formed more quickly would account, he considered, for the straighter tails. He overlooked, I think, the circumstance, that the shape of the luminous cloud-trail would not in reality depend at all upon the length of time which the cloud might take in becoming visible. Light would pass with the same velocity through the different kinds of tenuous gas, and whether the cloud became visible at once along the space thus passed through, or did not become visible for several seconds, or minutes, or even hours, it would become visible at the farther end of its course only just so long after it had become visible at the nearer end, as light had taken in traversing the length of cloud so formed. This interval of time would be the same for the quickly-appearing as for the slowly-appearing luminous cloud, and there would, therefore, be no difference between their forms. It would be necessary to account in this way for the curvature of the larger tail in the figure, as compared with the straightness of the smaller tails, that the curved tail should have been more slowly extended from the head; whereas the theory gives the same rate of extension for both, namely, the rate at which light travels.

Fig. 2.—Donati's Comet, September 26, 1858.

We seem almost forced, by the phenomena of such a comet as Donati's, to the theory of the actual repulsion of matter from the head of the comet into the tails—matter repelled most swiftly forming the straighter tails, while matter repelled more slowly, and seemingly in greater abundance, forms the great curved tail.

We shall proceed to consider in our next paper the evidence which seems to show that, strange though this theory of material repulsion may be, it is in point of fact the only admissible theory. If this shall be established, we shall have to admit the existence of a repulsive force, whose action on the grosser material of planetary bodies is insensible.

"All knowledge, and wisdom which is the seed of knowledge, is an impression of pleasure in itself."—*Bacon*.

The Population of the Globe.—According to MM. Bohm & Wagner's *Bevölkerung der Erde*, Europe has now a population of 315,929,000 inhabitants, Asia 834,707,000, Africa 205,679,000, America 95,405,000, Australia and Polynesia 434,000, the Polar regions 82,000, giving a total of 1,455,923,000, being an increase of 16,778,000, according to the latest known censuses. At the end of 1877 Germany had a population of 43,943,000, Austria and Hungary (1879) of 38,000,000, France (1876) of 36,900,000, Turkey in Europe of 8,860,000, Russia of 87,900,000. In Asia, China possesses 434,900,000 inhabitants, Hong Kong 130,144, Japan 34,300,000, according to the census of 1878. The British possessions in India number 240,200,000 people (an estimate made before the census of this year), the French possessions 280,000, Cochin China 1,600,000, the East Indian Islands 34,800,000, the islands of the South Sea 878,000. The area of Africa is estimated at 29,383,000 square kilomètres, divided as follows:—Forests and cultivated land 6,300,000, savannahs 6,235,000, steppes 4,200,000, deserts 10,600,000. The inhabitants of British North America number 3,800,000, of the United States 50,000,000, of Mexico 9,185,000, and of Brazil 11,100,000. The Polar regions extend round the Arctic Circle with an area of 3,859,000 square kilomètres, and the Antarctic regious about 600,000. The population of the former is small, with the exception of Iceland, which has 72,000, and Greenland 10,000.—*Times*.

Reviews.

IN Mr. Paterson's "Studies in Life,"* he presents a series of lectures delivered to the members of a Young Men's Christian Association. His subject is full of interest, and his book is interesting, and, on the whole, well written; but it would have been much more interesting, and, as a literary work, it would have been far better, if he had forgotten that he was addressing a religious body, or (which comes to much the same thing) if he had remembered that he was speaking about science. Mr. Paterson seems to think that he must state nothing which he cannot prove to be in exact "accordance with what we read in the Book, and what we might expect from the narrative we have there." It is true that, after starting on this principle, which for a student of science is an illogical one, he is careful to discover an accordance on a plan of his own, and then to say that it is not because such and such theories oppose the Bible, but because they are inconsistent with facts, that he rejects them; but he only makes his position more illogical still by this most transparent device. The science writers of a hundred years ago were wiser in their generation. They said (we quote from the "Encyclopaedia Britannica," 1778): "This opinion, however plausible, we are not permitted to adopt, being taught a different lesson by Revelation . . . . we cannot doubt of the authority of Moses." If Mr. Paterson were content to do this, his position would be as logical as theirs. But, after expressing in effect the same opinion, he proceeds to argue the matter out, as though he were in doubt of his position. All the space thus occupied is simply wasted; and the reasoning can hardly fail to be as offensive to those who accept the authority of the Bible unquestioningly, as it is to those who cannot see what place Bible references can possibly have in scientific treatises. Mr. Paterson should know, every real student of science should remember, that science is of no creed as it is of no country. A writer of science has no more occasion to show that the science he teaches accords with his or any one else's interpretation of any religious book, than the tailor has to show that the clothes he makes are on a pattern accordant with Christian, with Mussulman, or with Buddhist doctrines.

Hence we must limit our praise of Mr. Paterson's work to the remark that if one-fourth were removed and the price proportionately reduced, it would be a work which students of biology would find worth getting and reading. With the fourth referred to would go much false science. We venture, too, to say that the interests of religion are, to say the least, not advanced by such passages as we refer to. It cannot conduce, for example, to the reverential spirit Mr. Paterson inculcates, to read a paragraph, beginning with the statement that Darwin's theory is essentially atheistic, going on to refer to what Mr. Paterson knows "in his inmost soul by the revelation of the Holy Ghost"; and closing with the statement that, even if the Bible does not forbid, hybridity sets up an impassable barrier.

In "Health Studies" † and "The Human Body," ‡ Mr. Paterson appears to better advantage, though he loses even here no opportunity of making science and religion simultaneously ridiculous.

Yet all three works are full of interesting and, for the most part, instructive matter; and but for the serious defect we have pointed out, they might all three be strongly recommended. Albeit, there is another fault—to wit, an affectation of simplicity, a very different thing from real simplicity. No one could write more simply than Faraday, yet his simplest words never wanted dignity, and never offended his reader's sense of self respect: when we find our author speaking of the features of the face as Mouthgate, Nosegate, and so forth, and gravely telling us that the nose is useful, "even though it may also be ornamental," and the like, we cannot accord the same praise to him.

* "Studies in Life," by H. Sinclair Paterson, M.D. (London: Hodder & Stoughton.)

† "Health Studies," by the same author.

‡ "The Human Body," by the same author.

"The Fiji Islands."—We omitted to notify in the footnote to our review of Mr. Horne's work on "The Fiji Islands," that it is published by Mr. Edward Stanford, Charing Cross.

THE GREAT PYRAMID MEASURES, AND THE DIAMETERS AND DISTANCES OF THE SUN, EARTH, AND MOON.

By Joseph Baxendell, F.R.A.S.

[It must not be understood that we accept our esteemed contributor's views. They illustrate well the whole theory of pyramid coincidences, but these coincidences disprove, in our opinion, what Mr. Baxendell considers that they prove.—Ed.]

A FEW months ago the results of a partial discussion of the Great Pyramid measures, given by Professor C. Piazzi Smyth, in the fourth edition of his work entitled "Our Inheritance in the Great Pyramid," led me to believe that the data which had formed the basis of the design for the Pyramid were the diameters and distances of the sun, earth, and moon, combined with the ratio (π) of the circumference of a circle to its diameter—a quantity which forms an important feature in the relations of the Pyramid measures; and, also, that in order to reduce the results of the astronomical data to magnitudes suitable for the design and construction of the Pyramid, a scale of one pyramid inch to a length, one-thousandth part greater than the present English mile, or 63,360 pyramid inches, had been used by the architect; but as I found that the values of the diameters and distances given in various astronomical works, especially those for the diameter and distance of the sun, would not yield results agreeing *exactly* with the Pyramid measures, although they were generally remarkably close approximations, I was induced to undertake a more extended discussion and analysis of the measures, with a view to ascertain, if possible, the exact values which had been employed by the architect in his reductions, and it thus became necessary to attempt a solution of the following problem. Given approximate values of the diameters and distances of the sun, earth, and moon to find the values which in simple combinations will give, with *strict exactness*, the various Pyramid measures and numbers, the scale for the reductions being one Pyramid inch for a Pyramid mile of 63,360 Pyramid inches. For some time I had considerable difficulty in forming the requisite number of suitable equations for the complete solution of this problem, but ultimately succeeded, and obtained the following values:—

	Pyramid miles.	English miles.
Diameter of the Sun	855,938	856,793
Equatorial diameter of the Earth	7,917·7	7,925·6
Diameter of the Moon	2,157·2	2,159·3
Mean distance of the Sun.........	91,758,800	91,850,558
Mean distance of the Moon	238,483	238,721

Let S = distance of the sun; M = distance of the moon; s = diameter of the sun; e = equatorial diameter of the earth; m = diameter of the moon. Then the following equations, in which pyramid miles and inches are adopted, will show the relations between these numbers and the pyramid measures:—

1. $\frac{se}{m} = 1{,}000{,}000\pi$.

It is probably owing to the remarkable relation in the magnitudes of the three bodies shown by this equation that the quantity π forms so prominent a feature in the relations of the Pyramid measures.

2. $\sqrt{s\pi^2} = 9{,}131{\cdot}05$ = length of one side of the base of the Pyramid.
3. $\sqrt{s2\pi} = 5{,}813{\cdot}01$ = height of the Pyramid.
4. $\frac{s\pi^2\pi\sqrt{\pi}}{25{,}000} = 1{,}881{\cdot}59$ = length of Grand Gallery.
5. $\frac{2\sqrt{s\pi}\sqrt{\pi}}{25} = 412{\cdot}13$ = length of King's Chamber.

6. $\frac{\sqrt{s\pi}\sqrt{\pi}}{1{,}000}$ = 5·151,646 = the number which has been called the key number to the dimensions of the King's Chamber, and of the Pyramid generally.

7. $S = \frac{25{,}000{,}000e}{m}$

8. $M = \frac{em\pi}{3^2 5^2}$

9. $M = \frac{se^3}{9ms}$

10. $\frac{2S}{5{\cdot}151{,}646M}$ = 149·37 = height of ante-chamber.

11. $\frac{3\sqrt{SMs\pi^3}}{250e}$ = 36,524·22 = perimeter of base of the Pyramid.

12. $\frac{3\sqrt{SMs\pi^3}}{500e\pi}$ = 5,813·01 = height of the Pyramid.

13. $\frac{es\pi^4}{75\sqrt{SM}}$ = 1,881·59 = length of Grand Gallery.

14. $\frac{80e\sqrt{s\pi^2}}{3\sqrt{SM}}$ = 412·13 = length of the King's Chamber.

Among the equations I obtained during the investigation were several which gave a smaller value for the diameter of the sun; and as I am not aware that any sensible difference has ever been observed between the polar and equatorial diameters, this result seemed adverse to the theory of a connection between the Pyramid measures and the diameters of the three bodies, until it occurred to me that probably one diameter referred to the photosphere, and the other to the comparatively dark and solid or liquid body of the sun. This latter diameter is 853,718 Pyramid miles, or 2,220 miles less than that of the photosphere, and the following equations, in which it is represented by the Greek letter σ, will show its connection with the Pyramid measures:—

15. $\frac{\sigma^2\pi}{e^2}$ = 36,524·20 = perimeter of base.

16. $\frac{\sigma^2}{2e^2}$ = 5,813·01 = height of Pyramid.

17. $\frac{\sigma^2}{100e^2}$ = 116·26 = length of ante-chamber.

18. $\frac{\sigma^2\sqrt{\pi}}{50e^2}$ = 412·13.

19. $\frac{\sigma^4\pi\sqrt{\pi}}{400{,}000e^4}$ = 1,881·59.

20. $\frac{\sigma^2\sqrt[4]{\pi^3}}{e^2 2\sqrt{10^5}}$ = 1,881·59.

21. $\frac{\sigma^2\sqrt{\pi}}{4{,}000e^2}$ = 5·1,516.

The length of the earth's polar axis is assumed by pyramidists to be 500,000,000 pyramid inches, or 7891·41 pyramid miles of 63,360 pyramid inches to the mile, or 7,899·30 English miles, while the value derived by Col. Clarke, from an elaborate discussion of measurements of arcs of meridian, is 7,899·11 English miles—the difference being therefore, less than two-tenths of a mile. I was, therefore, much surprised to find that the Pyramid measures would not yield a less diameter for the earth than 7,892·54 pyramid miles, or more than a mile greater than the generally-accepted length of the polar diameter. The question therefore arose—Can this latter length be in error to the extent indicated, or is the value I have obtained connected in any way with some marked feature of the Pyramid? It seemed to be highly improbable, if not impossible, that the results of the calculations of Bessel, Airy, and Clarke could be in error to the extent of more than a small fraction of a mile, and assuming, therefore, that the figure of the earth is truly spheroidal with major axis = 7,717·7, and minor axis = 7,891·41 Pyramidal miles, I calculated the geo-centric latitude in which a diameter will be 7,892·54 miles, and found it to be 78° 25′ 33″; and, deducting this from 90°, we have 11° 34′ 27″. A glance at this result at once suggested that it was the polar distance of the Pyramid pole-star, *a* Draconis, multiplied by the quantity π, and on dividing 11° 34′ 27″ by π I obtained 3° 41′, which is a very close approximation to the calculated polar distance of *a* Draconis at the time of the building of the Pyramid. Now a section of the earth through the parallel of latitude marked out in so singular a manner has a diameter of 1,583·54 Pyramid miles, or exactly one-fifth of the earth's equatorial diameter, and an area of 1,969,462 miles, or one twenty-fifth that of a section through the equator, which is 49,236,600 miles. The occurrence of the Pyramid numbers 5 and 25 in connection with the diameter thus indicated in so striking a manner gives a peculiar importance to it, and accordingly I have found that expressions in which it is a factor can be formed which give *exactly* the various Pyramid measures. Thus, representing this diameter by the Greek letter η (eta), we have

22. $\eta = \frac{S\sqrt{\pi}}{1{,}000 \times 5{\cdot}151{,}646}$ = 7,892·54.

23. $\eta = \frac{Se^2}{\sigma^2}$

24. $\frac{S\sqrt{\pi}}{1{,}000\eta}$ = 5·151,646.

25. $\frac{S}{\eta}$ = 11,626·02 = 100 times length of ante-chamber.

26. $\frac{S^2\pi\sqrt{\pi}}{1{,}000\eta^2 5{,}151{,}646}$ = 36,524·22.

27. $\frac{\sqrt{2e\eta}}{100}$ = 111·795 = height of granite wainscot in ante-chamber.

28. $\frac{450\sigma^2\eta}{5{\cdot}151{,}646e^3m\pi}$ = 119·37.

29. $\frac{S^2\pi\sqrt{\pi}}{400{,}000\eta^2}$ = 1,881·59.

30. $\frac{S\pi 5{\cdot}151{,}646}{100\eta}$ = 1,881·59.

31. $\frac{S\pi}{4\pi}$ = 9,131·05.

32. $\frac{S}{2\eta}$ = 5,813·01.

It may be remarked that the diameter η is exactly one seven-thousandth part greater than the polar diameter, and that the parallels of latitude in which it occurs may be regarded as the limits of the habitable portion of the globe.

The results of my investigation having proved that a measure corresponding to our English mile, and containing 63,360 Pyramid inches, was used by the architect of the Pyramid, it became a matter of interest to ascertain, if possible, how it originated, and ultimately I arrived at the following formula:—

33. $10\sqrt{\frac{Se}{m}}$ = 17,724·5 miles, which is the circumference of a circle whose area is 25,000,000 miles, or equal to the area of a section of the earth through the parallel of latitude in which the length of a diameter is equal to the mean of all the earth's diameters (7,904·545 P. miles). This area expressed in Pyramid inches, is equal to a square, the side of which has a length of 316,800,000 inches, and this, divided by 5,000 = 63,360 inches.

My experience in the development of the theory which has yielded the results given in this paper has convinced me that there is no feature of the Great Pyramid, or relation of its various parts, which cannot be expressed in terms of the astronomical data I have used, and in some cases, as I have already shown, two, three, or more equations can be formed, each containing one or more factors not in the others, but giving precisely the same result. It is evident, therefore, that the builder possessed a far greater amount of mathematical and astronomical knowledge than it has hitherto been supposed could possibly have been acquired by the ordinary course of observation and scientific investigation in the early age of the world when the Pyramid was built; and the fact that the values of the diameters and distances used by him are within the limits of the probable errors of the means of the best astronomical determinations of recent times proves that, so far at least as these values are concerned, modern science has made no real advance upon the science known to the builder of the Great Pyramid 4,000 years ago.

NEW MODE OF GROWING PLANTS.

By E. C.

To Dr. C. W. Siemens, the celebrated electrician, we are indebted for a series of experiments lately tried by him on the effect of the dynamo-electric light in promoting the vegetation and growth of plants. The idea that the electric current might be utilised in this way first occurred to him by observing that the blistering effect on the skin from this light was very similar to that produced by a hot sun. Without the aid of the sun's rays, *chlorophyll* is not formed, and this is an all-important element in vegetable life, as it produces the green colour of the leaves, and supplies the plants with carbon and starch for forming woolly tissues, by causing the decomposition of the carbonic acid vapour absorbed from the atmosphere by the leaf. The electric light, being, in fact, a sun on a small scale, has been formed to produce *chlorophyll* and other necessary chemical changes in a similar way to solar rays.

The apparatus employed for the series of experiments was a small upright Siemens machine, worked by a gas engine. The two carbons in the regulator lamp were respectively 10 and 12 millimetres in diameter, and the light it produced was equal to 1,400 candles. The first plan that was tried was by placing the lamp about 7 ft. above a melon pit and a reflector was arranged to concentrate all the light on the sash. Pots of rapidly-growing plants were, in succession, brought under this influence, some exposed only to the electric light, some to the sun alone, and some to the sun and electric light alternately. The latter made the most rapid progress, and were the best in colour. In the next experiment, the lamp was put inside a greenhouse, and as near the roof as possible. Various plants were placed in pots on the floor, at a variety of distances, and the light was kept burning all night for one week. The plants nearest to the influence of the lamp made the most rapid progress, and the foliage and plants were of a far brighter hue than if they had only had sunlight during the day. As regards forcing fruit, the electric light seems very efficacious. In ten days time some strawberry plants, which had been kept alternately under the influence of the sun and light, had large, full-flavoured fruit, while plants which had only been exposed to the rays of the sun for a similar period, had merely green berries on them. There is no doubt that the use of artificial sunlight in horticulture will be of immense advantage; but whether it can be used by market gardeners and people supplying the London markets, entirely depends on the price at which it can be produced. The machine used by Dr. Siemens, of 1,400 candle-light, costs about fivepence an hour to work, exclusive of a man, but including the cost of carbons. Dr. Siemens is of opinion, that a light equal to 6,000 candles would prove to be economical in working. This would have to be fixed 20 ft. above the ground. For forcing early fruits and flowers for the London markets, this discovery, if not too expensive, will be invaluable, as the rays may be concentrated on a brick wall, and by this means fruit may be rapidly ripened. At a lecture at the Royal Institution, given by Dr. Siemens, the action of the dynamo-electric light was tried on some tulips, and it had the effect of causing the small buds to expand to full-blown flowers in about twenty minutes. The electric machine will apparently soon be applied to purposes of general utility, as it enables work on a heavy scale to be carried out, and it is already employed for telegraph work, superseding the voltaic battery. Should Dr. Siemens succeed in producing it in a cheap form, it would be largely adopted, without doubt, in horticulture, as the advantages of being early in the market are well known. It seems almost an undisputed fact, that plants subjected to the influence of the dynamo-electric light arrive at perfection in rather less than half the time they would have taken if left only to the rays of the sun.

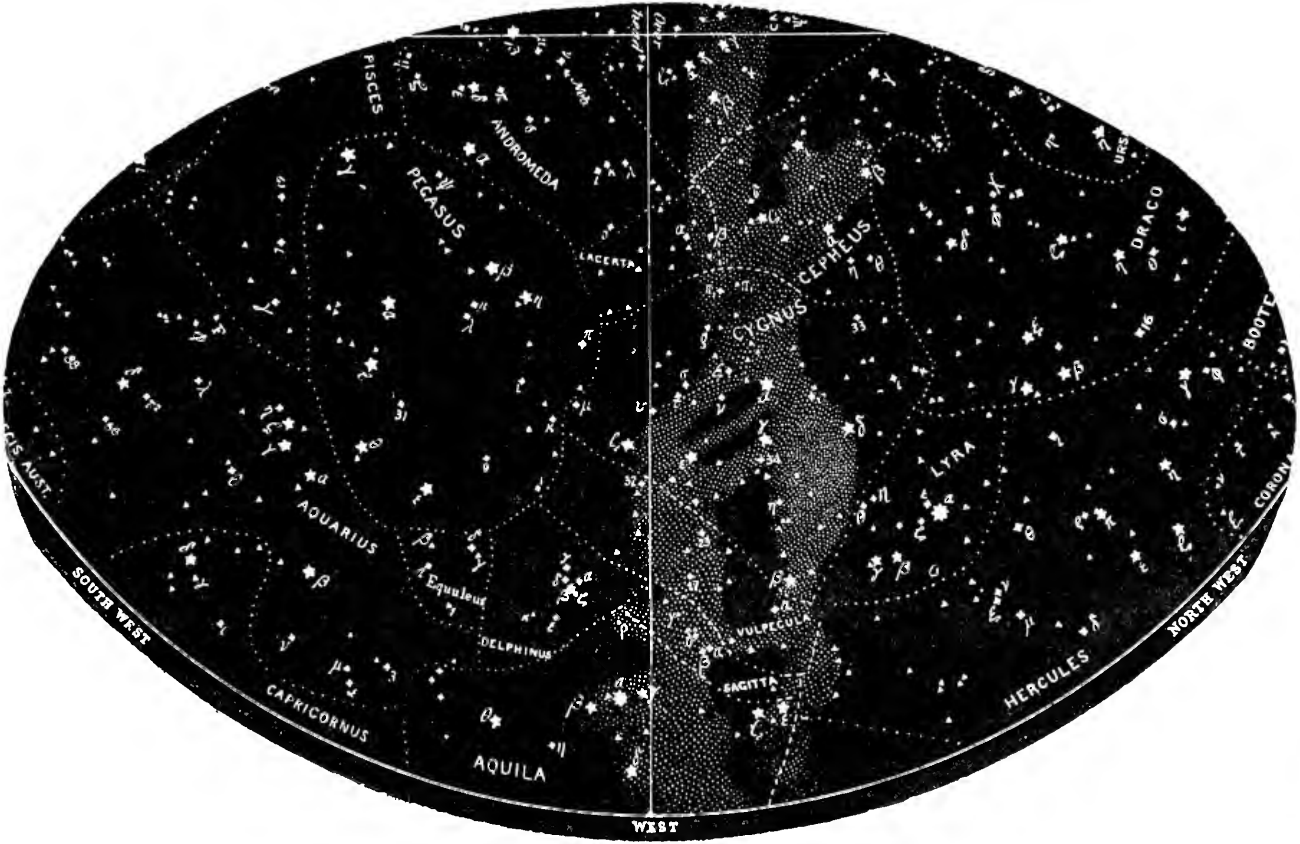
A PLANET OUTSIDE NEPTUNE.

Rather more than half-a-century ago, astronomers were beginning to suspect that outside the path of the planet Uranus another planet must travel, whose attraction caused Uranus to deviate somewhat from the motions which theory assigned to him. We know how this idea eventually led to the discovery of Neptune. From the observed disturbance of Uranus, the place of the disturbing body was determined by Leverrier and Adams independently, and when the telescope was turned to the region of the heavens where this as yet unknown planet should be, *there*, quite close to the calculated spot, was his disc seen. It seems not unlikely that before many years pass another planet further away than Neptune will be discovered. When Professor Newcomb, of Washington, published his "Tables of Neptune" in 1866, he said, "It is almost vain to hope for the detection of an extra-Neptunian planet from the motions of Neptune before the close of the present century." But since Newcomb's "Tables of Uranus" were published in 1873 the planet (Uranus) has been found to pass increasingly more in advance of its calculated place. Mr. D. P. Todd, Assistant in the Nautical Almanac Office at Washington, has found that by a graphical method (the problem not being yet in such a condition that processes of calculation can be applied), that the position of a disturbing body outside the orbit of Neptune can be approximately inferred. He places this body at a distance from the sun exceeding the earth's fifty-two times, so that its revolution around the sun would require 375 years. Its present position along (or near) the ecliptic would be within ten degrees either way of longitute 170°, so that it might be looked for ten degrees along the ecliptic, on either side of the star Tau Leonis. Assuming the distance correctly taken (as in the case of Neptune, the real planet may be at a much greater or at a much less distance than the hypothetical one, without greatly affecting the result as to the disturbing body's apparent position), the mass would be such that, assuming the density similar to that of the four giant planets, the apparent diameter would be about 2″1 (say about the 900th part of the moon's), and it would appear as a telescopic star of about the thirteenth magnitude. He puts the point where the planet's path crosses the ecliptic in longitude 103°, and the inclination to the ecliptic as 1°21′. By the kindness of Rear-Admiral Rodgers, superintendent of the Washington Observatory, Mr. Todd was allowed to begin the search for the trans-Neptunian with the magnificent 26-inch telescope. A power of 400 was employed, which would make the diameter equal to nearly half the moon's, and should therefore show the disc very obviously. On thirty clear moonless nights Mr. Todd searched along the neighbourhood of the ecliptic from longitude 126° 8′ to longitude 186° 1′, without leaving any unsearched space between these longitudes. He says, "if a trans-Neptunian planet is ever discovered, having a diameter as great as 2″, I shall be very much surprised if it is found that it must have eluded my search." But he suggests that at and about the time of the next opposition (which, if his assumed position is correct, would occur within ten or twelve days on either side of March 20), a new search should be made, with a telescope of sufficient power. If a careful and sufficient search near the indicated longitude should prove unavailing, it would be necessary to extend the observation to a limited zone, all round the heavens. It is not likely that the inclination of any trans-Neptunian planet to the ecliptic would be more than 2°, so that an ecliptical zone 4° wide would probably suffice for survey.

Reports of Societies.—We have been requested to insert reports of the meetings of various societies. But the papers read before scientific societies are, as a rule, unsuited to our columns, and we prefer (for the present at any rate) to give our space, which is limited, to matter not requiring translation before it can be understood by the general reader. We shall be very glad to find space for clear and simple accounts by the authors of papers communicated to societies, of the matter which they properly enough present in technical terms to fellow workers. And when papers are of sufficient interest, we shall present our readers with translation specially drawn up for these pages. But reports of scientific societies as usually prepared, that is condensed versions of statements too technical even without condensation to be generally understood, would be simply a waste of space in a journal expressly intended to be of interest to the general public. Moreover, a large amount of matter communicated to learned societies has no interest (even when explained) to other than experts. Our wish is not that all such matter as is thought suitable for communication to scientific societies should appear in these columns (by a sort of editorial pitchforking), but that whatever is worth knowing outside those societies may be sifted out and placed, when duly translated, before our readers. The former course would be the easier and the cheaper; the latter seems the more honest, as alone in accordance with our programme.

Indiarubber Gathering in Columbia.—An interesting account is given of this process in a report just issued by the United States Consul at Carthagena. When the hunter has found a rubber tree, he first clears away a space from the roots, and then moves on in search of others, returning to commence operations as soon as he has marked all the trees in vicinity. He first of all digs a hole in the ground hard by, and then cuts in the tree a V-shaped incision, with a machete, as high as he can reach. The milk is caught as it exudes and flows into the hole. As soon as the flow from the cuts has ceased, the tree is chopped down, and the trunk raised from the ground by means of an improvised trestle. After placing large leaves to catch the sap, gashes are cut throughout the entire length, and the milk carefully collected. When it first exudes, the sap is of the whiteness and consistence of cream, but it turns black on exposure to the air. When the hole is filled with rubber it is coagulated by adding hard soap, or the root of the mechvacan, which have a most rapid action, and prevent the escape of the water that is always present in the fresh sap. When coagulated sufficiently, the rubber is carried on the backs of the hunters by bark thongs to the banks of the river and floated down on rafts. The annual destruction of rubber trees in Columbia is very great, and the industry must soon disappear altogether, unless the Government puts in force a law that already exists, which compels the hunters to tap the trees without cutting them down. If this law were strictly carried out there would be a good opening for commercial enterprise, for rubber trees will grow from eight to ten inches in diameter in three or four years from seed. The trees require but little attention, and begin to yield returns sooner than any other. Those that yield the greatest amount of rubber flourish on the banks of the Sinu and Aslato rivers. The value of the whole indiarubber imported into the States annually is about $10,000,000.

THE WESTERN SKIES IN NOVEMBER.

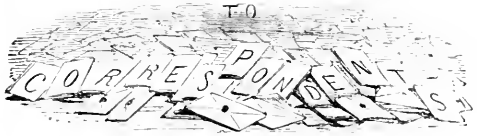


This Map shows the position of the stars in the Western Skies:—

On November 3, at 11¼ o'clock. | On November 10, at 10¾ o'clock. | On November 18, at 10¼ o'clock. | On November 25, at 9¾ o'clock. | On December 3, at 9¼ o'clock.
On November 7, at 11 o'clock. | On November 14, at 10½ o'clock. | On November 22, at 10 o'clock. | On November 29, at 9½ o'clock. | On December 7, at 9 o'clock.

The stars in the western skies are passing downwards towards the horizon, moving aslant, from left to right; but those shown on the right or northern side of the map will not set, but pass over towards the north, there to pass below the pole.

*** Stars of the first magnitude are shown with eight points, those of the second with six, of the third with five, of the fourth with four, of the fifth with three.



Letters to the Editor.

[*The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with their writers. He requests that all communications should be as short as possible, consistently with full and clear statements of the writer's meaning.*]

All communications should be addressed to the Editor of KNOWLEDGE, *74, Great Queen-street, W.C.*

All Cheques and Post-Office Orders to be made payable to Messrs. Wyman & Sons.

∴ All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.

All Letters or Queries to the Editor which require attention in the current issue of KNOWLEDGE, *should reach the Publishing Office not later than the Saturday preceding the day of publication.*

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

Our Correspondence Columns.

ERROR IN COMPUTING PERIPHERY OF ELLIPSE.

[14]—Will you pardon me for pointing out a slight error on page 37 of KNOWLEDGE?

In the reduction of the expression

$$7\pi\left[1-\frac{1}{4}.\ \frac{13}{49}-\frac{1}{64}.\ \frac{(13)^2}{(49)^2}\ \&c.\right]$$

You give the result as 20·58 in., instead of which it should be 20·508 in. I give the details as follows:—

$$7\pi\left[1-\frac{1}{4}.\ \frac{13}{49}-\frac{1}{64}.\ \frac{(13)^2}{(49^2)}\right]$$

$$=7\pi\left[\frac{64.\ (49)^2-16.\ 49.\ 13-(13)^2}{64.\ (49)^2}\right]$$

$$=7\pi\left[\frac{153664-10192-169}{153664}\right]$$

$$=21.\ 99912.\ \frac{143303}{153664}$$

$$=20{\cdot}508.$$

The solution by logarithms gives a similar result.

Yours faithfully,

WILLIAM J. HARDING.

P.S.—May I suggest that the above expression may be given as an example worked out by logarithms to show their great convenience?—W. J. H.

[Mr. Harding is quite right. Turning to my computation, I find logarithm of result given correctly as 1·3119321, which is the logarithm of 20·50841. I took out the number, however, incorrectly. The difference between the circumference of an ellipse having axes 7 and 6, and the circumference of a circle having diameter 6½, is only a tenth of what I deduced, and is in fact less than the hundredth of an inch. Had I taken axes less nearly equal, there would have been a greater difference; but it is only when the very irregular heads are considered that the difference arising in this way can be worth taking into account.—ED.]

COMETS.

[15]—I have read with interest the article on comets. I am always on the look out for astronomical news, and turned to the article in question in the hope that some light might be shed on the physical nature and purpose of these mysterious tourists in space. I have long been expecting some definite theory to be broached to account for them; some facts have been collected, but comets are still, apparently, without the pale of celestial civilisation and order. The connection between them and meteor streams is established. Are we, then, to regard meteor streams as the condensed material of comets left behind, and separated from the main body in the course of its revolution round the sun? If so, how is the fact accounted for that spectroscopic examinations shows that comets are mainly composed of the vapour of carbon, while the meteorites that have been examined generally contain a large proportion of iron, nickel, &c.? Is it possible that a flight of meteors at a great distance might present the appearance of a comet? Are comets to be regarded as a primary condition of matter, to be afterwards condensed into meteors, and these in turn to assist in the formation of new planets, and the gradual increase of old ones? I believe the theory, first put forth by the editor of KNOWLEDGE, that the rings of Saturn* are composed of a multitude of minute satellites, is generally accepted. Would it be in accordance with that theory to go a step further, and consider the rings to be dense meteor streams? and if so, might they have been introduced to Saturn's system by a comet or comets? Any information on these points will, doubtless, be acceptable to others besides

ONE WHO WANTS TO KNOW.

P.S.—The crape ring of Saturn was well seen here (Hastings) on the evening of Nov. 6, power 250, 4¼ in. refractor.

FIGURES OF THE CONIC SECTIONS.

[16]—When I am studying geometry, as in Euclid, I am able to describe circles of any size and in any position with compasses. But in studying the geometry of the conic sections, I am not able to make such illustrations as I want. I cannot draw the parabola or the hyperbola freehand; nor, indeed, can I draw a satisfactory ellipse in this way. But, even if I could, I want something more. I want to be able to draw with exactitude a parabola, hyperbola, or ellipse, as occasion may require, in any position and of any size or shape (parabolas, of course, are all of the same shape, but hyperbolas and ellipses are not). I observed a few weeks since in a back number of the *English Mechanic* what seems to me a very simple and satisfactory way of drawing parabolic and hyperbolic arcs, as follows:—

For the *parabola*, set two straight lines AB, BC, (Fig 1) at right angles to each other, and divide each into the same number of equal parts in the points 1 2 3 4 5, join A1, A2, A3, A4, A5, and through

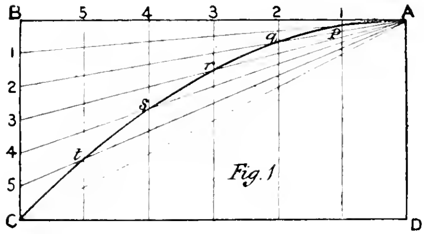


Fig. 1

the points of division along AB draw parallels to BC. Then the points p, q, r, s, t, in which the parallels through 1, 2, 3, 4, 5 meet A1, A2, A3, A4, A5, lie on a parabolic arc through AC, which can be swept out as in the figure.

For the *hyperbola*, set two straight lines AB, AC (Fig. 2) at right angles, and from a point O outside BAC draw through any points 1, 2, 3, 4 on AB, straight lines cutting AC in 1, 2, 3, 4. Through

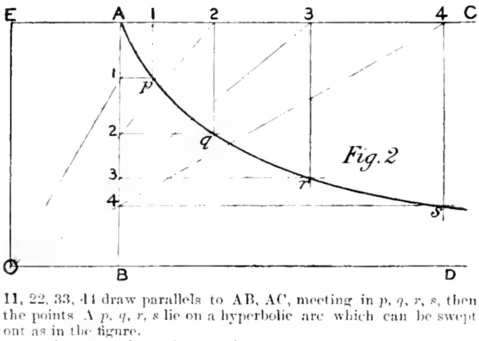


Fig. 2

11, 22, 33, 44 draw parallels to AB, AC, meeting in p, q, r, s, then the points A p, q, r, s lie on a hyperbolic arc which can be swept out as in the figure.

This is well, so far as it goes; but it does not meet my difficulty.

* [The theory was first advanced by the Bonds, in America, and independently by Clark Maxwell, of Glasgow.—ED.]

We get in this way a parabolic and a hyperbolic arc (the ellipse cannot be represented at all in this way by the simple use of parallel straight lines), but we have no means of drawing a parabola or hyperbola of given size, having a given focus and directrix, or in the case of the hyperbola, having a given centre and given asymptotes. I am aware that there are mechanical means, some of which seem simple enough, for drawing all the conic sections, but they do not suit my purpose. I want to be able at any time, with the instruments found in an ordinary box of instruments, to draw a parabola, hyperbola, or ellipse of determinate size, position, and shape. Can you help me to meet this difficulty?

GEOMETRICUS.

[I will take an early opportunity to describe some simple methods of drawing the conic sections in the way required by Geometricus. The methods described in his letter are correct; but in practice it is well to have methods which give a series of enveloping lines, guiding the hand in sweeping out the curves after such points as p, q, r, s, and t have been obtained; or the curve may be struck out in that way without obtaining determinate points as above. To illustrate my meaning, let Geometricus join C 3 in Fig. 1, which line will be trisected where crossed by p 5 and s 4. Join these points of trisection with the points 2 and 1 (on A B) respectively; then the two straight lines thus drawn and C 3 will touch the parabolic arc at C, s, and q respectively. With A B there will be four tangents. If A 3 and C 3 (3 on A B) be divided into six equal parts, and the successive points of division along C 3, be joined with the successive points of division along 3 A, there will be obtained seven tangents touching the arc A C at A, p, q, r, s, t, and C. Tangents for the curve A, q, s, Fig. 2, may be obtained in another way, which I will describe, as also methods for drawing ellipses, either by obtaining any number of points along the curve, or by getting a series of tangents enclosing it. In the meantime, I note that A is the vertex, AD the axis of the parabolic arc AC in Fig. 1; neither its focus nor its directrix is given directly by that method, but by taking BC bearing a definite ratio to AB, the position of the point which is at the extremity of the latus rectum (or focal chord perpendicular to the axis) can be determined at once, and thence the position of the focus and directrix. In Fig. 2, A q, is part of one branch of a rectangle hyperbola, of which O is the centre, and OD, OE are asymptotes. The point A is not a determinate point on the curve unless the rectangle OA is also a square, in which case A is the vertex of the branch. By making OA a rhomboid, a hyperbolic arc having its asymptotes inclined at any angle, can be obtained; and if the rhomboid is also a rhombus, the vertex of the arc so described will be at that angle of the rhombus which lies farthest from O.—ED.]

CAN ICE-YACHTS SAIL FASTER THAN THE WIND?

[17]—"Upsilon" (letter 3, p. 16) is one of the many and intelligent men who have been perplexed by this apparently simple question. I think the following considerations appeal to the judgment, perhaps, more forcibly than those you append to "Upsilon's" letter:—

If a ship is sailing before the wind, a pressure is manifestly exerted on its sails, and in consequence, the velocity increases until the ship has the same velocity as the impelling fluid (the air). This is the theoretical limit, for then the pressure ceases.

But if, on the other hand, a ship is sailing with the wind abeam, no matter how great the velocity, the moving air exerts a pressure on the sails. The component of this pressure resolved in the direction of the ship's motion tends to increase that motion, and since the wind pressure is constant in action and direction, the ship may be considered to be moving under a uniformly accelerating force. Hence, if there be no drifting to leeward (of course, a theoretical consideration), there is no theoretical limit to the velocity which the ship may attain. In the case of ice-yachts, the drifting and friction are at a practical minimum, and the speed they attain may be very much greater than that of the wind.

CRUSADER.

[The reply we gave last week takes the line which "Crusader" suggests. He will see, however, that the accelerating force is not uniform, but diminishes as the velocity of the ship increases. It would be a pretty problem to determine, with certain necessary assumptions as to sails, frictional resistance, &c., the maximum velocity attainable with a given wind. ED.]

IS THE SUN HOT? (*Abstract.*)

[18]—The sun is regarded as the fountain head of all terrestrial energy. The gravitation of the central mass of the sun causes tremendous compression, giving birth to the forces that are transmitted to us. Now the forces that we are most sensible to are heat and light, but there is another force that we are not so sensible to, i.e., chemical force. Did "Anti-Guebre," when he drew near to his fire, ask himself the following question: Where did the heat originate? The answer is: The sun. For the sun transmitted his energy (by the medium of the æther) to the earth, the force was utilised by plants, plants in course of time changed into coal.

Now, as regards the "mountain proposition." It is a well-known fact that the "rays" of the sun pass *through* the atmosphere without materially altering its temperature, and are absorbed by the earth, which gives out *again* the heat which it has absorbed to the surrounding atmosphere. Now, on a mountain or in a balloon, we are further from the actual source of heat, and the air, being thin and rarefied, does not absorb heat like the denser and nearer-to-the-earth atmosphere.

The explanation of the mountains, &c., will also apply to the cirrus clouds.—Yours, &c. SUN.

[19]—With regard to Mr. Newton Crosland's letter (No. 5, p. 35) on this subject, which is after all mere supposition, surely it is more natural to suppose that a body which produces all the phenomena of heat is in itself hot—at all events, till we find more evidence to the contrary than N. C. can furnish. He says the inflammatory action may be merely the chemical conversion of substance into force. I should like to know what is combustion but this? Your correspondent "Tyro" seems under the impression that light is visible; perhaps when he hears it is not, he will be able to account for the non-appearance of the broad flood of effulgence.—Yours, &c., C. J. SHAW.

[20]—I may, perhaps, be permitted to say a word in opposition to "Anti-Guebre's" views.

Before discussing any point, I shall at once state that I take the "conception of a medium filling space, and fitted mechanically for the transmission of vibrations of light and heat"—in other words, the *luminiferous æther*, as the foundation of my remarks. "Anti-Guebre" leaves the subject of radiation and absorption untouched, and here I think we have the explanation of his observations.

The conception of the æther filling space may now fairly be said to be inductively proved. Prof. Tyndall's experiment, in which he allows heat waves from a radiant body to pass through a glass tube, taking the temperature by the thermo-electric pile, shows, beyond a doubt, that *air* is incapable of absorbing heat.

He says "oxygen, hydrogen, nitrogen, and the mixture, atmospheric air, prove to be practical vacua to the rays of heat." Waves of heat, then, travel from the sun to us without having been absorbed; but directly they impinge upon the earth, and upon absorbing and radiating bodies, they produce heat, and cause a brisk radiation. Taking the accepted scientific definition of heat as "a brisk agitation of the parts of an object," we can easily understand that when the atoms of the æther "swing" with the atoms of the bodies upon which they impinge, a quicker movement takes place, and consequently heat is generated. Absorption, then, is a source of heat, and heat so generated is radiated into the air and turned to account. The power of the atmospheric aqueous vapour to absorb heat *radiated from the earth* is immense, and it is calculated that 10 to 15 per cent. of heat from the earth is absorbed within 10 or 20 feet of the earth's surface. It has been observed that where the air is dry, and remarkably free from aqueous vapour, as in Australia, the temperature of the night is 50° to 60° below that of the day, because there is no check to radiation. Anti-Guebre's, and all observations fall in with these explanations, and prove them. The further we go from the earth's radiation, the colder we get. It is a remarkable fact, proved by experiment, that aqueous vapour has the power to absorb rays of heat coming from the *earth*, but is incompetent to absorb rays from the *sun*.

By this remarkable adjustment the earth is rendered habitable.

G. F. P. DYER.

1, Queen-square, Bath.

WASTE OF SOLAR HEAT.

[21]—The letter in your present issue touches on a point in which it has long appeared to me that scientific language is erroneous. We talk of "Heat" coming from the Sun; and under that impression, "Anti-Guebre's" complaint of the "loss of Heat" has some plausibility. But it appears to me that the energy radiated by the Sun is only one factor of the result called Heat.

Energy	+ or	×	Something	=	Heat
Energy	+ or	×	Something else	=	Light.
Energy	+ or	×	Something else	=	Electricity,

and Heaven knows how many other yet unknown powers. There-

fore, it is by no means certain that this energy should be wasted, although it fails to (i.e., is not designed to) form heat on this little globe.—Cogito.

A CLEVER SEA-GULL.

[22]—During a recent passage over the North Sea, a flock of sea-gulls followed the steamer for many miles. At last I noticed that one of them, a remarkably fine bird, had, by some chance, got an angler's line attached to its wing. The poor thing flew about the rigging, its companions, meanwhile, uttering loud cries. After great cawing, the bird flew quickly towards the ship, dashed round one of the ropes several times, and ultimately flew off, leaving the line twisted round the rope. Was this what Mr. Herbert Spencer would call "reason in inferior animals?"

Hoping to see your journal succeed, as it deserves to do,

I remain, yours,

Geo. B. Fraser.

Helensburg, November 5, 1881.

A LUNAR ILLUSION.

[23]—With reference to geometrical illusions, I am sure that many persons must have noticed the following, and yet my attention has never been drawn thereto either in conversation or in print:—The crescent moon being Diana's bow, we shall give the name "sagitta" to the line drawn from the middle of the invisible bow-string connecting the two horns of the moon to the middle of the convex side of the crescent, or the illuminated limb of the moon; and we shall still apply the name, even when the moon is no longer a crescent. The sagitta is necessarily aimed directly at the sun; and yet, to the eye, it always seems to point above, often very considerably above, the sun. The reason is that we are accustomed in drawing, and looking at, pictures on flat surfaces, to consider and treat points or forms as they are projected on the "plane of vision"; but they are really presented to us as projected on a spherical surface, whose centre is the eye. The difference is trifling when the angular field of view is small, and from habit we neglect it, and we are led to do this even when the difference is very important, and we know it to be so. Take the simplest case of the illusion mentioned. The sun is on the horizon, and the moon is 90° distant, not perceptibly different from half-moon; we shall suppose her altitude to be 25°. The moon is at the highest point of the great circle of the sphere of vision passing through the sun, the moon, and the point of the horizon opposite to the sun. As the sagitta points directly to the sun, it lies on that great circle; but as it is at the highest part of that great circle, it is horizontal. Now, when we produce the horizontal sagitta sunwards by the eye, we cannot help picturing to ourselves a line which remains always parallel to the horizon, and our mental production of the sagitta, instead of hitting the setting sun, passes 25° above it.

The illusion is connected with the fact that, in turning to look from the moon to the sun, the observer rotates his head, or perhaps his whole body, round a vertical axis, and not about an axis perpendicular to the plane containing his eye, the moon, and the sun. By turning himself properly in the latter manner, the observer can, by the eye, correctly produce the sagitta so as to hit the sun; it will be better, though not necessary, to screen off the horizon. We have neglected refraction, the effect of which is small comparatively, but in the case considered, the difference of refraction goes to diminish the illusion, and not to help it.

M.

[Five or six years ago I put the question raised by "M" before the readers of the *English Mechanic*. It is connected with a question of some interest to artists, viz., the true rules of perspective for pictures including a very large visual area.—Ed.]

LATIN QUOTATIONS.—OPTICAL ILLUSION.

[24]—I welcome with much pleasure the appearance of your new scientific journal, and feel sure that it will meet with the support it deserves. It is not everyone who cares to pay sixpence for *Nature*, and the cheaper ones are sadly wanting in the tone and character which I think should pervade everything connected with science. I should like, however, to make one deprecatory remark. I think that the too frequent use of Latin quotations should be avoided in a paper addressed principally to those who have not had an university education. Many who take a lively interest in science have not had time to study the dead languages,* and to them (unless accompanied by a translation) these quotations are simply repellent.

I enclose an instance of an optical illusion just observed. It will be noticed that Mr. Smith's address appears to decline to the left. Yours truly,

Nov. 9, 1881. C. J. Watson.

Mr. J. J. SMITH,

Blank Green,

SMITHBOROUGH,

Near BLANKTOWN

* [There are quotations so familiar that they can scarcely be regarded as belonging to a dead language. Such was the one I used, *Nescit vox missa reverti*, which means that "what has been said cannot be unsaid."—Ed.]

OPTICAL ILLUSIONS.

[25]—The No. 1 illusion I made an independent discovery of many years ago, as also, no doubt, have many more who have been much used to drawing geometric and mechanical patterns. Since the advent of Knowledge, however, I have thought a little more over it. The explanation of this figure is, I believe, to be found in the peculiar movements of the muscles of the eyeballs. The principal movements of the eyeball are vertical and horizontal. Vertical lines are followed by means of one pair of muscles, which move the eyeball through a vertical plane. Horizontal lines are followed by corresponding muscles. Oblique lines are followed by the eye through the combined action of both these pairs of muscles. It is true that there is also an oblique pair of muscles, but from their position, I do not think they assist the eye to follow an oblique line, being more suited to rotate the eye upon the axis of its lenses. Referring now to diagram No. 1, the eye follows the line EG by a simple movement, but in following the line AB, both pairs of muscles are employed. This movement being more complex, is not so certainly continued when the guide of the line is lost by the interposition of the space EH. The eye is led away, as it were, also by the commencement at the point B of the line BG, which breaks the continuity of the oblique movement. It seems to me further complicated by the movement of

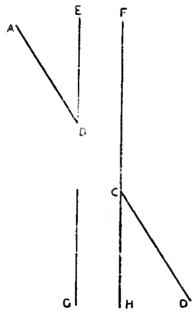


Fig. *a*.

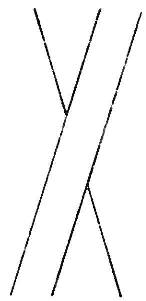


Fig. *b*.

two eyes, which work more easily together in the vertical and horizontal movements than in the oblique. This may be noticed when closing one eye, the illusion being then not so complete.

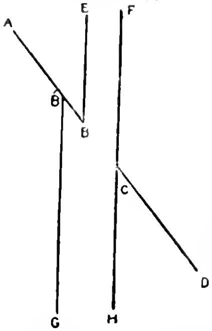
In confirmation of my remarks, I send you two diagrams. If the line EG be broken at B, the illusion is almost destroyed, the line having lost its power of attracting the eye to a simpler movement (Fig. *a*).

Again, if the same diagram be set before the eye symmetrically disposed to the vertical position, so as to present to the eye similarly situated oblique lines, the illusion will vanish, as then the oblique movements of the eyes are required to follow both the parallel line *and* the one which is opposed to them. This, then, brings the diagram to the form of an ordinary Roman X (Fig. *b*) in which there is no difficulty in following the single line. If this same diagram be set before the eye with the parallel lines either vertically or horizontally disposed, the illusion will reappear. Hoping Knowledge will be as successful as it deserves, I remain, yours, &c.

W. D. Richmond.

[26]—In his interesting article on "Illusions" in your first number, Mr. Foster writes

"The lines A, B, C, D of Fig. 5 appear to be curved so as to be nearer at the middle than at either end; while the lines E, F, G, H, in the same figure, appear to be so curved as to be farther apart in the middle than at either end."



The impression on my mind on looking at the figure is *exactly the reverse* of this. Is the letterpress at fault,* or does the difference depend upon my mental reception of the phenomena?

Referring to Fig. 1, might I be allowed to suggest as an explanation of the illusion, that the mind unconsciously grasps the impression of the vertical distance between EG and FH to the exclusion or depreciation of the greater distance between B and C. I find that if I construct the figure as below, making the distance of B'G from FH equal to the diagonal distance BC, the illusion disappears.—Yours faithfully.

Wm. H. Allen.

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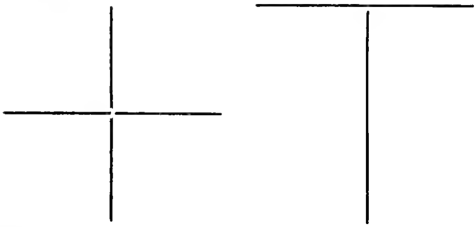
[27]—A correspondent sends the following:—



How much longer is the perpendicular than the horizontal cross-bar?

A NEW THEORY OF THE TIDES.

[28]—Your correspondent, "A Fellow of the Royal Astronomical Society," in your first Number, was very hard upon paradoxers. Would he be very much shocked to hear—if you will permit me to utter the contumacious opinion—that one of the most blundering paradoxes with which I am acquainted is the Newtonian doctrine of the tides? Let us examine this question carefully and briefly. Every one knows that the tide rises or falls simultaneously on opposite sides of the earth. Newton endeavoured to explain this phenomenon by the hypothesis that the moon attracted the water on the side nearest to itself, and at the same time drew the earth away from the water on the opposite side. This theory has been accepted with universal approval, but can anything be more erroneous, not to say absurd? We are coolly asked to believe that when the sun and moon are in conjunction on one side of the earth, drawing up the tide, one of the most mobile of substances, the water, on the other side, is left bulging out into space, where there is no rival attraction to hold it, and that it is capable of resisting three attractions—that of the sun, of the moon, and of its own earth—all at once!

If this is the best explanation that can be given of the tides, no wonder that Aristotle proclaimed the difficulty to be "the grave of human curiosity."

I maintain that it is a monstrous scientific fallacy.

Those who pull down ought to be compelled to reconstruct; those who denounce one system are bound to offer some substitute which may be considered preferable. Therefore, I must not shrink from this imperative task. With your permission I will here give a quotation from my paper on "The Astronomy of the Future" in "Pith":—

"What we say is, that the magnetic attraction of the moon, as a magic wand or beam, penetrates the earth from one side to the other—sword-like it sheathes itself in the diameter of our globe. At the spots where this spiritual and invisible falchion makes its entrance and exit, the waters rise to meet it in obedience to its mighty beckoning and summons; and as the moon is for ever shifting its position, so the waters are *for ever* chasing over the surface of the globe the two mundane extremities of the moon's irresistible, triumphant electrical wand! Or we may, as an alternative, assume that this magnetic influence, which is attractive on one side of the earth, becomes repellant on the other."

I now wish to invite your readers' attention to another point connected with the Newtonian System of Astronomy. I fancy that we must discard the theory of "universal gravitation" which has been so belauded, in favour of the theory which I have termed "The Polarity of the Universe." Gravitation, with its clumsy addition of a centrifugal force, is simply an impossible explanation of the movements of the heavenly bodies. Centrifugal force, which is entirely of a mechanical origin, cannot be originated and sustained between two bodies unless they are physically connected and moved from a central power; whereas magnetic polarity, with its attraction and repulsion, is a satisfactory solution of the phenomena of motion in the solar system. As long as a planet presents its poles in a slanting direction towards the sun, the revolution of the planet is an everlasting dynamic certainty, and it is quite possible that the sun may possess a number of poles, one for each planet.

In the first article on "Comets," you say that Newton discovered that their motion was regulated by the law of gravitation. I contend that this discovery was a delusion. We know that some comets move off in curves, which render their return to our system an impossibility; and if there occurs, in Nature, one exception to a law, what becomes of such law? It is null and void. I submit that comets *do* obey the laws of polarity, and that they *do not* obey the law of gravitation.

I believe that we must abandon the teaching of Sir Isaac Newton, and consider him one of the paradoxers and scientific old fogies of the past.—Yours, &c., Newton Crosland.

London, *Nov.* 12.

[I fear Mr. Newton Crosland belongs to the class of confirmed paradoxers, those who having encountered difficulties which they have been unable to surmount, suppose that they have made discoveries concealed from others. As a mere matter of fact, the accepted theory of tides would give (apart from effects of oceanic friction) low water under the moon and opposite that region; yet the Newtonian theory that the moon's attraction would of itself cause a leaping up of the water, both under the moon and opposite, is sound. Gravitation requires no centrifugal force, and, indeed, it is only in treatises by ill-informed writers that the theory of a centrifugal force as such is propounded. What is called centrifugal force is in reality simply the measure of what the centripetal force does, it is only another way of viewing the centripetal force. I believe Mr. Crosland's views will be received when Newton is entirely forgotten—but not till then.—Ed.]

DR. FERRIER AND VIVISECTION.

[29]—A distinguished professor at one of our colleges once said to me: "The manner in which our countrymen receive the conjectures of popular teachers of new doctrines only proves what most of us knew before, that they are the most gullible of all intelligent nations." The remark may be applied not only to the votaries of science, but to the followers of any agitator who from the rostrum calls upon his supporters to uphold or denounce what he considers to be right or wrong. As a nation, we are opposed to cruelty and oppression. We boasted in former days of our Anti-slavery Leagues, and our Society for the Prevention of Cruelty to Animals receives (and justly) a large share of public support. If a man kicks his donkey or starves his dog, he is speedily brought before a justice of the peace, who rarely fails to punish the offender. Last Thursday the humanitarians applied for a summons against Dr. Ferrier, under the Vivisection Act, for performing an experiment which was described as being "frightful and shocking."

The summons was granted, and long ere this many thousands of kind-hearted people of both sexes have given vent to their horror of the professor's experiments on the two unfortunate monkeys, and doubtless have settled in their minds that he is a fit subject to be made an example of, and worthy of punishment.

The vexed question of vivisection has been well ventilated by abler pens than mine. It is not my intention to say a word either for or against the practice, but the impartial reader should pause before he passes sentence in this matter. *Audi alteram partem* should be the motto of every right-thinking Englishman, and important subjects, particularly those relating to medical science, should be carefully analysed before an opinion is expressed, and such a man as Dr. Ferrier ought not to be placed in the category with the human brutes who daily appear in the dock to answer charges of wanton cruelty.

[* The error is in the figures; the upper should have been the lower.—Ed.]

I have long watched with interest the results of Professor Ferrier's labours in the cause of humanity, and a few words upon the nature of his experiments may not be out of place here.

Physiology is, comparatively speaking, a new science. Before the discovery of the circulation of the blood, physicians were little better than charlatans. Many of the early philosophers were mere dreamers; they made little or no advancement in the natural sciences, but their extravagant ideas influenced civilised nations for nearly twenty centuries. Aristotelianism and peripatetic dogmas *hindered* the progress of science. When the great Bacon arose giant-like, amidst the chaos of hypotheses and truthless doctrine, and laid the foundations of the inductive and experimental philosophy, then a new era commenced, and scientific progress followed.

There is no subject in physiology more attractive than the study of the human brain; that wonderful and complex organ has occupied the attention of philosophers in all ages, but little light has been thrown upon its functions until recently. Several physiologists of the seventeenth, eighteenth, and early part of the present century, wasted a deal of their time in fruitless searches after the exact seat of the soul. Some held that the soul was equally diffused through every part of the body. Descartes maintained that it is in the pineal gland of the brain, and Borri asserted that in the brain is formed a certain very subtle, fragrant juice, which is the principal seat or residence of the soul, and that the subtlety and fineness of the soul depended on the temperature of this liquor, rather than on the structure of the brain. Now, all these fanciful theories were very interesting to read, but they did not benefit mankind, and it was not until Flourens commenced his experiments upon the brains of animals that any definite knowledge of the functions of the cerebral organ was arrived at.

A worthy follower in the footsteps of the French physiologist is Dr. David Ferrier. The work of the latter on the "Functions of the Brain" has been justly described as "marking the end of an old era and the beginning of a new one in cerebral physiology." In briefly summing up the results of Dr. Ferrier's experiments on the brains of animals, I wish to direct attention to the localisation of two or three of the cerebral functions as demonstrated by the learned professor. The upper end of the Fissure of Sylvius, called the angular gyrus, is the centre of the sense of sight. When that part of the cerebral substance is destroyed, the sense of vision is gone in the opposite eye, though all the other senses remain, and an animal can hear, and smell, and taste, and touch, and perform every voluntary action as before. "It is a remarkable fact," says Dr. Ferrier—and let it comfort you, oh ye anti-vivisectionists—"that the brain is unsusceptible to every kind of irritation except electricity. You may cut and cauterise the brain without exciting sensation, although it is the organ of feeling. This we have learnt from experiments and the testimony of men who have had their brains injured." In another convolution of the brain, called the temporo-sphenoidal lobe, Dr. Ferrier found the centre of the sense of hearing—from the lower extremity of the lobe proceeds a large tract or process which is called the olfactory tract. When this part of the brain is destroyed, the sense of smell is abolished. All these discoveries explain certain facts which have been observed in connection with disease. Of course, I have only selected a few of the many illustrations of the Professor's experiments, and it only remains for me to point out the vital importance of such discoveries to the medical profession, and, I may add, to all who are suffering, or likely to suffer, from cerebral disease. It is impossible to experiment upon the human subject. There is no alternative but to practise upon the living animal, if an accurate knowledge of the functions of that important organ is to be gained.

Surely, when the facts are known, few will accuse the Doctor of wanton cruelty. All his experiments are made for the benefit of suffering humanity and the advancement of science. To conclude, in the Professor's own words:—"You may rest assured of this, that every addition to our knowledge of the brain will inevitably lead to a better appreciation, and more successful prevention and treatment of a large, and it is to be feared, rapidly-increasing class of distressing diseases of the brain and nervous system, which, even to those best acquainted with them, are still involved in profound obscurity."

W. L.

A. PLANET'S MOVEMENTS.—THE TIDES.

[30]—In looking over the first number of KNOWLEDGE, I find on page 13 the following—for an explanation of which I shall feel thankful.

1. "The planet Mars, it will be noticed, passes during this month what is called a stationary point; he is advancing (moving from right to left) till November 17th, after which he retrogrades. But he does not really come to apparent rest, owing to the wide sweep of the loop he forms between his stationary points. Jupiter, Saturn, and Neptune are all three retrograding." 1. Do the planets move in their orbits uniformly, or turn backwards and forwards, or how? 2. What is the cause for this, and is this retrograde motion performed during each revolution; if so, how often? 3. What am I to understand by "Mars not coming to apparent rest, owing to the wide sweep of the loop he forms between the stationary points?" What does this loop signify; and is there really such an occurrence as Mars or the other planets coming to a rest, &c.?

2. In Guillemin's "Heavens," page 391, in note by R. A. P., I find the following: "If we consider only the dynamical relations, we find that the place of low-water should be under the moon, and at the opposite part of the earth, the place of high-water between these regions." Will you kindly explain how the point of high-water ought not to be directly under the moon, but at a point at right angles to it?

ZETA.

[The movements referred to are only the apparent motions of the planet in the heavens. The planets constantly advance in their own orbits. I will take a later opportunity to answer "Zeta" more fully on both subjects.—ED.]

WHAT IS THE CAUSE OF GRAVITY?

[31]—I was glad to see the appearance of a paper like this, giving the views of the best men of to-day about the more interesting things in nature in plain language. As you invite correspondence, permit me to state that, although a toiler who cannot find much time for reading, I take, nevertheless, a very great interest in the study of nature. It gives me hope and affords me pleasure. And, as you say, it is certainly religious and improves our feelings. As regards myself, I take an especial interest in the law of gravity, not as to how, but as to its cause. I have read that Sir Isaac Newton could not tell. I should feel very grateful if you would give us the various theories that have been given out on this subject, no matter how silly they be, in as short a form as possible. On inquiry, the answer is always "Attraction"—a meaningless word. I have for many years observed every phenomenon resembling the action of gravity, and have come to conclusions as to what its cause is, that on careful comparison harmonise with all the well-known and undoubted fundamental laws of nature. I have read of experiments made and theories submitted that in no way come near the mark, but I may not have reached the right source yet. Experiments to prove gravity are difficult to make because we cannot exclude the earth's gravity from them even for a moment. But, by the help of reason, it can be made, and even clear to a mind that can comprehend other natural phenomena that can only be seen by the mind's eye. But, as soon as the idea has been grasped, numberless things come to view that resemble the earth's gravity on a smaller scale. "Attraction," as a word, is misleading, for to attract there must be a substance having these impossible qualities of pulling down every kind of matter in a vacuum of any substance.

However, to write my view on this would be months of labour and certain ruin. I merely wanted to suggest how interesting a subject this would be to many more.

Yours, &c., F. J. D., "SELBST."

[The subject is, doubtless, most interesting. It is, also, exceedingly difficult. Does "Selbst's" theory accord with the demonstrated fact that the action of gravity is communicated far more rapidly than light travels?—ED.]

PHRENOLOGY.—VECTORS.—INTONING IN SYNAGOGUES.

[32]—I don't know whether you will consider the following questions worth asking or answering in KNOWLEDGE.—

1. Assuming that phrenology is all wrong, *what are the causes* that determine the shape of the head? I am myself a sceptic, in the proper sense of the word: but Lewes' opinion, that the brain acts as a whole, and that its functions are not localised, seems to me, *à priori*, highly improbable. 2. Having lately begun to take great interest in mathematical physics, I read carefully Maxwell's little book on "Matter and Motion." The mode of treatment was almost entirely strange to me; and what I want to know is, whether the new method has any special value: whether any, or what peculiar benefits are gained by the use of vectors in mechanics, as distinguished from the old geometrical and analytical methods. In any case, Maxwell's treatise wants a good deal of expansion and illustration to make it intelligible to a tyro. 3. My last question, I am afraid, hardly falls within your province, unless you take in *omne scibile*; but it is one to which I have hitherto failed to get an answer. In reading the Scriptures in the Jewish synagogue, a certain intonation is employed. Is this expressed or expressible in the ordinary musical notation, or in any musical notation whatever, and if so, where could it be found?

G. P.

Queries.

[11]—Logarithms.—Will you kindly state the best cheap "Table of Logarithms and Antilogarithms," &c., suitable for a general science student, but more especially for one preparing for examination in "mine surveying," in connection with the City and Guilds of London? He would observe that he has seen a book of tables advertised at 2/-, and written by W. C. Unwin, and if you think that will be quite suitable, "yes" opposite the initials will be deemed a sufficient answer. The writer was intending to purchase the above book, but on reading your remarks on logarithms in No. 1 of Knowledge, determined to seek your advice.

James Grundy.

We use ourselves Chambers's "Tables" for ordinary work, and Hutton's when certain subsidiary tables are required. We have not used the work named above, but probably some of our readers can give Mr. Grundy the information he requires.—Ed.]

[12]—Is it possible for the same star to be both morning and evening star on the same day? Does not Tennyson's Hesper-Phosphor refer to the same star at different periods of its month?—R. A. Bullen. [To the first question, No; to the second, Yes, only for month should be written synodical period, meaning the period in which the planet goes through all its apparent movements with respect to the sun, from being in conjunction with him to its next return to that position.—Ed.]

[13]—Water Spheroids.—If a little water is spilled from a height of a few feet upon a surface of smooth water, a portion of the fluid, instead of mixing with the rest at once, is formed into small globes or spheroids which, I think, seldom if ever exceed ¼ inch in diameter, most of them being very much smaller. These little drops move over the water for distances varying from a few inches to a few feet, and then gradually though rapidly diminish and disappear.

In explanation of this very common phenomenon, I have been informed that a layer of air of greater density than the atmosphere is formed over the surface of the water, and that the part of this layer between the globules and the plain surface of water offers so much resistance that for a time the two bodies of fluid are unable to unite. It would be interesting to know what causes this denser layer of air, and why it is only for a time able to overcome the force of gravity acting upon the spheroids.—E. C. R.

[14]—Velocity of Sound.—I understand Tyndall to say that the increase of temperature in the condensed part of a sonorous wave augments its velocity one-sixth. Is the increase gained by the rise of temperature in the condensed portion not counterbalanced by the decrease of temperature in the rarefied part?—Sound.

[15]—The Missing Link.—What is the "missing link," of which we hear so much in connection with Mr. Darwin's books? Is it what should come between an ape and a man? An answer on this point would much oblige—An Ignoramus.

[16]—German and English.—Is there any work where I can find an account of the progress by which the English and German languages have come to differ so widely in character from each other? In good English writing we find the arrangement of subject, predicate, &c., quite different from that adopted in good German writing. Yet the languages had a cognate origin, and must once have resembled each other somewhat closely in those points in which they now differ. Strangely, too, while the German language has become in structure less simple than the English, the German words for abstract ideas, and their scientific terms, are much simpler than ours. When we speak of oxygen, a German speaks of sour-stuff; but where an English writer would say that a mixture of oxygen and hydrogen will explode, under such and such conditions, a German would say that sour-stuff and wasser-stuff, in certain proportions mixed, and under certain conditions so-and-so treated, explode shall.—Eclecticus.

[17]—Retrogradation of a Planet.—Which planet has the longest arc of retrogradation? And on what circumstances does the length of the arc depend?—Astronomicus.

[18]—Chemical Treatises.—Can any reader of Knowledge tell me what books will give me the best general idea of the so-called new chemistry?

Arctic Navigation.—Advices from Copenhagen state that the news received from the Dutch Polar Expedition on board the schooner *William Barents* is very unfavourable. Owing to the continuous ice barrier which extends nearly to Norway, Spitzbergen could not be reached, nor even the Bear Islands; and after one more attempt to force through northward, the expedition will return home, as the captain is convinced that this year Nova Zembla is completely enclosed in a barrier of ice.

Replies to Queries.

[1]—Ultima Thule.—The expression "Ultima Thule" occurs in Seneca's "Medea," Act. iii., verse 375:

—Venient anni-
Saecula series, quibus oceanus
Vincula rerum laxet, et ingens
Pateat tellus, Typhisque novos
Detegat orbes; nec sit terris
Ultima Thule.

The passage has been quoted to show that the ancients had heard of the New World, as our school books call the ancient continent of America. It seems, however, to prove rather the reverse, as showing that Thule (whether Thule were Iceland or one of the Orkneys, Shetlands, or Hebrides) was, in Seneca's time, regarded as the remotest-known region of the earth. X. Z. Z.

[1]—Ultima Thule.—Pytheas, a citizen of Massilia (Marseilles), previous to the time of Alexander the Great, undertook a voyage of discovery to the far north. The regions discovered by him were enveloped in fog and "chaos" and uninhabited. He designated the farthermost limit of his discovery Thule, whence "Ultima Thule." The actual identity of Thule is shrouded in mystery. Of Jutland, Shetland, Norway, and Iceland, each claims precedence as the Thule of Pytheas. Speaking offhand, I believe Pliny uses the expression "Ultima Thule." I cannot recollect the way in which he used the term. Some imagine Thule as a creation of the poets to express the extreme limit of the world.—W. G. Rolfe.

[1]—Ultima Thule.—If your correspondent is a classical scholar he may refer to Ptolemy, Tacitus, and others. Pliny describes it as "an island in the northern ocean discovered by Pytheas after sailing six days from the Orcades." I quote the following from Brewer:—"Called by Drayton, Thuly. Pliny, Solinus, and Mela take it for Iceland; others, like Camden, consider it to be Shetland, still called Thyleus-el (Isle of Thyle) by seamen, in which opinion they agree with Marinus and the description of Ptolemy and Tacitus." Bochart says it is a Syrian word, and that the Phœnician merchants who traded to the group called it Gezirat Thule (Isle of Darkness); but probably it is the Gothic Thule, meaning the "most remote land," and is connected with the Greek telos, the end.

Where the northern ocean in vast whirls,
Boils round the naked, melancholy isles of farthest Thulè.

Thomson, "Autumn."

Thule was the most northern part known to the ancient Romans.

Tibi serviat ultima Thule

Virgil, "Georgics" i., 30.

—R. T. Wright.

[3]—Flight of Birds.—If "Aërial" considers for a moment the very small proportion the bones of a bird bear to its bulk, and the still smaller cavities he speaks of, I think he will see that the heated air the latter contains can but very slightly assist the bird's flight, or rather buoyancy; besides which, the heat of a bird would only cause a slight expansion of the enclosed air. Is "Aërial" sure the bone cavities do contain air, and not some gas?—E. C. R.

[3]—Flight of Birds.—The flight of birds cannot be aided in any appreciable degree by the presence in the air-passages of the bones of air, lighter (because warmer) than the air in which the bird is flying. The actual supporting power obtained in this way can be easily shown to be utterly insignificant. It is simply the difference between the weight of the air in the passages and that of an equal volume of the outer air. This is less, of course, than the weight of a volume of air equal to the volume of the air-passages, and much less than the weight of a portion of air equal in volume to the bird, which would correspond to a very small fraction of the bird's actual weight, perhaps about one 500th part. The support obtained in this way cannot be at the utmost more than the 10,000th part of the bird's weight, and is not worth considering in dealing with the question of flight.—Fitful.

[4]—The Earth's Inclination.—In reply to "Moonstruck's" query, I should say that if the earth's axis was perpendicular to the plane of the Ecliptic, the weather at the Poles would be much colder than a Polar summer at present, because the rays of heat would fall very obliquely upon the earth surrounding the Poles. At the Equator the heat would be greater than at present, because the sun would daily appear to cross the zenith.—E. C. R.

[4]—The Earth's Inclination.—If the earth rotated on an upright axis, there would be perpetual spring or autumn all over the earth; but it must be remembered that spring and autumn at the equator are the hottest seasons of all, while at the poles they mean a sun just skirting the horizon, so that the coldest winter in temperate regions would not be so cold as this so-called polar

spring. To put the matter more definitely. If the earth rotated on an upright axis, the sun would everywhere, all the year round, rise exactly in the east (apart from the slight effects of atmospheric refraction) and set exactly in the west, attaining at midday an elevation equal to the complement of the latitude—90° at the equator (which would bring the sun overhead, 70° in latitude 20; 40° in latitude 50; 20° in latitude 70; and having no elevation at all in latitude 90, or at either pole.—STUDENT.

[8]—VOLUME OF SPHERE.—The simplest way of showing that the volume of a sphere is two-thirds that of the enclosing cylinder, is given in the following sketch, which, if "Archimedean" is anything of a geometrician, he will have no difficulty in filling in:—Show, first, that the surface of the sphere is equal to the *curved* surface of the enclosing cylinder, by taking any two planes close to each other through sphere and cylinder, parallel to the top and bottom of the latter, and showing that the part of spherical surface between these planes is equal to the part of cylindrical surface between them. This having been shown, imagine the surface of sphere, thus known, divided into indefinitely small areas, each of which may be regarded as the base of a pyramid, having the centre of the sphere as vertex—all these pyramids together giving the volume of sphere. Their combined volume is equal to that of a pyramid having a base equal in surface to the surface of the sphere (that is, of its enclosing cylinder, by what has been already shown), and a height equal to the radius of the sphere. This volume, by well-known property of pyramid, is represented by one-third the product of numbers representing the curved surface of cylinder and the radius of sphere. But curved surface of cylinder

= circumference of base × 2 radius of sphere

= circumference of a great circle × 2 radius of sphere.

Hence volume of sphere

$$= \frac{1}{3} \text{ radius} \times \text{circumference of a great circle} \times 2 \text{ radius}$$

$$= \frac{2}{3} (\text{radius})^2 \times 2\pi\ (\text{radius}) = \frac{4\pi r^3}{3} \text{ (if radius} = r).$$

But volume of enclosing cylinder = area of base × 2 height

$$= \pi r^2 \times 2r = 2\pi r^3.$$

Hence volume of sphere is two-thirds that of cylinder.

MATHEMATICUS.

EFFECTS OF LIGHTNING ON TREES NEAR A TELEGRAPH WIRE.—Some instructive facts in this connection have been brought to light by M. Montigny, in recent examination of poplars bordering part of a road in Belgium, between Rochefort and Dinant. The part in question is some 4,600 mètres in length, and runs westward; it is level for some distance, then rises gradually to a height of 61 mètres, through a wood, traverses a wooded plateau 200 mètres in extent, then descends, still through wood, to a plain. A telegraph wire runs near the row of Virginia poplars on the north side, and it appears that, out of nearly 500 poplars forming this row, 81, or a sixth, have been struck by lightning. Hardly any have been struck in the other row. The trunks have been mostly struck on their south side, and nearly opposite the wire. Comparing different portions of the road, it is found that in the horizontal part none of the (129) trees show injury from lightning, or at most only one (a doubtful case), but as the road rises through the wood, the cases quickly multiply, and on the wooded plateau as many as nine out of 14 trees, or 64 per cent., have been struck. On the slopes the proportion is 25 per cent. M. Montigny distinguishes three kinds of injuries—(1) the bark torn and detached on a limited part of the trunk; (2) a furrow, straight or (rarely) spiral, made on the tree, from near the wire, down to the ground; and (3) a peculiar oval wound, with longer axis vertical, and lips coloured light brown. Now, the furrows, which are probably due to the most violent discharges, are relatively most frequent on the plateau and on the western slope, which the storms usually reach first. M. Montigny is of opinion that the lightning, while attracted by the wire, does not strike this first, then the tree, but strikes the tree directly. His conception of the process is to the following effect:—Suppose a thunder-cloud charged with positive electricity. A long telegraph wire under it, though insulated, may acquire as great negative tension in the nearest part as if in direct communication with the ground, and the tension is greater the nearer to the cloud. While the inductive influence affects the wire most, near objects, such as trees, share in the influence according to their conducting power. The lightning, attracted in the direction of the wire, yet does not strike this, the insulating cups presenting an obstacle to its prompt and rapid escape. It finds a better conductor to earth in a neighbouring poplar, wet with rain. From the facts indicated it results, that of two similar houses, one built on a plain, the other in a wood, and having a telegraph wire fixed to them, the latter is the more liable to injury by lightning, and the danger is greater if the wood enclosing the house be upon an eminence.—*Times.*

Our Mathematical Column.

FROM the way in which logarithms are commonly spoken of, one would suppose that they were originally intended to perplex the student, instead of having been devised specially to assist him. It may be that this is due chiefly to the use of a name whose real meaning is not known, while, were its real meaning known, the use of numbers so named would still remain a mystery to all save mathematicians. The word logarithm is really intended to signify ratio-number. But hundreds who use logarithms, and thousands who would do well to use them, would not be one whit the wiser for knowing that there are tables of numbers which may be regarded as ratio-numbers. If a name had been given to logarithms which suggested to all something of their real use to computers, we should find them more valued and more commonly employed than they are. But unfortunately the singular idea that nothing but a long unintelligible name is worth anything in science—an idea about as worthy of respect as the Barbadoes mother's admiration for the name Chrononhotonthologos which Captain Marryat gave to her baby—has caused these useful tables to bear a ridiculous because un-English name.

In reality, a logarithm *is* a ratio number, though that says very little to most men of its use. The logarithms of the numbers 1, 2, 3, 4, 5, &c., up to 100,000 in our books, are in reality the powers to which 10 must be raised to give the numbers 1, 2, 3, 4, 5, &c., respectively. If we know the power to which 10 must be raised to give the number 13, and also the power to which 10 must be raised to give the number 17, we have only to add these powers together to obtain the power to which 10 must be raised to give the product of 13 by 17, or 221. If our tables give all the powers of numbers from 1 to 221, we need not actually multiply together 13 and 17 to get their product 221. All we need do is to add the logarithm of 13 to the logarithm of 17; the sum is the logarithm of 13 and 17 multiplied together. We look out in our tables the logarithm corresponding to the sum of the logarithms of 13 and 17, and we find that the number corresponding to this logarithm is 221.

Here, of course, we have not been saved a particle of labour. While we were looking out these logarithms, a charity boy could have multiplied 13 and 17 together half a-dozen times.

But, supposing we had occasion to multiply together the numbers 21,714, and 56,912, and to divide the product by 78,124 and 62,315; that is, to divide the product of the first pair of numbers by the product of the second pair of numbers. Then, if we know the powers to which 10 must be raised to give the above four numbers respectively, we can tell, by simple addition and subtraction, the power to which 10 must be raised to give the answer to our little sum; and if we know also what is the number which would result from raising 10 to the power just mentioned, this number is the answer we require. Suppose, for example, that $10^a = 21.714$; $10^b = 56.912$; $10^c = 78,124$; $10^d = 62,315$. Then we know by the properties of ratios that

$$21714 \times 56912 = 10^{a+b}$$

$$78124 \times 62315 = 10^{c+d}$$

while $$\frac{21714 \times 56912}{78124 \times 62315} = 10^{a+b-c-d}$$

Now a table of logarithms gives us for all numbers from 1 to 100,000 the powers corresponding to a, b, c, and d in the above example. (We can find such powers also easily enough from the tables for all numbers from 100,000 up to 10,000,000.) So that all the computer in the above case would have to do would be to look out the powers corresponding to a, b, c, and d, to add a to b and c to d, subtracting then the latter sum from the former. The result would be the power to which ten must be raised to give the answer to his sum; and he would only require to find that power in his tables to get the number he wanted. He would, probably, do all this in about the time that a first-rate computor would have got half way through the multiplying of 21,714 and 56,912 together.

This is, of course, only a general account of the use of a book of such powers, or of a book of logarithms. It should suffice to show that despite their hard name, they are worth understanding by all who have much computing to do; and there are few who would not do well, by studying a little the use of logarithms, to make themselves ready to employ the tables when occasion may arise.

Turn now to a few details.

The logarithms given in the tables are in reality the logarithms of the numbers 1, 2, 3, 4, 5, 6, 7, 8, 9; 1, 1·1, 1·2, 1·3, 1·4, 1·5, 1·6, 1·7, 1·8, 1·9; 2, 2·1, &c.; that is, however large the number in the lefthand column, it is understood to lie between 1 and 10, so that its logarithm lies in value between 0 and 1 (ten to the power *nought* gives 1, and ten to the power 1 is ten). Decimal points are not

shown in the tables but in reality they are implied. Thus, the tables give—

the logarithm of 3191 as 5039268;

this really means that

the logarithm of 3·191 is 0·5039268. (A.)

(The student should here turn to his tables and see for himself how the matter stands; he should also ask himself what statement A really means. On one side we have the number 3·191; on the other, a decimal fraction slightly exceeding ½, which we are told is the logarithm of 3·191. Now, remember that the logarithm of a number is that power to which 10 must be raised to give the number, and we see at once how A is to be interpreted. For 10 to power ½ is the same as the square root of 10, which we know to be rather greater than 3, so that in the equation

$$3 + \text{some small fraction} = \sqrt{10} \text{ or } 10^{\frac{1}{2}}$$

we have a rough approach, near enough to illustrate its meaning to the statement—

the logarithm of 3·191 = 0·5039268.

A few inquiries of this sort, with a book of logarithms in hand, will very soon give the intelligent reader a good idea of their meaning and use. Take one other case. We find from the tables that

the logarithm of 2 is ·3010300.

This means that 10 raised to the power $\frac{30103}{100000}$ is equal (very approximately) to 2. Now let us see whether taking the simple fraction $\frac{3}{10}$ we find 10 to the power $\frac{3}{10}$ approach in value to 2, though, of course, not quite so closely as would 10 to the power ·3010300. We know that $10^{\frac{3}{10}}$ is the same as the 10th root of 10^3 or of 1000; and we know that this is not very far from 2, for 2 raised to the tenth power gives 1024.)

But the tables are not the less complete that they only give the logarithms of such numbers as 3·191, 9·874156, and so forth. For, since shifting a decimal point one place to the right means multiplying by 10, while shifting the point one place to the left means dividing by 10, and since the logarithm of 10 is 1, we have only to add or subtract 1, 2, 3, 4, 5, &c., to the logarithm given in the table, to get the true logarithm of any number whatever within the limits ranged over by the table. Thus—

Logarithm of 3·191 = 0·5039268
Logarithm of 31·91, or of 10 × 3·191 = log. 10 × log. 3·191 = 1·5039268
Logarithm of 319·1, or of 10^2 × 3·191 = 2·5039268
Logarithm of 3191, or of 10^3 × 3·191 = 3·5039268

and so forth, the whole number in the logarithm being always 1 less than the number of digits in the integral part of the original number (that is, of digits on the left of the decimal point).

Again, logarithm of 0·3191, or of 3·191 ÷ 10 = 0·5039268 − 1, which for convenience and symmetry is written—

Logarithm 0·3191 = $\bar{1}$·5039268
Logarithm of 0·03191, or of 3·191 ÷ 10^2 = $\bar{2}$·5039268
Logarithm of 0·003191, or of 3·191 ÷ 10^3 = $\bar{3}$·5039268

and so forth, the whole number under the minus sign being always 1 more than the number of cyphers following the decimal point of the number.

It will be observed that in the case of numbers less than 1 we do not follow what might, at first sight, seem the obvious course, of subtracting the tabular number from 1, 2, 3, or whatever is the number under the *minus* sign, leaving the logarithm negative. Thus we write $\bar{1}$·5039268 short for −1 + 0·5039268; we do not write −0·4960732. The reason is that it is found convenient to have *all* the quantities on the right of the decimal point positive throughout all computations.

Mathematical Question.—Is there any solution of either or both the following sets of simultaneous equations?

$$\begin{array}{lcl} x^2 + yz = a^2 \text{ (i)} & & x^2 + xy + y^2 = a^2 \text{ (i)} \\ y^2 + zx = b^2 \text{ (ii)} & \text{and} & y^2 + yz + z^2 = b^2 \text{ (ii)} \\ z^2 + xy = c^2 \text{ (iii)} & & z^2 + zx + x^2 = c^2 \text{ (iii).} \end{array}$$

A. N. J.

We know of no solution to the former set; that is, it cannot be made, so far as we know, to produce a quadratic equation. The second set may be solved thus ("A. N. J." will probably only need to be shown the steps):—

Subtracting (ii) from (i) gives $(x + y + z)(x - z) = a^2 - b^2$ (iv).

Subtracting the sum of the squares of (i) (ii) and (iii) from twice the sum of the products (i) × (ii), (ii) × (iii), and (iii) × (ii), we obtain

$$xy + yz + xz = \sqrt{\tfrac{1}{3}[2a^2b^2 + 2b^2c^2 + c^2a^2 - a^4 - b^4 = c^4}$$
$$= p^2 \text{ (say) (v)}$$

adding (i) (ii) (iii) and 3 (v) we get

$$x + y + z = \sqrt{\tfrac{1}{2}(a^2 + b^2 + c^2 + 3p^2} = m \text{ (say) (vi)}$$

From (iv) and (vi) $m(x - z) = a^2 - b^2$,

and similarly, $m(y - x) = b^2 - c^2$

whence, subtracting, $m(y + z - 2x) = 2b^2 - a^2 - c^2$
$= m(m - 3x) = 2b^2 - a^2 - c^2$

or $x = \frac{1}{3m}(m^2 + a^2 + c^2 - 2b^2)$

By symmetry $y = \frac{1}{3m}(m^2 + b^2 + a^2 - 2c^2)$; $z = \frac{1}{3m}(m^2 + a^2 + b^2 - 2a^2)$.

Our Whist Column.

By "Five of Clubs."

We give this week a simple whist game, showing the inferences which can be drawn from the play by one of the players (the leader), and making notes also on the play as it proceeds. The inferences are all of the simplest kind, supposing the game to be conducted according to the accepted principles for sound play. This will appear as we proceed, in later papers, to develope these principles.

The Hands.

Diamonds—Q, 9, 7, 2.
Spades—Q, Kn, 10, 3.
Hearts—Q, 6.
Clubs—Kg, 6, 4.

Diamonds—Kn, 10, 8, 6, 4.
Spades—8, 7, 4.
Hearts—Kg, 2.
Clubs—10, 5, 3.

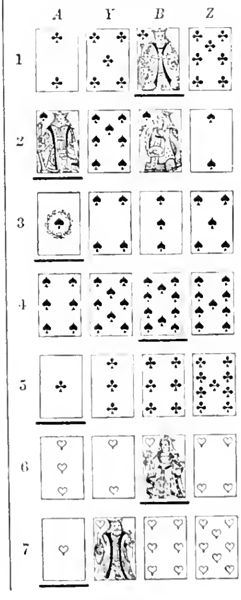
B
Y Dealer Z
Trump Card, Diamond King.
A

Diamonds—A, Kg.
Spades—9, 5, 2.
Hearts—Kn, 10, 9, 8, 4.
Clubs—Q, 9, 7.

Diamonds—5, 3.
Spades—A, Kg, 6.
Hearts—A, 7, 5, 3.
Clubs—A, Kn, 8, 2.

Score:—A B = 0; Y Z = 3.

Note.—The underlined card wins trick, and card below it leads next.



A's INFERENCES.

1. Either 3 C and 4 C are both with *B*, or else *Y* or *Z* is signalling for trumps. *B* has not the Queen.

Note to trick 1.—Having five trumps, one honour, and his partner having an honour, *Y* would be justified in signalling for trumps were the score low, but not as the score stands.

2. *B* has Kn S, 10 S, and probably one or more small Spades. *Z* is not signalling for trumps, and therefore has neither 3 C nor 4 C.

Note to trick 2.—*A* does well to take the trick and return the Ace, thus leaving *B* the command of the suit.

3. *Y* has signalled, and therefore has either 4 C or 3 C; the other being with *B*. As *Z* turned an honour, *Y* and *Z* are probably two by honours, in which case *A B* must make five by tricks to save the game.

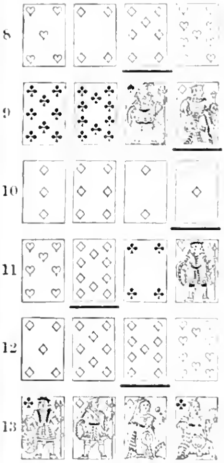
4. The last Spade is with *B* (the Knave).

5. *B* has 4 C, but no more Clubs.

Note to trick 5.—*B* returns the highest of two cards.

6. *Z* has not the King of Hearts; *B* has not the Knave (Hearts must be *Z's* best suit, trumps being *Y's*).

Note to trick 6.—Under the circumstances *Y* should have played the King. It is his best chance of getting a lead.



7. *Y* has the Knave of Hearts. The only chance of saving the game lies in the probability (*A* having four Hearts, and *Z* a long suit of Hearts) that *B* may be able to ruff Hearts next round.

8. If *Y* has played properly, all his remaining cards should be trumps, and the game is lost.

Note to trick 8.—If *Y* trumps at all he should play his highest. His small trump is useless. He should know that the best Heart is with *Z*.

9. *Z* has the Ace of trumps (Kn 8 being a thirteenth card).

Note to trick 9.—*Z's* play here is bad. He knows his partner has an honour, with a strong hand in trumps, and two tricks only are wanted to win the game, besides the two certain tricks *Z* holds in his hands.

10. If *B* has not Queen and 10 or 9 of trumps, the game is lost, for the remaining cards in *Y's* hand must be trumps (one honour at least).

11. *Z* should have led the Queen of Clubs. The Knave of Hearts is equally a winning card of its suit; but the play should have shown *Z* that *B* has a small Club, and that therefore *Y* can win trick 11 with his smallest trump. As it is, *Y* has to play highest third hand. *B* does rightly in not over-trumping. If he did, the remaining two tricks would be *Y's*, but by letting trick 11 pass, *B* remaining with the tenace in trumps (and knowing *Z* to be without trumps), has the two last tricks sure.

Our Chess Column.

Sir,—In your issue of the 11th inst. you gave an admirably-arranged perspective summary of the openings. In reference to the Evans Gambit declined, you say:—Declining the proffered Queen's Knight's pawn subjects the second player to a cramped defensive game. I beg to draw your attention to the fact that recent analytical researches have proved that by declining the Evans Gambit with B to Kt.3, Black is in no way at a disadvantage. The game may be declared even; and should White attempt to advance his Queen's pawns too vigorously Black would obtain the better game.—Mephisto.

Two Knights' Defence.

The game having opened on the line of the Two Knights' Defence, or thus—

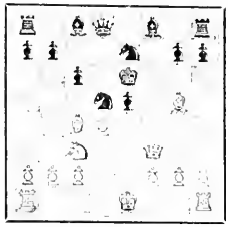
1. P. to Kt.4. / P. to K.4. 2. Kt. to K.B.3. / Kt. to Q.P.3. 3. B. to Q.B.4. / Kt. to K B.3.

White has two ways of continuing the attack, and one line of play which we recommend to the learner as based on the sound principle of developing and strengthening his own position, rather than rushing at his opponents. This last named line of play is simply 4. P. to Q.3., followed presently by Castles, B. to K.3. (or to K. Kt.5. if Black Castles early on King's side), Kt. to Q.B.3., and so forth; bringing all the pieces well into play before unduly pressing an attack. If Black plays a similar game, a steady but interesting and instructive game may be expected.

It seems, however, to be the generally-accepted view that first player has to attack and second player to defend; though theoretically, scarcely the slightest attacking power results from the first move, and what there is may be most readily lost. Accordingly, the usual way of continuing the Two Knights' game is either by 4. Kt. to K.Kt.5, or by 4. P. to Q.4. The latter is probably the sounder move. The former belongs to the class of premature attacks; though White wins a Pawn for the nonce if Black reply correctly. Black's best move in reply is 4. P. to Q.4, on which 5. P. takes P.

Black cannot now safely play 5. Kt. takes P., but we will follow out the interesting attack ensuing if he should play thus. There follows 6. Kt. takes K.B.P. / K. takes Kt. (Black must take the Knight, as it attacks both Queen and Rook), 7. Q. to K B.3 / K. to K. 3. (the only play by which Black can save the Knight), 8. Kt. to Q.B.3. / Q.Kt. to K.2 9. P. to Q. 4. / P. to Q.B.3 (Black cannot take the Q.P. without losing the Knight by 10. Q. to K 4 (ch), 10. B to K Kt.5. The position is then:—

Black.



White.

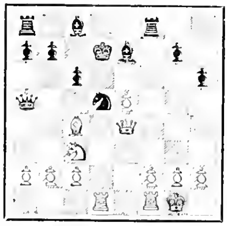
Position after White's tenth move.

It is obvious that White has now a very strong attack. If Black pushes his K. Rook's Pawn to attack the Bishop, White simply takes Knight with Bishop, and on Black's retaking (with Bishop, of course), White Castles on Queen's side with an overwhelming attack. If Black plays, instead, 10. P. takes Q.P., White Castles on Queen's side, with a crushing attack, whether Black take the Queen's Knight or not. If Black play 10. Q. to Q.R.4 White Castles on King's side, and after 11. ——— / P. to K.R.3 12. B. takes Kt. / B. takes B. there would probably follow—

13. Q.R. to Q.sq. / R. to K.B.sq. 14. Q. to K.4 / K. to Q.2 15. P. takes K.P. / ———

and we have the position—

Black.



White.

Position after White's 15th move.

Here White threatens to take the Knight either with Knight or Bishop, and if Pawn should retake to play R. takes P., winning Queen for Rook. If Queen move to Q.B.2, Q.Kt.3, or Q.Kt.5, or if King move to Q.B.2, White would take Knight with Bishop, and if Black then took Bishop with Pawn, White would win by Kt. takes P. (Note, that if Black King remains on Queen's file, or moves there from Q.B.2, he is exposed to a disclosed check by which his Queen's Rook would fall.) If Black plays away his King to K.'s square, White takes Knight with Knight, then, if Black retakes, White checks with Q. at K.Kt.'s 6th, winning the Rook (by B. takes P.), if Black interposes it, and the Queen (for Rook) if Black moves his K. on to Queen's file.

Returning to the position shown in the first figure, Black may play 10. P. to Q.Kt.4, on which White retreats his Bishop to Q.Kt.3, and if Black pushes his Q.Kt.'s P., White takes Kt. with Kt., and Castling on Queen's side obtains an overwhelming attack.

At his ninth move, Black might have played 9. P. to K.R.3. instead of 9. P. to Q.B.3; but 10. Castles, followed by 10. R to K. sq., gives White an exceedingly strong attack.

In fact, on this line, Black's game seems hopelessly compromised; though we believe Zukertort is of opinion that this still remains to be proved, and that precisely as the Muzio attack, formerly thought irresistible, is now shown to be unsound*, so the sacrifice of the

* Even this, in turn, may be questioned; at any rate, Winawer played the Muzio against Zukertort in the Berlin Chess Tournament, and made a drawn game of it.

Knight in this form of the Two Knights' Game may, after all, be unsafe.

It appears to us that Black may perhaps find safety from 10. P to Q K 4 11. B to Q Kt 3 / B to Q Kt 2 (starting from position shewn in first figure.)

Next week we shall consider what seems to be far a better course for Black than 4. P. takes Knight, namely 4. Kt. to Q R 4.

We shall be glad to answer any questions suggested by the opening moves so far considered.

The following end game from Ponziani is of the same class as those given last week.—

No. 3. Black.

WHITE.

White to play, and draw.

Answers to Correspondents.

**** All communications which have to receive early attention should be addressed to the Editor of* Knowledge, *74 and 75, Great Queen-street, London, W.C.*

Hints to Correspondents. 1. *No questions asking for scientific information can be answered through the post.* 2. *Letters sent to the Editor for correspondents cannot be forwarded; nor can the names or addresses of correspondents be given in answer to private inquiries.* 3. *No queries or replies savouring of the nature of advertisements can be inserted.* 4. *Letters, queries, and replies are inserted, unless contrary to Rule 3, free of charge.* 5. *Correspondents should write on one side only of the paper, and put drawings on a separate leaf.* 6. *Each letter, query, or reply should have a title, and in replying to letters or queries, reference should be made to the number of letter or query, the page on which it appears, and its title.*

W. H. Allen, J. W. Brookes, A. L. J., Perplexed, and others. The illusion must be a strong one, as it remains, though, by a misprint, incorrectly described in Mr. Foster's paper.—Cymro. Your mathematical query appears in mathematical section.—F. W. B. The work you mention is published by Messrs. Spon & Co., Charing-cross, who would inform you of the price. We do not know, and have no list of their works. We scarcely think "Anti-Guebre" would be satisfied by the account given of the sun in Huxley's Physiography; we mean that it would scarcely convince "Anti-Guebre" of the inaccuracy of his position. See Letter 5 in Number 2. The correct answer to the mountain-snow difficulty is not nearly so simple as many imagine. But of course "Anti-Guebre" is mistaken. A. B., Manchester. Thanks for the promised papers on Photography. We have no doubt they will prove very interesting to our readers.—J. K. M. We quite agree with you, but technical terms which appear without explanation in articles meant for general reading are decidedly objectionable.—Inquirer. (Your name is scarcely well chosen; there are so many inquirers.) He has made mistakes, but who that has been active in scientific research has not. He is, we think, *facile princeps* in his own field of labour, though we would not care to say so; comparisons (and the superlative is a form of comparison) are nearly always offensive.—Conicoid. Explain more fully your requirements. If you only want a general knowledge of the properties of the conic sections, Drew's "Geometrical Conic Sections," published by Macmillan, would serve your purpose admirably. But for thorough study of curves of the second degree, you would have to read more advanced treatises, which we will indicate, should you desire it.—Guebre. You will find the information you require in Lockyer's "Physics of the Sun" (Macmillan); but you will do well to read, as a more recent contribution to the subject, the excellent work on the sun, by Professor Young, Appleton & Co., New York.—Edw. Cl. Your recommendations duly noticed, and will be attended to at once. Thanks for the addresses.—A. W., Edinburgh. All exceedingly welcome.—A. C. R. Could not send any copies of No. 1; none at the office could be spared. Twenty-five copies of No. 2 were sent.—Francis Mazin. Your suggestion has, you will find, been attended to. The pagination will now be right throughout, as title-page will replace advertisement leaf.—Pyramidalist. We are not sure whether the interest in the Great Pyramid prophecies is so wide as you suggest; but you will find in a short paper by Mr Jos. Baxendell, the well-known astronomer, much that will interest you.—Dr. Jules Morel. Many thanks for your kind expressions of sympathy with our plan. The work you have sent will doubtless afford much interesting information to our readers on the subject of food. Cycloidal. If you really want to study the Geometry of Cycloids at great length (but is it worth while?) we can only name "Proctor's Geometry of Cycloids," because we know of no other work going over the same ground. But, be warned in time, and take a lighter subject. If "wilful will to water, wilful must wet;" though, by the way, this is the last thing you have to fear, for the book is dreadfully dry. Cycloidal geometry is not of overwhelming importance in scientific study. We should say (but mind, we do not know for certain) that scarce a hundred copies of the work just named have been sold.—Anti-Humbug. Your letter is quite unsuitable for publication in our columns. Its tone is absolutely contrary to that which we wish to maintain. If you contented yourself with the suggestion that those who differ from you are mistaken, we should still object to your letter; but your implied assertion that they *know* they are mistaken is intolerable. If you are so warm yourself against those whose opinions you oppose, consider, at any rate, that among them may be some as warm as yourself, whose feelings would be certainly hurt by your remarks and insinuations. In the words of one of the writers you mention, "Is life worth living?" if we are continually fighting?—Trunsiker Charles Keats. We do not object to the trouble, on which alone you dwell, so much as to the expense; the three works you wish us to send you cost together £6. Consider the effect of multiplied "orders" of that kind.—E. C. Articles of the kind sent are such as sub-editors make with the help of paste and scissors. We must not encroach on their sphere of labour, or, rather, on this portion of it. W. L. Thanks for the drawings; albeit we by no means accept either the two-headed man or the alligator boy. The so-called man-dog and his son were real enough, though not correctly described even as to their appearance.—G. W. Spence. See First Hint to Correspondents. Delaunay's discussion about the Inferiority of Women to Men was a communication to a learned society, and has not, we believe, been published in separate form. S. D. P. The razor-fish is not a fish, but a *Conchiferous Mollusc*, one of the *Solenaceans*, or *Dimyary Bivalves*. "I trust I make myself understood." Seriously, it is what is commonly, but incorrectly, called a shell-fish, the double shell somewhat resembling a razor.—Star-Gazer. See answers to "Perplexed" and others.—W. T. Lynn. Thanks for the correction respecting Palitzsch and Halley's Comet; it shall appear next week, if possible. The mistake is definitely made in Herschell's "Familiar Lectures," p. 114, though, as you say, not in the "Outlines."—Dr. H. Draper, New York. Many thanks.—G. W. D. Your letter is rather long, but shall appear, possibly abridged, as soon as possible.—G. A. Thanks for your interesting article on Buttercups; it shall appear next week.—J. M. A star atlas cannot include all desirable features; some must be sacrificed.—A. B. E. Thanks.—Thomas Witty, Hull. I scarcely think our readers would like to see any space devoted to the absurdities of the school you mention. It is hardly necessary for me to say that I repeatedly saw the sun south of the east and west horizon points when I was in Australasia. I have also repeatedly noticed that two and two make four.—R. M. See answer above to Mr. Mazin.—A. T. C. Yes. That point will be touched on in articles on Comets. A comet is not necessarily seen both when approaching and when receding, or even under either condition. The word unconformable, on p. 27, is not used in a geological sense, or, indeed, in a technical sense at all. It is put between inverted commas because borrowed (from Sir John Herschel); but every dictionary has it. The cribbage query is not suited to these columns; you will find that to make up numbers below nineteen the symmetrical arrangements which give large even numbers, as 6, 7, 7, 8, pairs royal, double pairs royal, are not so essential but that odd scores can be made. For scores above 18 you can only in one way get odd numbers, namely, by the "one for his nob," and this will not give 19, because the 18 which has to be made up to complete the score of 19 cannot be made up from a knave and four other cards; 20, 22, 24, 26, and 28 can. This, after all, shows only *how*, not why it is. The engraver, unfortunately, omitted one of the tails of the comet of 1744. Will consider your suggestion about covers.

E. L. R., I. C. H. W., Beta, W. A. R., J. A. W. O., J. A. W.=O., T. W. Shore, W. D. R., Charles Hamilton, △, G. C. W., G. G. D., J. McG. A., W. J. M., J. J. H. (B.A. Lond.), W. G. (Bird's-eye View), Arthur Black, H. S., &c. Letters received, and will appear or be answered in due course.

Note.—We have received many letters calling attention to the difficulty of obtaining copies of No. 1. The demand for copies was much larger than had been anticipated. The matter had to be entirely reset for a second edition, now publishing. Our publishers will be obliged if subscribers will call their attention to the fact, should they experience any difficulty in obtaining Knowledge at any of the railway book-stalls, or from their local newsagents. To ensure a supply of the back numbers, orders should be sent without delay.—Ed.

Contents of Knowledge *No. 1.*

Contents of Knowledge *No. 2.*

KNOWLEDGE
AN ILLUSTRATED
MAGAZINE OF SCIENCE
PLAINLY WORDED-EXACTLY DESCRIBED

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CONTENTS.

THE ORIGIN OF BUTTERCUPS.

By Grant Allen.

HERE, in my hand, I hold a solitary little golden buttercup, picked this morning in an autumn meadow, but still as bright and sturdy as though it had grown up in warmer days beneath the sunny skies of June. Common and familiar as it is, the buttercup is yet a very interesting flower from the point of view of its origin and evolution. Not that it is a highly-evolved or very singular blossom, with a long and intricate history at its back, like some of the orchids and snapdragons, whose complexity almost defies explanation; on the contrary, the importance of the buttercup in the eyes of the historical botanist is mainly due to the extreme simplicity of its typical arrangement. It is a very early type of plant, which has scarcely undergone any alteration from the form it must have acquired already many millions of years ago. There are other flowers of the same family, such as the larkspur, the columbine, and the monkshood, which still bear obvious traces of being derived from an ancestor exactly like the buttercup, but which have diverged widely from the original stock in their curious irregular flowers, sometimes spurred, sometimes hooded, and sometimes so altered from the primitive radial shape as to be scarcely recognisable. What makes our buttercup so interesting, on the other hand, is the fact that it represents an early stage in the history of these more highly-developed forms. In order to understand *them* we must first understand *it*. This buttercup, in short, is one of the most central members of the family to which it belongs; while some of its congeners have diverged in one direction and some in another, it has still kept unaltered for us the primitive lineaments of the common ancestor from which all alike have ultimately sprung.

Buttercups, as everybody knows, are tall meadow weeds, and the one which I hold in my hand belongs to the tallest species of all, which we know *par excellence* as *the* buttercup; for we have in England alone no less than some sixteen representatives of the entire genus. Let us look a little closely into its structure, and see what hints we can gather from its existing shape as to its past history and evolution.

First of all there are the leaves. These, one notices at once, are raised on long stalks, and deeply divided into several segments. Sometimes there are only three divisions to each leaf, sometimes five, and sometimes seven; the reason why they thus run in uneven numbers being, of course, that there is always a single terminal leaflet together, with one, two, or three lateral leaflets on either side of it. Again, each of these segments is itself further divided into three toothed lobes. Now, such a complex leaf as this shows by its very nature that it must be the product of considerable previous development. All very early leaves are quite simple and rounded; it is only by slow steps that a leaf thus gets broken up into many divided segments. In this respect, then, the meadow buttercup cannot be regarded as the simplest member of its class. There are some other buttercups, such as the ivy-leaved crowfoot, which creeps along the mud of ditches, or the lesser celandine, which springs in the meadows in early April, whose leaves are entire and undivided. In the lesser celandine they are almost circular, and in the ivy-leaved crowfoot they are slightly angular; but both these plants, having plenty of room to spread in the unoccupied fields of spring or the unappropriated ditches, have never felt the necessity for subdivision into minute segments. They have free access to the air and the sunlight, and so they can assimilate to their hearts' content the carbon of which their tissues are built up. It is otherwise, however, when similar plants push out into new situations, less fully supplied with carbonic acid or with sunshine. For example, there is the water-crowfoot, a mere divergent variety of the ivy-leaved species, which has taken to growing in ponds or rivers. Here it cannot obtain the materials for growth so readily as on its native mud-banks; and it has been compelled, accordingly, to split up its submerged leaves into long, thin, hair-like filaments; but when it reaches the surface, its foliage spreads out once more into the broad ancestral blades of the ivy-leaved crowfoot. It is just the same with the true buttercups. They have taken to growing in the open meadows, where the competition for vegetable food-stuffs is keen, and the struggle for existence very bitter. Hence they have been compelled to divide their leaves into many finger-like segments; and only those which have succeeded in doing so have managed to hold their own in the struggle, and so to hand down their peculiarities to future generations. As a rule, just in proportion as vegetation is thick and matted, do the plants of which it is composed tend to develope minutely divided and attenuated foliage.

It is the flower, however, that most people think of as the essential part of a buttercup, and it is by means of the flower that all the higher plants are usually classified. Now, the blossom of the buttercup is almost an ideally simple typical specimen. It consists of three parts or series of organs, from within outward. First comes a little central boss or cushion, supporting several carpels or unripe fruitlets. Each of these carpels contains a single embryo seed. Outside these comes a row of many stamens, which are the organs for producing the yellow dust which we call pollen. Now, no carpel can mature into a fruit containing ripe seed until it has been impregnated by pollen from a stamen, and these two sets of organs are, therefore, the only really essential parts of the whole flower. But in common language, what we mean by a flower is not these little central knobs and tassels, but rather the bright-coloured petals outside, which in the buttercup are five in number and golden yellow in colour. What, then, is the use to the plant of these expanded and very strikingly-coloured organs?

A flower is at bottom merely a device for producing seed. But in order that the seed may prove capable of germinat-

ing, the ovules in its carpels must necessarily be fertilised by pollen. Now, all the earliest flowers consisted merely of stamens and carpels; they had no petals at all. But, as Mr. Darwin has shown, flowers which are fertilised by pollen from a neighbouring plant produce more seed and healthier seedlings than those which are fertilised by the produce of their own stamens. Hence, any modification which promoted such cross-fertilisation would benefit the plants in which it occurred by giving them an advantage over their rivals in the struggle for existence. Now, there are two ways in which flowers have thus acquired special adaptations for fertilising one another. Some of them have developed hanging stamens which shake out their pollen to the wind, and such flowers are also provided with feathery collecting surfaces to the carpels, so as to catch the stray grains which may happen to be wafted to them from their neighbours by the breeze. Plants of this type never possess bright-coloured petals. A second class, on the other hand, have learned to utilise the winged insects which visit their blossoms in search of food. These welcome little pilferers, in passing from head to head, carry the pollen of one plant to the carpels of another, and so assist the flower in effecting the desired cross-fertilisation. This class, to which the buttercup belongs, has usually developed various inducements of food, scent, and colour, in order to attract the fertilising insects. Those flowers which best succeeded in alluring their little winged guests would naturally hold their own against all less highly endowed competitors, and would hand on their own constitution to their descendants. In this way insect-fertilised plants have acquired the bright petals and sweet scents which chiefly make them noticeable to our human senses. A brief examination of the buttercup blossom will show us the use which they subserve, and the way in which they act in the simplest forms of insect-fertilised flowers.

Pull out one of the golden petals from the outside of the head, and you will see at its base a small hollow spot, covered by a tiny concave scale. That spot is the nectary, and it contains a single drop of pure honey. The honey is put there to entice bees and other insects; it is the wage offered them by the plant in return for the service which they perform for it in fertilising its seeds. The golden hue of the petals, again, acts as an advertisement for the honey; the bees know that such bright hues are never found in any flowers except those which specially lay themselves out to bid for their favour. When a bee sees the brilliant colour, he flies straight towards the blossom and settles on the little boss of carpels in the centre. Here he sips the honey for his own behoof, and at the same time dusts himself with pollen on behalf of the flower; for, as soon as the blossom opens, the stamens discharge their precious burden, beginning from the outside and slowly ripening towards the centre. At this early stage, however, the carpels are not yet mature for impregnation, and so they avoid being fertilised from the pollen of their own stamens. If the bee flies away to another buttercup which happens to be still in the same stage of development, he only collects more pollen about his head and thighs; but if he alights on a somewhat older buttercup, he finds its stamens withered and its carpels fully mature for impregnation. Some of the pollen is then sure to fall on the sensitive surface of the carpels. Thus, while he seeks honey for himself, he unconsciously affords his host all the advantages of cross-fertilisation; and it is because he does so that the flower has been enabled to develope its complicated arrangement of petals and nectaries for his delectation.

The buttercup, then, with its five separate simple petals, its many stamens, and its central one-seeded carpels, may be regarded as a good example of the earliest type of insect-fertilised flowers. In some other plants, such as the harebell and the primrose, the separate petals have coalesced into a single tubular corolla; while in others, again, they have assumed various fantastic shapes; but all of them are ultimately derived from flowers like the buttercup, which thus contains in itself all the essential elements of a perfect insect-fertilised plant.

SOLIDS, LIQUIDS, AND GASES.

By W. Mattieu Williams.

PART II.

MULTITUDES of examples may be cited illustrating the viscosity of bodies that we usually regard as types of solidity, such, for example, as the rocks forming the earth's crust. In the "Black Country" of South Staffordshire, which is undermined by the great ten-yard coal-seam, cottages, chimney-shafts, and other buildings may be seen leaning over most grotesquely, houses split down the middle by the subsidence or inclination of one side, great hollows in fields or across roads that were once flat, and a variety of other distortions, due to the sinking of the gradual rock-strata that have been undermined by the colliery workings. In some cases the rocks are split, but usually the subsidence is a gradual bending or flowing down of the rocks to fill up the vacuity, as water fills a hollow or "finds its own level."

I have seen many cases of the downward curvature of the roof of a coal-pit, and have been told that in some cases the surrounding pressure causes the floor to curve upwards, but have not seen this.

As Knowledge will doubtless have many readers in the colliery districts, some of them may be able to supply reliable evidence of this, accompanied with careful measurement of the amount of upward curvature or upheaving in a stated breadth of road or working.

Earthquakes afford another example. The so-called solid crust of the earth is upheaved and cast into positive billows that wave away on all sides from the centre of disturbance. The earth-billows of the great Lisbon earthquake of 1755 travelled to this country, and when they reached Loch Lomond, were still of sufficient magnitude to raise and lower its banks through a perpendicular range of two feet four inches.

It is quite possible, or, I may say, probable, that there are tides of the earth as well as of the waters, and the subject has occupied much attention and raised some discussion among mathematicians. If the earth has a fluid centre, and only a comparatively thin crust, as some suppose, there must be such tides, produced by the gravitation of the moon and the sun.

Ice presents some interesting results of this viscosity. At a certain height, varying with latitude, aspect, &c., we reach "the snow line" of mountain slopes, above which the snow of winter remains unmelted during summer, and, in most cases, goes on accumulating. It soon loses its flocculent, flakey character, and becomes coherent, clear blue ice by the pressure of its own weight.

A rather complex theory has been propounded to explain this change—the theory of *regelation*—*i.e.*, refreezing, a theory which assumes that the pressure between the surfaces first thaws a film of ice at the surfaces of contact, and that presently this refreezes, and thus effects a heating or general solidification. Faraday found that two pieces of ice with moistened surfaces united if pressed together

when at just about the temperature of freezing, but not if much colder. Tyndall has further illustrated this by taking fragments of ice and squeezing them in a mould, whereby they became a clear, transparent ball, or cake, and schoolboys did the like long before when snowballing with snow at about the thawing point. Such snow, as we all remember, became converted into stony lumps when firmly pressed together. We also remember that in much colder weather no such cohesion occurred, but our snowballs remained powdery in spite of all our squeezing.

I am a sceptic as regards regelation. I believe that the true explanation is much simpler: that the crystals of snow or fragments of ice in these experiments are simply welded, as the smith unites two pieces of iron, by merely pressing them together when they are near their melting point. Other metals and other fusible substances may be similarly welded, provided they soften or become sufficiently viscous before fusing. Platinum is a good example of this. It is infusible in ordinary furnaces, but becomes pasty before melting, and, therefore, one method adopted in the manufacture of platinum ingots or bars from the ore, is to precipitate a sort of platinum snow (spongy platinum) from its solution in acid, and then compress this metallic snow in red hot steel moulds by means of pistons driven with great force. The flocculent metal thus becomes a solid, coherent mass, just as the flocculent ice became coherent ice in Tyndall's experiment or in making hard snowballs. Wax, pitch, resin, and all other solids that fuse, *gradually* cohere, are weldable, or, in very plain language, "stick together" when near their fusing point.

I have made the following experiment to prove that when this so-called regelation of snow or ice fragments occurs, the ice is viscous or plastic, like wax or pitch. A strong iron squirt, with a cylindrical bore of half-an-inch in diameter, is fitted with an iron piston. This piston is driven forth by a screw working in a collar at one end of the squirt. Into the other end is screwed a brass nozzle with an aperture about one-twentieth of an inch diameter, tapering or opening inwards gradually to the half-inch bore.

Into this bore I place snow or fragments of ice, then holding the body of the squirt firmly in a vice, I work the lever of the screw, and thus drive forward the piston and crush down the snow or ice fragments, which presently become coherent and form a half-inch solid cylinder of clear ice. Applying still more pressure, this cylinder is forced like a liquid through the small orifice of the nozzle of the squirt, and jets or sprouts out as a thin stick of ice like vermicelli, or the "leads" of ever-pointed pencils, for the moulding of which the squirt was originally constructed.

I find that ice at 32° can thus be squirted more easily than bees'-wax of the same temperature, and such being the case, I see no reason for imagining any complex operation of regelation in the case of the ice, but merely regard the adhesion of two pieces of ice when pressed together as similar to the sticking together of two pieces of cobblers'-wax, or softened sealing-wax, or bees'-wax, or the welding of iron or glass when heated to their welding temperatures, *i.e.*, to a certain degree of incipient fluidity or viscosity.

If a leaden bullet be cut in half, and the two fresh-cut faces pressed forcibly together, they cohere at ordinary atmospheric temperatures, but we have no occasion for regelation here. The viscosity of the lead accounts for all. At Woolwich Arsenal there is a monster squirt, similar to my little one. This is charged with lead, and, by means of hydraulic pressure, the lead is squirted out of the nozzle as a cylindrical jet of any required diameter. This jet or stick of lead is the material of which the elongated cylindrical rifle bullets are now made.

But returning to the point at which we started, on the subject of ice, viz., its Alpine accumulation above the snow-line. If the snow fall there exceed the amount that is thawed and evaporated, it must either go on growing upward until it reaches the highest cloud, or atmospheric region, from which it falls, or is formed, or it must descend somehow.

If ice can be squirted through a syringe by mere hand-pressure, we are justified in expecting that it would be forced down a hill slope, or through a gully, or across a plain, by the pressure of its own weight when the accumulation is great. Such is the case, and thus are glaciers formed.

They are, strictly speaking, rivers or torrents of ice; they flow as liquid water does, and down the same channels as would carry the liquid surface drainage of the hills, were rain to take the place of snow. Like rivers, they flow with varying speed, according to the slope; like rivers, their current is more rapid in the middle than the sides; like rivers, they exert their greatest tearing force when squeezed through narrow gullies; and, like rivers, they spread out into lakes when they come upon an open basin-like valley, with narrow outlet.

The *Justedalsbrae* of Norway is a great ice-lake of this character, covering a surface of about 500 square miles, and pouring down its ice-torrents on every side, wherever there is a notch or valley descending from the table-land it covers. The rate of flow of such downpouring glaciers varies from two or three inches to as many feet per day, and they present magnificent examples of the actual fluidity or viscosity of an apparently solid mass. This viscosity has been disputed, and attempts have been made to otherwise explain the motion of glaciers, but while it is possible that it may be assisted by varying expansion and contraction, the downflow due to viscosity is now recognised as unquestionably the main factor of glacier motion.

Cascades of ice may be sometimes seen. In the course of my first visit to Norway, I wandered alone over a very desolate mountain region towards the head of the Justedal, and unexpectedly came upon a gloomy lake, the Styggevand, which lies at the foot of a precipice-boundary of the great ice-field above-named. Here, the ice having no sloping valley-trough by which to descend, poured over the edge of the precipice as a great overhanging sheet or cornice, which bent down as it was pushed forward, and presented on the convex side of the sheet some fine blue cracks, or "crevasses" as they are called. These gradually widened and deepened, until the overhanging mass broke off and fell into the lake, on the surface of which I saw the result, in the form of several floating icebergs that had previously fallen.

Something like this on a small scale may be seen at home on the edge of a house roof, on which there has been an accumulation of snow; but, in this case, it is rather sliding than flowing that has made the cornice; but its *down-bending* is a result of viscosity.

GERMS OF DISEASE AND DEATH.

By Dr. Andrew Wilson, F.R.S.E.

Most readers have heard of the "Germ theory," and there are few persons who do not know what the hypothesis of that name means and implies. Popularly regarded, this theory holds that a very large proportion of the diseases that affect and afflict man and his neighbour animals, owe their origin to minute forms of life—whether animal or vegetable, or both, is still, in most cases, a

matter of doubt. To select a single illustration of the application of this theory, we may take the case of small-pox, and its analogous condition, the fever produced by vaccination. When an infant is vaccinated, the physician introduces into its system, through an abrasion of its skin, a minute quantity of vaccine lymph, which, as everybody knows, is obtained either from the vaccination pustule of an already vaccinated child, or direct from the calf. In either case, there are introduced into the infant body, certain minute germs—suspended in and living naturally amongst the vaccine lymph—and in due course these germs multiply and increase within the frame, thereby producing the characteristic fever, and the equally characteristic pustule at the seat of the operation. So, also, with small pox, which vaccination imitates in a mild way, and of which, moreover, it is a preventive. Here the germs of small pox, obtained directly or indirectly from an already infected person, attack the body. Gaining admittance thereto, they propagate themselves within the tissues and through the medium of the blood. Sooner or later all the characteristic symptoms of the disease are manifested, and having run its course, it dies away as mysteriously, to all appearance, as it came. Now, there is something strikingly analogous in all this to the growth of an animal or plant. There is a period of "incubation" in the fever, just as in the production of the living being there is a period of development. There is a growth of the fever, as the animal or plant grows towards its maturity; and there is a decline of the disease, as the living form passes to its old age and death. So far, then, the parallel between ordinary life and the birth, growth, and decline of a disease, is very close and clear.

But the analogies are not yet exhausted. Each fever produces its like, as do animals and plants. Each disease reproduces its kind, as Tyndall has somewhere observed,* as rigorously as dog and cat reproduce their like. The phenomena, or, as a doctor would call them, the "symptoms," of each disease are, as a rule, highly distinctive. The symptoms of scarlatina are not those of small-pox; measles is different from the other two; whilst typhus fever is again thoroughly different from all three. Analogy may, as Darwin says, be a deceitful guide; but when the facts are so closely allied, as are the facts of epidemic diseases to those of animal and plant development, the use of analogy cannot be doubted in rendering the relationship clearer.

We are now in a position to understand more clearly the utility and strength of the germ theory in certain of those aspects which bear most materially on science at large. It would only serve to strengthen the idea that our epidemic diseases are simply the offspring of lower life, if we reflect in passing that there are known to science a very considerable number of lower plants which produce in man's skin effects and diseases as characteristic as those which a fever induces in his system at large. Thus, the disease known as "ringworm" is caused by the growth in the human skin of a parasitic fungus, and a whole series of skin affections is known wherein lower plants play the part of direct causes. Thus, if it is a matter of certainty that a particular skin-disease is caused by lower plant-growth, so no less is it by analogy likely that all other contagious and epidemic diseases are in reality the products of life.

So much for the general idea that permeates the "germ-theory" of disease. Within the past few months some highly important additions have been made to our knowledge of the part played by lower organisms in the production of disease. M. Pasteur, whose researches into the development of lower organisms have placed him in the foremost rank of scientific workers, has detailed at length the results of his investigations into the causes which produce the curious disease known as *charbon*, *anthrax*, and *splenic fever*. This disease, whilst but rarely attacking man, is fatal to horses, cattle, and sheep. France suffers greatly from this "plague of boils," and it is also known in various other countries as a literal scourge. Pasteur, it should be mentioned, had already acquired much valuable experience in the investigation into the cause of the *pébrine*, or silkworm disease, which in 1863 had devastated the silk industry of France. Pasteur showed that *pébrine* was caused by the growth and multiplication, within the bodies of the insects, of minute "corpuscles," which were practically lower forms of life. Even the eggs from which the worms were hatched were shown to be liable to infection from *pébrine*; the eggs, in such a case, inheriting the disease from the parent moth which laid them. As the result of a long and laborious series of experiments, Pasteur showed that the *pébrine* would spread like an infectious disease by the contact of whole with diseased worms. He showed that, just as man isolates his fever patients, so the French silk-grower had to isolate and separate his diseased worms. But the knowledge which led to this effectual result was knowledge that had been won by an uphill fight, and that had been gained by the object-glass of the microscope, and by the whole-souled devotion of many months' industry.

* Quoting a remark by Miss Nightingale.—Ed.

THE LAWS OF PROBABILITY.

There are few subjects in which men take a more general interest, yet few in which they make greater mistakes, than the subject of probabilities. From the man whose mind is most perfectly trained in the analysis of evidence, down to him whose thinking apparatus can scarcely be regarded as a mind at all, all men endeavour to guide their conduct in matters uncertain, or at least to form their opinion on such matters, according to the probabilities. They would use different words in describing their purpose. A mathematician might, perhaps, speak definitely of the *à priori* and *à posteriori* probabilities in favour of an opinion or of a course of conduct. Wendell Holmes' "lout," he "who lies outstretched on a tavern bench with just mental activity enough to keep his pipe from going out," would hardly use the same expressions; but in his imperfect way he is all his life doing the same thing. He does not even stretch out his ungainly limbs on one bench rather than on another, or in one attitude rather than another, without to some degree considering his chance of comfort or security. But in all the ordinary relations of life he more definitely weighs the chances, though often, if not always, in an utterly inexact balance.

It is not the loutish mind only, however, nor the average intellect, which inexactly estimates probabilities. The most profound knowledge of the mathematics of chance does not save men from error. Often, indeed, the grossest blunders have resulted from an attempt to weigh ordinary matters in a mathematical balance. The fault has not lain, however, with the balance, but with the user. If a man chooses to weigh groceries in a chemical balance, paying no attention to the fact that either the parcels he weighs come in contact with other things lying round, so that the balance cannot possibly show their true weight, or else, perhaps, that the construction of the balance is such as will only permit of its indicating true results within certain very

narrow limits of weight, he will obtain a much less satisfactory result than he would if he used a common weighing machine incapable of showing differences of a few grains or scruples. Yet he should not condemn the balance, but himself for misusing it.

Not unfrequently mathematicians have fallen into a parallel error. Indeed, there are those who assert that mathematicians are more apt than other men to err in this way, because they try to apply the exact laws of probability to matters about which they only have inexact knowledge. To use an illustration of Huxley's, mathematicians possess a mill of exquisite perfection, which grinds you stuff of any degree of fineness, but they are apt to put peacods into it and to regard the product of their grinding as wheaten flour.

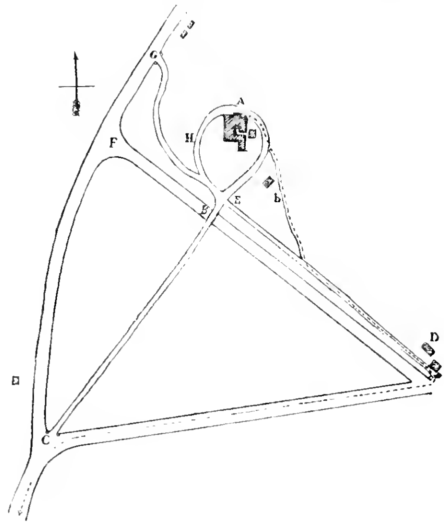
However, there is no real connection between profound mathematical research and utter absence of common sense. Mathematicians do not often err in this way. The laws of probability would be worth knowing even if they did. Nor is the application of these laws to many cases where they really are applicable likely to detract from what is commonly called the common sense way of viewing matters. Indeed, the common sense method may be regarded as only a rough way of applying the just method. The probability of its leading to a tolerably correct result is much less than the probability of a correct result when due account is taken of details; just as the chance of correctly estimating the cost of a complicated work by roughly adding a number of roughly-estimated items is less than the chance of a correct estimate when these items, separately estimated as exactly as possible, are properly added together. In neither case do we get an exact result, but the chances of gross error are much greater in the former than in the latter; for to the chances of error arising from an exact estimate of the several items in the latter case there is added the chance of a large error in a rough estimate of the totality of these items.

It must be admitted that by justly combining the various probabilities on which a question depends, we are more likely to attain a correct opinion than if we formed only a rough idea of what would result from such a combination. Yet the incautious application of the laws of probability, combined with undue reliance on results which, at the most, had been only shown to be probably true, has often led to absurd blunders.

INTELLIGENCE IN ANIMALS.

PASSING over several cases which seem to carry the matter no further than those already cited, we come next to a case which appears to us one of the most striking ever recorded. The writer, Mr. E. H. Pringle, remarks that it is an instance of sagacity which finally set at rest any doubts he had ever entertained that the difference between human and animal intelligence is one of degree only. We can see no way in which the story can be explained without assuming the exercise of something more than that mere practical reasoning which probably underlies all the so-called instinctive actions of animals:—

Mr. J. W. Cherry, of the Madras Forest Service, is owner of a dog, a bull-terrier, called "Bully." (This breed is notoriously not the most intelligent of the canine tribe, so that the behaviour of Bully appears all the more striking.) "We lived," says Mr. Pringle, "in the bungalow A,* the compound of which was bounded south and west by public roads DC and GFC, both leading to the cantonment of Bangalore in the direction C. There were three gates into the compound at C, D, and G, the main approach to the bungalow leading over a bridge B that spanned a public road FD. The compound was filled with trees and shrubs, and bordered by dense lantana hedges, so that with the exception of a portion of the western road at F, neither of the cantonment roads were visible from the bridge, nor could the footpaths be seen thence. Now, Bully had a lady friend (canine) living in the cantonment, and at times she was so attractive, that absences without leave on the part of the dog were frequent. After one of these excursions, Bully had been brought back and chained up for the night. Next morning, while his master and I were sitting at early breakfast, it was decided that he should be released, and to effectually



stop further delinquency, a peon was sent down to the bridge with orders to intercept him if he started for the cantonment. Bully was brought in and unchained; he had that unmistakable air of detected guilt deservedly punished, and spent some time in begging for scraps from the table in a most deprecatory manner. Shortly, however, he strolled into the verandah, and then down the front steps on to the gravel walk. After wandering about aimlessly for a few minutes, he quietly started down the approach AHB. We followed, keeping out of his sight. At the turn of the road, Bully met with the unexpected apparition of the peon standing on the bridge. In a moment, though not a word was spoken by the man, the dog turned and came straight back to the room, whither we had in the meantime slipped back unobserved, and re-entered it, wagging his tail violently, and looking exceedingly sheepish. He now lay down, and closed his eyes. The cocked ears showed that sleep was mere pretence, and he soon rose again, went out into the front garden, and hunted for buried bones—purely imaginary ones, I believe. His search gradually led him down the hill by a footpath leading from A to G, we keeping him in sight, as before, and he finally reached the road at the bottom. There all disguise was dropped, and he started off for the cantonment. As he neared the spot F, the peon

* The figure presents all the features essential to a correct understanding of Mr. Pringle's narrative.

espied him, and shouted out his name. He turned at once, climbed the hill, and came into the bungalow, where the same farce of repentance was gone through. Bully now seemed to have made up his mind that escape was impossible; he lay down on a mat in the verandah, and remained there for a long time. But for the persistent cock of the ears, we should have imagined the animal really asleep. Mr Cherry eventually went to his office-room, and I remained in the verandah, reading the morning paper and occasionally glancing at Bully. He lay very still, but once or twice I detected him opening his eyes and raising his head to look round him. Each time he caught my eye he wagged his tail vehemently for a moment or two, and then resorted to his sham sleep. It may have been for half-an-hour or thereabouts that this state of things continued. I then became interested in an article in the paper, and when I next looked up, Bully was gone. I called Mr. Cherry, and the house was searched for Bully. The peon was sent for and interrogated; he had not seen the dog. As a last resource, inquiry was made of the horse-keepers down at the stables, D. The reply was "Yes, the dog had passed through the gate, D, some time before." Taking advantage of my occupation, and the absence of his master, Bully had left the house and taken his way to the cantonment by the only path by which he could have escaped unnoticed by the peon—that shown by the dotted line.

It seems to us quite impossible to account for the dog's action, as above narrated, without attributing to him the exercise of reasoning powers, not merely in selecting the route by which he finally escaped, but in the manœuvres by which he endeavoured to assure those who were watching him that he had given up all hope of escaping. Doubtless, if he had reasoned more perfectly, he would even have allowed his ears to seem asleep, instead of leaving them cocked. But very few of us human beings simulate sleep without some such error, by which any observant person would be enabled to detect the trick. Either the muscles of the face are not perfectly relaxed; or the hands or feet are left in a constrained attitude; or the position of the body generally is unlike that which a sleeping person would assume; or else the breathing is unnaturally restrained. And again, though Bully was too demonstrative in his contrition, and afterwards in his attempts to hide the consciousness of failure, it was not for want of reasoning power. Few of us know how to act such parts as he tried to play, with perfect correctness; nor do those who know succeed always in acting such parts as they could wish. Probably Bully was as sensible as the onlookers that he was not quite successful in his acting. It is obvious, however, that he directed his efforts as carefully to the end he wished to obtain as a human being of average reasoning powers and skill in counterfeiting sleep, &c., could have done.

ILLUSIONS.

By Thomas Foster.

In my last paper there were two mistakes, or rather a single mistake, for it amounted really to the inversion of one of the figures. In Fig. 5, the lines marked AB, CD, should have been put opposite the letters EF, GH, and *vice versâ*. As they actually stand, the description should run as follows: The lines AB, CD appear to be curved, so as to be farther apart at the middle than at either end; whilst the lines EF and GH, in the same figure, appear to be so curved as to be nearer in the middle than at either end.

Such illusions as are illustrated at pp. 11 and 12 vary in effect according to the position from which the illusory drawing is seen. Thus, if we so place Fig. 1 that the eye looks along, or nearly along, the direction AB, we see at once that CD is in the same straight line with AB. Again, although we cannot look simultaneously in the directions BC and ED in Fig. 3, it will be found that when we look along BC, so that the illusion ceases so far as the part of the circle near C is concerned, the illusion ceases altogether, the part near D no longer appearing to form part of a smaller circle. In Fig. 4, the illusion is not at all effective when we hold the lines AB, CD, &c., upright, but is very marked when they are looked at aslant. But again, even when we look at them aslant, and the illusion is strong, we yet find that running the eye backwards and forwards along the parallels, the illusion varies all the time; for the eye cannot fail then to perceive that the distance between the parallels does not vary as the illusory effect seems to suggest. Thus, the lines CD, EF, which, when the eye is at rest, seem to draw closer at D and F, are found not to do so when the eye is carried from CE to DF; and a singular effect of motion is produced, the lines CD and EF seeming to move apart as the eye runs down them towards DF. The other lines seem to move correspondingly, and thus we have an illusion of motion, which will be considered later among illusions of that sort.

In the illusory figures now to be considered, the lines which appear curved, though straight, are set in different positions so as to be differently affected, and thus we have illusions affecting the shape of enclosed figures such as shall be considered separately further on.

If we look at Fig. 8 as it stands, that is with the point A uppermost, we find the lines AB, AE looking strongly curved; BC and ED rather less curved; and DC looks almost straight—at least, this is the effect to my eye. (I find EA most curved of all.) Thus the figure, which is

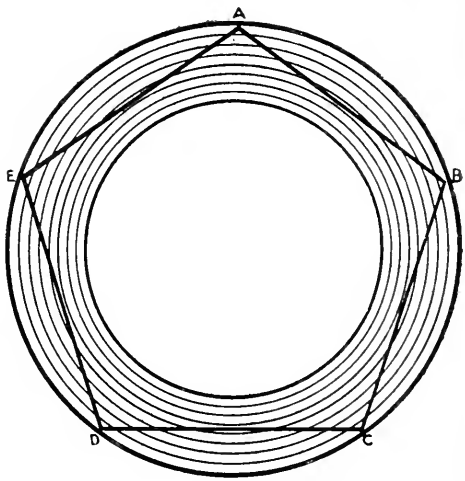


Fig. 8.

really a true pentagon with straight sides, looks like a figure having unequally curved sides. Turning the picture round in its own plane, we find that as the sides of the pentagon by this turning motion pass to the top, they appear more curved, so that they appear to change in shape as they move, or to have a motion of their own.

The apparent curvature of the really straight lines in

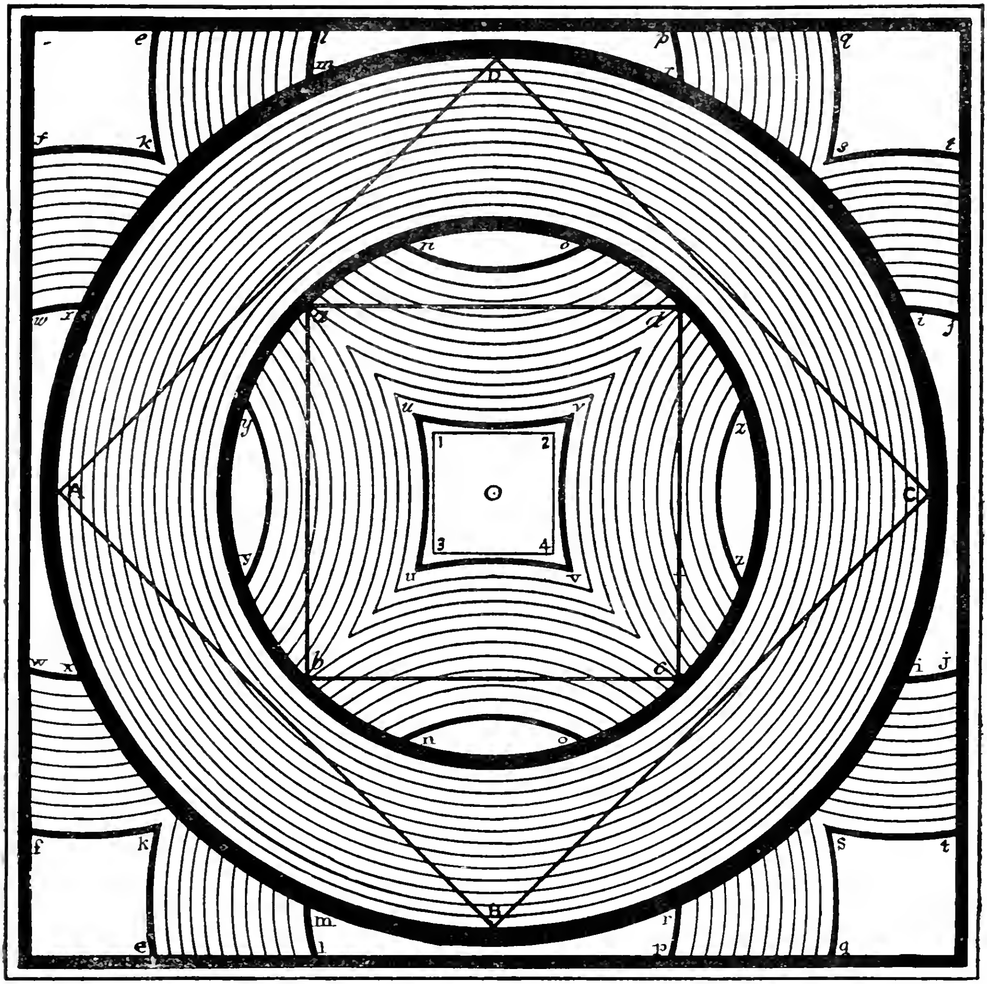


Fig. 9.—A Nest of Illusions.

Fig. 8 has been regarded by Mr. Proctor as having possibly some bearing on the irregular shape, which the outline of the planet sometimes seems to have. It is referred to by Mr. Webb in his "Celestial Objects for Common Telescopes," who illustrates it, however, by a figure in which the distinctive characteristic is lost; the straight line in his figure does not cross the curved lines, and the suggestion of curvature, if it exist at all, is too slight to be readily recognised.

The effect of position on illusions of this sort can hardly, I think, be better illustrated than it is in Fig. 9, which may be described as "a nest of illusions."

Thus, First, the sides of the square ABCD, which are really straight, appear to be curved inwards.

Secondly, the sides of the square *abcd*, which are really straight, appear to be curved outwards, as (in less degree) do the sides of the square 1234.

Thirdly, the space between the two large heavy circles *mir* and *nzn*, appears to be superposed on the rest of the figure.

Fourthly, arc *no* (lighter curve) appears to belong to a smaller circle, instead of belonging, as it really does, to the same circle as the arcs *lm* and *pr*, the same holding with the other corresponding circular arcs of the figure.

Fifthly, it is difficult to realise that the space between the arcs *st* and *ij* is as broad as that between the arcs *xy* and zz, &c.

Sixthly, the circles between *no* and zz appear to draw closer together, though in reality concentric, as they approach the heavy circle *noz*, &c.

Seventhly, the [illegible] appear to draw further apart along the region $2cd$, &c.

But it will be found that the illusion varies in amount for corresponding parts of different portions of the figure; and that, as in the simpler case illustrated in Fig. 8, the different parts of the figure seem to vary in shape as the picture is turned round.

Reviews.

AUTHORS AND PUBLISHERS.*

THIS work, we are told in the preface, is intended chiefly for those who for the first time are about to commit their literary productions to the press, and who are unacquainted with the prevailing practices in regard to printing and publication. "The information contained in it will, no doubt, be more or less familiar to experienced authors." This may be so, but all the same, it is a work which every author, whether experienced or not, should undoubtedly possess. And, unless we mistake, it is a work in which the general public will find much to interest them. It is described as a rudimentary handbook, giving just those particulars concerning paper, printing, binding, and publishing, the preparation of copy, the correction of proofs, the embellishment and illustration of books, and the relations of publishers and their clients, &c., with which an author requires to be acquainted. The publishers have endeavoured to touch upon every point which is likely to arise between the period of the preparation of the manuscript for the press and the actual publication of the book, presenting at the same time a precise and accurate account of the mechanical details of printing. There are also most valuable notes on advertising, reviewing, and the law of copyright.

I am myself tolerably familiar with the matters dealt with in this work. At least, I have written nearly forty works, which have been published for me by five or six firms, with all of which I have had pleasant relations. Yet even for so old a hand as myself, the work before me is full of interest. When I began to write, it would have been still more valuable to me. It would have taught me one lesson, at any rate, which many have only learned by a rather dear experience. Publishers have made mistakes, we know; but, as a rule, the advice of an experienced publisher should be followed by a comparatively inexperienced author, and it should always be carefully weighed. I well remember how thoroughly mistaken I supposed Messrs. Longmans to be when they advised me to print no more than a thousand copies of my first work—"Saturn and its System." I had put so much hard work into that treatise, had filled so many pages with long and complex calculations, had drawn the illustrations so elaborately, that I thought many thousands must needs care for my book, and buy it if it were not too dear; while if but a thousand copies were printed, it must of necessity be rather dear. When they told me that very few cared for formulæ and diagrams, for the "great inequality" of Jupiter and Saturn, and so forth, I thought they underestimated the intelligence of the general reader. That was sixteen years ago, and the first edition of "Saturn and its System" is only just approaching exhaustion.† They knew, and I did not, what was best and wisest.

The advice of publishers about advertising, selection of journals to which a book should be sent for review, and other matters of that sort, is nearly always sound, and is always based on sound considerations. When advertising is left entirely to publishers, they are apt, in some cases, to be a little extravagant, so far as my experience goes. At least, I have found two books, separately published by one firm, selling no better than two precisely similar books, published under similar conditions at another time, by another firm, which were much less expensively advertised. But the selection among so many serials as exist, of those in which a publication should be advertised, is so difficult, and requires so much experience, that the author does wisely to avail himself of his publishers' advice in this respect.

The choice of a title is a point which authors would not care to leave entirely to their publishers; yet more than one of my own books bear titles which were either invented by the publishers, or modified at their suggestion. In the work before us, many curious illustrations of authors' mistakes and weaknesses on this point are given. The most curious, perhaps, are the titles selected by Puritan writers, whose title-pages exhibit such eccentricities as these:—

"Eggs of Charity, layed by the Chickens of the Covenant, and boiled in the Water of Divine Love. Take ye and eat."

"Some fine Biskets baked in the oven of Charity, carefully conserved for the Chickens of the Church, the Sparrows of the Spirit, and the Sweet Swallows of Salvation."

"A Reaping Hook, well tempered for the Ears of the Coming Crop."

"Hooks and Eyes for Believers' Breeches." (May we infer, by the way, that in the seventeenth century hooks and eyes were used "in this connection"—that is, where buttons and button-holes are used in our time?)

"High-heeled Shoes for Dwarfs in Holiness."

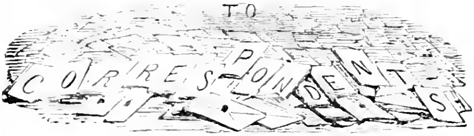
The matter relating to choice of paper, sizes of type, and corrections (this last especially) should be carefully studied by all who write or intend to write books. Others will find it interesting and instructive.

"Authorship and Publication" is eminently readable throughout. Technicalities are avoided where possible, and explained where they cannot be avoided. There is an amusing collection of technical expressions for the various names used to define correctly different degrees of anonymity in authorship, the student of which will be able thenceforward to distinguish an *allonym* from an *anonym*, and a *cryptonym* from a *boustrophedon*.

* "Authorship and Publication: A Concise Guide to Authors in matters relating to Printing and Publishing, including the Law of Copyright and a Bibliographical Appendix." (London: Wyman & Sons.)

† I was gratified to learn a few years since, from a criticism of Mr. Geo. Holden (then of the Washington Observatory, now of Ann Arbor), in which he rather severely denounced a book of mine, which is still in manuscript and was then unwritten, that the sales of my first book had been so great as to tempt me to seek a fortune by writing scientific treatises. Otherwise I should have thought the book had involved a rather heavy loss. But we should always believe what we see in print.

IMITATION FLOWERS MADE WITH LIQUID FILMS.—A pretty experiment has been recently described by the well-known Belgian physicist, M. Plateau. He bends fine iron wire so as to present the contour of a flower of six petals. The central ring, to which the petals are attached, is supported on a forking stem, which is stuck in a piece of wood. After oxidising the wire slightly with weak nitric acid, the flower is dipped in glyceric liquid, so as to receive films in the petals and the central part. It is then turned up, placed on a table near a window, and covered with a bell jar. For a little while it appears colourless, but soon a striking play of colours commences. In the experiment M. Plateau describes, the flower continued showing modifications of colours for ten hours, when dusk stopped observation. Next morning several petals had burst. The liquid used was of very mediocre quality. M. Plateau recommends preparation of the liquid thus:—Dissolve a fresh piece of Marseilles soap, cut up into small pieces, in 40 parts by weight of hot distilled water. Filter after cooling, and mix thoroughly three volumes of the solution with two of Price's glycerine. The solution should be left at rest till all air bubbles are gone.—*The Times.*



Letters to the Editor.

[The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with their writers. He requests that all communications should be as short as possible, consistently with full and clear statements of the writer's meaning.]

All communications should be addressed to the Editor of KNOWLEDGE, *71, Great Queen-street, W.C.*

All Cheques and Post-Office Orders to be made payable to Messrs. Wyman & Sons.

⁂ All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.

All Letters or Queries to the Editor which require attention in the current issue of KNOWLEDGE, *should reach the Publishing Office not later than the Saturday preceding the day of publication.*

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

Our Correspondence Columns.

TO OUR READERS.—NOTICE TO CORRESPONDENTS.—THE SUN'S HEAT.—WASTE OF ENERGY.—THE EARTH'S REVOLUTION.—THE FIFTEEN PUZZLE.—FLIGHT OF BIRDS.—CORRECTED SUN-DIAL.

WE begin to see more clearly than we did at first the lines on which our Correspondence columns will have to be conducted, though we shall always be glad to have hints and suggestions from our readers towards the improvement of this section of KNOWLEDGE. Dropping the editorial "we," let me note that from the beginning I have proposed to take my share in the Correspondence, not only in answering queries, explaining difficulties, &c., in subjects I am able to deal with, but also in asking questions, inviting explanations, and so forth. It will be found, also (indeed, I think the last and present numbers give some illustration of the fact), that space will as readily be found for corrections of the mistakes I make myself, as for those into which others may fall. In the two first numbers I appended, in several cases, my replies to the respective letters to which they related; but hereafter I propose to reply each week to letters and queries falling into my department which have appeared in the previous week. Others have promised me their assistance in dealing similarly with other inquiries. But I hope many who have information to give on subjects of interest respecting which inquiries may be made, will do so, as occasion may suggest. We wish to leave as few inquiries unanswered as possible.

I take this opportunity of reminding correspondents that their letters should reach the office by Saturday at latest, if they are to appear in the number for the following week. Already, owing to the unexpectedly rapid growth of the correspondence, it has been found necessary to arrange that no letters received after Monday shall be even read till the matter for the following week has been made up; and, of course, in making room for letters, the rule "first come, first served" must be attended to, not indeed with absolute stringency, but in the main. We must also beg our correspondents to bear with us if we are obliged to abridge some letters. There shall be no favouritism, either in selection, or in inserting more or less from letters which reach us; in every case we shall be guided by our view as to the wishes and requirements of our readers. Nor need correspondents be surprised, any more than we are, if every letter they may send us, or every part of each letter, should not be thought precisely what would suit the plan and purpose of KNOWLEDGE.

Taking first letter 2, p. 15, I note that "Anti-Guebre's" difficulty is one which I have found, during my lecture tours in Great Britain, America, and Australasia, to be more common than any other whatsoever. I suppose I must have received more nearly a thousand than five hundred letters presenting precisely "Anti-Guebre's" difficulty, so that, I may remark in passing, Mr. Newton Crosland must not regard himself as sole inventor and patentee of the paradox in question. The answer is not so simple as many seem to suppose. None of our correspondents have, indeed, answered "Anti-Guebre's" difficulty correctly and fully. If we remember that the existence of snow in large quantities on mountain tops and of ice particles in the cirrus clouds, implies the prior existence at those heights of large quantities of the vapour of water, we shall see that the asserted dryness of the higher air (and, therefore, the unimpeded passage of heat rays through such air) can hardly be regarded as demonstrated. Again, to say that rarefied air has no capacity for heat is, in reality, to speak in terms belonging to the old and erroneous theory of heat as a sort of fluid. Many seem to imagine that the mere rarefaction of a gas is a cooling process, overlooking the statement which accompanies all correct accounts of experiments on the subject, that it is not the rarefaction itself, but the work done in expansion that causes loss of heat. In like manner, when gas is compressed, it is not heated because of its greater density, but because a considerable part of the extraneous work applied to produce compression is transformed into heat. In one case we have force obtained at the expense of so much heat, in the other heat is produced by the expenditure of such and such force. If the air around lofty mountain tops were simply very thin, and there were never any interchange between the higher and lower air, not a particle of snow would ever be seen on our mountain tops, nor should we ever see the cirrus or feathery snow clouds in the upper air. But because there is interchange, because the air which rises along mountain slopes expands and does work in expanding, it is made cooler and cooler, till at great heights it becomes altogether refrigerated, while the air which descends to replace the air which has risen becomes warmer, because in descending it is compressed by the action of gravity (an extraneous force) and a portion of the force thus exerted appears in the form of heat. Again, in the case of the upper feathery snow-clouds, the refrigeration comes mainly from the ascent of aqueous vapour. The wool-pack clouds which we see on a summer day are the upper parts of columns of ascending aqueous vapour, where the vapour has been condensed to water particles or vesicles, and so appears in the form of visible cloud. At the upper surface of these clouds a process of evaporation is continually taking place, and currents of vaporous air are continually ascending. When these ascend high enough to be sufficiently refrigerated, they form in turn into water-drops or vesicles, but under different circumstances from those prevailing where the cumulus or wool-pack clouds are formed. The refrigeration is more rapid owing to the rarity and *relative* dryness of the higher air, and thus the water particles up there form snow-crystals, and (under the conditions stated) cirrus clouds are formed. That they are not always formed is due to the circumstance that these conditions do not always prevail.

As to the waste of solar and stellar energies referred to in the same letter, it is probably only the limited nature of our knowledge which causes that to seem like waste which may, for aught we know, involve the most perfect adaptation possible of the energies in question. We may say of such inquiries, with the poet :—

> "Reason, alas! it does not know itself:
> Yet man, vain man! would with this tiny plummet
> Measure the deep profound.
> He sees but part of the chain—the nearest links—
> His eye not reaching to the equal beam
> That poises all, above."

We need not infer that what we see is seen incorrectly because we see not all.

In letter 5, p. 35, Mr. Crosland uses arguments which only require to be understood to be refuted. What meaning can be found in the statement that "light and heat are phenomenal products, caused by magnetic and electric forces, in a state of intense activity, acting upon atmospheric conditions"? Why not equally well say "Magnetism and electricity are phenomenal products, caused by luminous and calorific forces, acting under aqueous conditions"? Again: the sun might "possess the power of producing the phenomena of incandescence, without being itself incandescent." But, as a matter of fact, he is incandescent, if incandescence means, as it does, glowing. Electricity produced in another room may make a wire on the table at which I write white hot; but when looking at the wire I see it to be glowing white, and when by actual tests I find it to be radiating heat, I cannot agree that it is neither incandescent nor hot, because something which is neither glowing nor a source of radiant heat may yet produce both light and heat.

Several correspondents think "Tyro's" letter (6, p. 35) should not have appeared. It is not very pertinent, but it serves to show one chief way in which paradoxes arise, viz., from want of thought. "Tyro" must have seen hundreds of cases where a luminous body causes a distant opaque body to appear bright, without any flood of effulgence along the space between; yet he expects to see that on the large scale which he does not see on the small scale. I do not take exception to his remarks as relating really to light, not to heat, because I assume he considers that where there is lustre (inherent) there is also heat. It is not always, though it is generally the case.

"Tyro's" other query (letter 9, p. 36) is one often made, though most text-books insufficiently explain the difficulty. The only kind of revolution which some readers of astronomical works seem capable of understanding is such as we see where a ball at the end of a rigid rod is swayed by the rod round a centre. The real revolution of the earth more nearly resembles the motion of a spinning top when thrown (on a curved path) through the air, the axis moving all the time parallel to itself, or retaining an unchanged direction while changing constantly in place.

In my remarks on letter 13, p. 37, the words ten millions, last line but two of second paragraph, should be ten billions. There are in all more than twenty millions of millions of possible positions in the "fifteen puzzle," though some, overlooking the circumstance that every change in the position of the blank square changes the puzzle, suppose there are only about 1½ millions of millions.

As regards query 3, p. 30, on the flight of birds, it is quite certain, as others, I see, have pointed out, that the air in the passages of the bones cannot help the bird by adding to its buoyancy. If an absolute vacuum could be produced in these passages, which (if the passages remained unchanged in volume) would give the maximum degree of buoyancy, the only lifting power which would result would be simply equivalent to the weight of so much air at the actual temperature of the air in which the bird is flying at the moment, as would fill the passages in the bones. Air equal in volume to the bird itself would not have more than a 100th part of his weight; how much less, then, would the counterpoising of his weight by a raising power corresponding to the weight of the tiny quantity of air which would fill the passages, be of any appreciable avail in helping him to fly?

I will describe and sketch in an early number a very simple instrument for telling time from a shadow (as in the case of the sun-dial), without any appreciable error arising from the shadow not being sharp. RICHARD A. PROCTOR.

THE MISSING LINK.

[33]—I am glad to see a correspondent ("An Ignoramus") has asked for an explanation regarding the "Missing Link" of Darwinism and Evolution. Briefly stated, here is the problem. If, as evolution postulates, the various forms of animal life have "sprung from one, or, at most, a few, primitive forms, then we must conceive of living nature as a tree of which all the parts are connected together from root to topmost twig. Now, if man exists, as he unquestionably does, at the top of the highest twig, it is evident he must be connected, by some forms more or less like himself, with lower quadrupeds, and through these latter, with still lower animals, and so on. Where, then, in the case of man, are the animal links that lead from the human to the pre-human, and from the pre-human to the purely animal? When evolution was first promulgated, everybody asked "Where are the missing links?" That common ignorance, which too often passes for common sense or for science, at once inquired where was the link that connected man with the monkey? This question is founded on gross ignorance of what evolution requires. No evolutionist assumes that man is descended *from any existing ape, or from any extinct ape either*. Mr. Darwin, in his "Descent of Man," is very careful to point this out. What evolution does say is, that probably man and apes originated far back in some common root-stock, whence the human branch proceeded, diverging for ever, from the ape branch, on its own way of development. If we take the four highest apes—gorilla, chimpanzee, orang, and gibbon—we find no one of the four to approach man as a whole more nearly than any other of the four. If an approach to the human frame were to be made from the ape-side, we should require a bit from each and all of the four to make up such a representation of the human type. The orang's brain is, for instance, more like man's than the gorilla's, but the latter approaches man's more nearly in some other points. It is the same with the gibbon and the chimpanzee. The "old ladies of both sexes," who used to assume that the gorilla as the "missing link" was a failure, did not know that the evolutionist thoroughly agreed with them. Where the "links" that connect man to his lower neighbours are to be found, is a difficult question to answer. Possibly we must go first to lower human life, and find an approach to animal characters in the skulls of savages and primitive men (*e.g.*, Neanderthal skull); but the geological record is imperfect. There are long gaps in the series which can never be filled. All living forms have not been preserved in the fossil series. With human remains, the chances of preservation are few and far between. Even primitive man buries or burns his dead; and thus the record of man's past history may ever remain obscure. But all the evidence points indubitably to man's origin from lower life. His development shows this idea to be true; the presence of rudimentary organs (such as ear-muscles, which are of no use to him) tells the same tale; and anthropology, in its researches into savage life and customs, verifies evolution. I hope "Ignoramus" will feel satisfied with the above answer. He should read Darwin's "Descent of Man" and Mr. Tylor's "Anthropology," and an article on "Missing Links," in a recent volume of the *Gentleman's Magazine*, will also help him in his endeavour to understand what evolution demands and implies.

ANDREW WILSON.

THE SUN'S HEAT.

[34]—I must confess I was somewhat surprised to read "Anti-Guebre's" letter in your first issue, considering the preceding remarks about "paradoxers;" but since he asks for information, I shall be glad to give it him.

In the first place, the air is not capable of being directly warmed by the rays of the sun, but it is warmed by contact with the heated earth; consequently the air is much warmer in the valleys than on the tops of the mountains. Again, in the valleys the air is not so subject to disturbance by winds, &c., as it is at a higher elevation; so when we ascend a mountain or rise in a balloon, we recede both from the body of the earth and from the heat thereof, for air is a very bad conductor of heat; but though the *air* is colder, the *sun's rays* are not. If "Anti-Guebre" had ever ascended a snow-capped mountain, he would know that the rays of the sun are insupportably hot there: for two reasons; first, the traveller is *nearer* the sun, and unprotected by clouds and aqueous vapour; secondly, he is exposed to the reflection from the snow. The snow protects the mountain itself from being much warmed, and is only very slowly melted, as it reflects the greater part both of the heat and light. The deposition during the night makes up for the loss experienced during the day. Alpine climbers usually complain that their lower limbs are nearly frozen by the snow, while their heads and shoulders are almost roasted by the sun.

Aqueous vapour plays a very important part in the warming of air, for it is the vapour rather than the air itself that receives the heat. Dry air cannot be warmed so easily as damp air. This is the reason why the air feels warm in dry, frosty weather, as it cannot conduct away the heat from the body; whereas, damp air feels cold, since the aqueous vapour *does* conduct away the animal warmth of our bodies. (Of course, I am not considering the subject of evaporation.) At high elevations the air is colder and dryer than at the surface of the earth.

With regard to the icy nature of cirrus clouds, there is nothing absurd in the supposition.

Anyone who watches the sky in summer will see clouds disappear and reform, often with great rapidity. This is due to the varying temperature of the air, or rather of the air currents, which either condense or volatilise the aqueous vapour in the air, according to their temperature. We know that certain clouds *do* consist of icy particles when we see a halo round the moon making a particular angle with the observer's eye. If "Anti-Guebre" thinks an ice-cloud ought to be instantaneously melted by the heat of the sun, he is in error, as, putting on one side the diathermancy of ice, and granting that the action of the sun would be to melt it, the evaporation at that altitude would be so rapid as to freeze again the water formed, consequently, the volatilisation of the cloud would take some time.

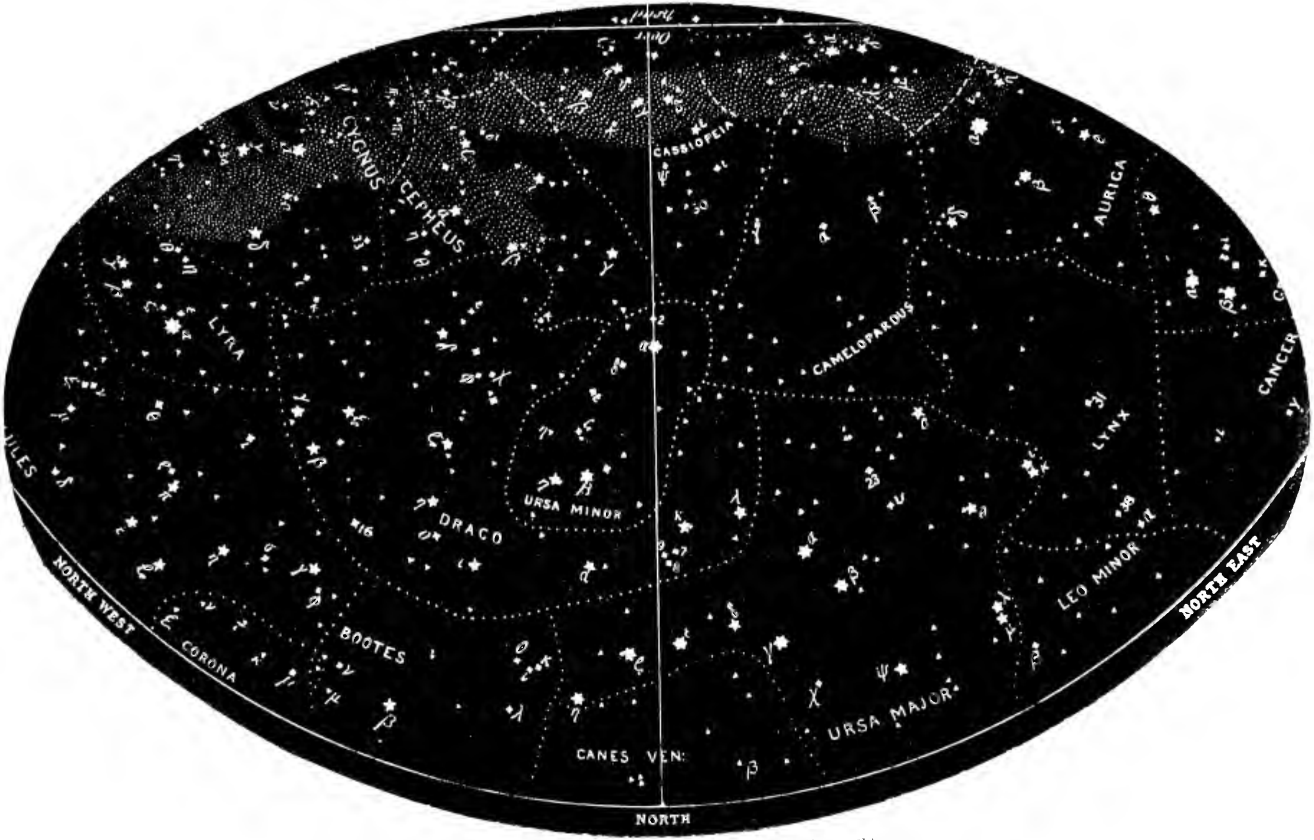
"Anti-Guebre's" "profit and loss" ideas of the solar system are too deep for me. It is true that we only receive $\frac{1}{2,070,650,000}$ of the sun's light and heat; but what right has he to say that the rest is wasted? He must first find out what becomes of it, and prove that it does no work. He might, with equal truth, say his own time is wasted when in bed, because he is not conscious of what takes place when he is asleep.—I am, &c., SM.

THE SUN'S HEAT (*Abstract*).

[35]—"Anti-Guebre" says that when he approaches the fire he feels warmer, but that, on the contrary, an approach to the sun produces a cooling effect, hence the sun is cold. In the case of the fire we make a very great difference in the absolute [it should be relative] distance between us and it, by a very small movement on our part; whilst in the case of the sun, any difference in the absolute [relative] distance between us and that orb which we are able to make is immeasurably small compared with the absolute distance; and though it may be said there *would* be an *augmentation*, though small, there are other causes at work which more than neutralise this augmentation.

It has, I think, been fully proved that the air itself is transparent for heat, and that the air is *warmed* by contact with the earth; the higher, therefore, we rise above the sea-level, the colder does the air become, and this effect is ample to overcome any slight increase in the heating power, owing to a decrease in the direct distance of

THE NORTHERN SKIES IN NOVEMBER.



This Map shows the position of the stars in the Northern Skies:—

On November 3, at 11¼ o'clock.	On November 10, at 10¾ o'clock.	On November 18, at 10¼ o'clock.	On November 25, at 9¾ o'clock.	On December 3, at 9¼ o'clock.
On November 7, at 11 o'clock.	On November 14, at 10½ o'clock.	On November 22, at 10 o'clock.	On November 29, at 9½ o'clock.	On December 7, at 9 o'clock.

The stars in the northern skies are carried from left to right under the pole, and from right to left above the pole, descending to the left or west of the pole, and ascending to the right or east of the pole. They are carried, in fact, round the pole in a direction contrary to that in which the hands of a watch move.

*** Stars of the first magnitude are shown with eight points, those of the second with six, of the third with five, of the fourth with four, of the fifth with three.

the sun. It seems to me that the matter is of far greater complexity than your correspondent imagines.—Yours, &c., VERAX.

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[36]—Your correspondent "Anti-Guebre" (p 15, No. 1) takes up rather a large amount of what I expect in future will be very valuable space, with his question as to whether the sun is hot, and in proof that it is not so, he brings forward the apparent fact that as we ascend high mountains we get cooler. This, however, is not exactly a fact, and it is very difficult to convince the unscientific public that it is not so, as I found to my trouble during a twenty years' residence as a medical man in India. Now the amount of heat which we are sensible of on a hot day does not come to the body directly or entirely from the sun, but from the earth, which has been heated by the sun's rays. In the hills (I refer more especially to tropical countries), the heat received by the soil during the day is radiated to such an extent during the night, that the surface becomes thoroughly cooled down. In the plains below, probably from the greater density of the superjacent air, the radiation is very small, and the heat is retained, to be added to by the succeeding day's sun. The movement of currents of air, also, is much more constant in the hills, and this acts like a fan in continually changing the air which has become heated by contact with the surface of the earth. The heat received directly from the sun should, of course, increase the higher we ascend, not because we are a few thousand feet nearer to it, but because so much less of its power is absorbed by the lower and denser layers of the atmosphere. And so it does, as is proved by the fact, not so generally known, that the black bulb thermometer indicates the increased temperature as the elevation increases. The readings of this at Ootacamund, in South India, average about 14° above those taken simultaneously at the coast towns. I have repeatedly noticed that Europeans who come from the low country up to the hill sanitaria, while enjoying the cool atmosphere, yet had their faces completely peeled by the increased direct action of the sun's rays. I have seen a child's hat blown off, and before it could be picked up again the child was stricken down by sunstroke, and this at a time when there had been ice on the plain during the night, and I have always noticed that while it was possible to walk short distances in the sun in the low country with an ordinary green silk umbrella, yet that at six or seven thousand feet elevation this became quite insufficient, the heat seeming to strike right through it, at once rendering the addition of a white calico covering absolutely imperative. B. M., F.R.C.S.

Nov. 18.

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[37]—In letter 2, page 15, your correspondent says that "the sun is not himself hot'; but, of course, he is the source of heat to the earth." How does he reconcile the two statements? He might as well maintain that his fire, which he uses as a conclusive proof to himself of the non-incandescence of the sun, is not in itself hot, though it is a source of heat to things surounding it. He also uses that question which has been so often advanced by inquirers, and as often replied to by scientists. Why is it that the higher you go from the earth the colder it becomes, if, as people say, the sun be hot? He forgets the important part the atmosphere plays with regard to our earth. I maintain that the nearer the earth is to the sun, not the nearer we get up through our atmosphere to it, so the heat transmitted becomes greater. When the earth is at its perigee, the sun transmits more warmth than when it is at its apogee. To this he may advance the argument that the perigee occurs in January. So be it. From this very cause winters are milder and summers cooler in the northern than in the southern hemisphere. Then, as he says the sun is not hot, can he explain why the earth should derive more heat from the sun when its rays fall vertically than when they fall obliquely? If the source of heat be contained in the earth itself, then should the poles be warmer than the equator. We know the opposite to be the case. In Letter 5, page 35, your correspondent says, "Light and heat are surely phenominal products, caused by magnetic and electrical forces in a state of intense activity." If so, why is there greater heat when the sun's rays are vertical, if it has nothing to do with the production of heat? In this letter there is more dealing with probabilities than with facts. Probabilities do not exist in Nature, and I trust that through your columns we may arrive at a clearer idea of the truth concerning this question of the sun's heat. I hardly see that Letter 6 requires an answer, as this is not so much a question of light as of heat.—Yours, very faithfully, G. G. D

Nov 15, 1881.

STAR NAMES.—COMETS' TAILS.

[38]—May I suggest, in regard to your excellent star maps, that it might be some improvement to them if the names of well-known stars, such as Capella, Aldebaran, &c., were marked in the map, as well as the name of the constellation and the α, β, γ, &c.; or, at least, attention might suitably be called to them in a note naming the brightest star in each particular map.

I have annotated your first map for the benefit of my boys, writing at the side—

Aldebaran = α Tauri,
Rigel = β Orionis, &c., &c.

No explanation I have yet heard as to the material, &c., of comet's tails appears very satisfactory. Has the idea ever occurred to you that a comet repels matter from its nucleus in the form of vapour, or a highly-attenuated condition of solid particles, not in one direction only, but *laterally* as well? And that we might only see the portion of this matter through which a ray of light was projected from the sun or from the glowing mass of the comet itself, at such an angle as to render its path visible to us in the same manner as we see sun-rays at sunrise and sunset, or when the sun is emerging from a cloud?

Would not this account for the apparently incredible rapidity with which comets' tails swing round the sun at perihelion, and also for comets, when distant from the sun, not exhibiting any tail at all?

There are other points I wished to mention, but have not time to-night. I heartily wish your excellent paper every success, and am, yours faithfully, SPECULUM.

PRACTICAL WORK WITH THE TELESCOPE.—MAP OF EASTERN SKIES.

[39]—I am very much pleased with the first two numbers of KNOWLEDGE, and I am doing all I can to make it known amongst friends. I have had some difficulty in getting the numbers, but perhaps this will be all right hereafter.

I have been a "star-gazer" for a few years, and have read a good many of the treatises on astronomy, but have never been able to turn my knowledge into any practical shape. If you could throw out a few hints as a guide for those desiring to start in this study, I think they would be of great service. What particular branch would you recommend to start with, and how ought the study to be carried out? What handbooks and instruments would you recommend?

Surely there is some mistake in your map of the eastern sky issued with No. 1. In your map you place Saturn highest above the horizon, then Jupiter, then the Pleiades; whereas the reverse is the order, Orion is placed very much on its side, and so on. Wishing you every success, I remain, yours, &c.,

Edinburgh, *Nov.* 16, 1881. G. M.

[With reference to the last question, has "G. M." understood that the circular outline of the map is the horizon? Thus it must be held with the word SOUTH-EAST lowest to show the position of the objects he names, which all lie, speaking generally, towards the south-east at the hour corresponding to the map. Note, however, that by an unfortunate error, corrected in part of the present issue, the hours named under the maps in Nos. 1, 2, and 3 are all, without exception, one hour too late. This should be corrected in each map, as the maps, apart from the planets shown, will be as correct for next year, or for twenty years hence, at the same dates, as for the present year.—ED.]

CELESTIAL OBJECTS.

[40]—I have obtained the first two Nos. of KNOWLEDGE, and am much pleased with the contents; the star maps especially I was very pleased to see. Would it be in accordance with your plans to publish, from time to time, a few notes descriptive of interesting and remarkable objects in the constellations? Several of my friends to whom I have shown KNOWLEDGE have promised to become subscribers; and to those of us who dabble a little in astronomy, the name of the Editor is a sufficient guarantee of the information we shall get on that subject, at any rate. Wishing KNOWLEDGE the success it deserves,—I remain, yours, &c. W. J. C.

ARE WOMEN INFERIOR TO MEN? (*Abstract.*)

[41]—I perused with some interest in the first issue of your paper an article on the question whether women are inferior to men. The study of the human mind and character seems to be neglected by most publications, although it is a most interesting subject. I am sure if you will open your columns for such subjects, you will not fail to gain appreciative and constant readers. The

subject in question is one of which different views may be taken, and I need therefore make no apology for writing a few words on it. The writer of the article seems to be under the impression that he has only to prove the fallacy of M. Delaunay's arguments to show that woman is not inferior to man. I have no doubt but what such a question will more frequently present itself to the public mind as the competition in the labour markets between men and women becomes keener. The author of the article throws just ridicule on the comparisons made by M. Delaunay, yet he himself does not hint in the least that the comparisons are altogether improper and most unjust to the fair sex, as they really are. Men are too apt to forget that the first duty of woman is to keep a home and rear their young; to unite with, and not struggle against men. Man and women united form a perfect being; they are one in every sense of the word; apart they are both imperfect. Wishing KNOWLEDGE every success, Yours, G. W. D.

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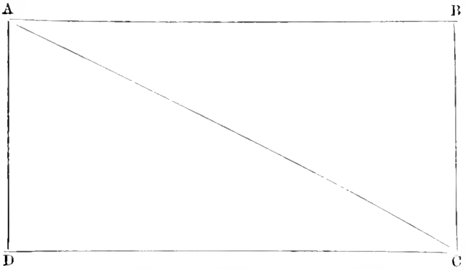
[42]—In your review of the above, in Vol. I., No. I, p. 8, you say, "We come next, in M. Delaunay's paper, to the more important question of cranial capacity and form; for, certainly (setting aside, of course, phrenological absurdities)," &c. 1. How do you *prove* your "*assumptive statement*" as to phrenology being *absurd*, *ergo*, false? 2. How do you account for your *special* knowledge relative to capacity and form, apart from phrenology? 3. Does the brain shape or mould the form of the cranium or skull?—Yours truly,

CHARLES HAMILTON.

SPEED OF AMERICAN ICE-YACHTS.

[43]—I think I can show non-mathematical readers a way in which they can satisfy themselves that an ice-yacht *may* sail faster than the wind.



In a board or card cut a groove, or slit from A to C; in it fix a freely moving slider; lay a ruler along A B, and, keeping it parallel to itself, slide it down to CD. The slider represents the yacht, the groove the effect of the yacht's runners, and the ruler the front of an advancing body of wind. As, then, the slider moves faster than the ruler, describing AC, while the latter describes AB, so the yacht may move faster than the wind. Of course, I do not say that the above arrangement explains all the forces in action; but I think it removes the chief difficulty, which seems to be in failing to see how the effect (so to speak) can be greater than the cause.

W. J. M.

[The reader will notice that "W. J. M." carefully limits the application of this reasoning. It shows only—as he mentions—that an ice-yacht *may* move faster than the driving force. Any amount of energy may be applied to move the ruler until the resistance of the slider is overcome. The ruler cannot move as described unless the slider gives way in the only way in which it can move; in the case of the yacht, the wind may blow over and past the yacht. But "W. J. M.'s" experiment shows all he claims for it.—ED.]

ARE MEN'S HEADS SMALLER THAN OF YORE?

[44]—In a letter on "Are Men's Heads Smaller than of Yore" in your Nov. 11 issue, a quotation is given from Professor Flower, in which several suggestions are thrown out as to reasons why hats should be smaller than they were twenty years ago, without a corresponding diminution in the size of heads. As a seller of hats for many years, I am able to verify several of those reasons to account for smaller-sized hats being required. No doubt, the hair being cut closer makes at least half a size difference, but the present manner of wearing hats on the top of the head, instead of down over the back of head and ears, as formerly, would make the most difference (at least half an inch), which any one can prove for themselves by measurement. Again, youths and young men now wear hats instead of caps, so that in sorting up sizes for sale, I find it necessary, if it is a fashionable shape, and likely to be worn by young men, to order—

Sizes	$20\frac{1}{4}$	$20\frac{3}{4}$	$21\frac{1}{4}$	$21\frac{1}{2}$	22 inches	
Relative numbers ..	1	2	3	2	1	

while for older styles, just because older men wear their hats further on the head, and like an easy fit, and not because their heads are larger, I have to order—

Sizes	$21\frac{1}{4}$	$21\frac{1}{2}$	22	$22\frac{1}{2}$	$22\frac{3}{4}$	$23\frac{1}{4}$ inches
Relative numbers ..	1	2	3	3	2	1

Yours, &c. HATTER.

ARE MEN'S HEADS SMALLER THAN OF YORE?—THE FLAT EARTH THEORY. (See letter 12, p. 36.)

[45]—Has it ever been noticed that most grooms, and such men, wear their hats, generally, on the *side* of the head? Possibly this may have something to do with the supposed change.

The following is from an article on Physical Geography, which appeared in a book published a short time ago:—

"It is remarkable that persistent efforts are still made, and supported by a great display of mathematical technicalities, to prove that it (the earth) is a circular disc; and within the last few years elaborately illustrated books have been produced in advocacy of the supposition."

I should be glad if you would say who are the publishers and what is the price of the books. Or if you could tell me on what facts the writers base their theory.—BETA.

[See reply to John Hampden in Answers to Correspondents.—ED.]

"KNOWLEDGE," TECHNICAL TERMS, SCIENCE, AND RELIGION.

[46]—I have been very much pleased with the first two numbers of KNOWLEDGE. If the forthcoming numbers be as good as Nos. I. and II. promise, you will supply a great want, and deserve an equally great popularity. I have done what little I can to advertise it, by placing it in our school reading-room, and by sending it by post or otherwise to various friends.

I had intended to suggest what "A.T.C." has mentioned, the advisability of printing the technical terms side by side with the more popular modes of expression; by this means a double education would be carried on.

I congratulate you upon your article maintaining the harmony, real if not apparent, of the teachings of science with revelation, and your remarks on the wickedness of neglecting to use the minds God has given us in the study of the "outward and visible signs" of His power. Your article must, I feel sure, have given great pleasure to many whose belief in evolution is no less devout than their belief in revelation. Would that all our religious instructors could act in the spirit in which you intend to conduct KNOWLEDGE. With every wish for your success, which I shall do all in my power to promote,

I am, yours, &c.,

J. JOHNSON HOYLE, B.A.Lond.

[Have omitted remarks relative to some of the questions in No. I., though fully agreeing with you.—ED.]

INFLUENCE OF SEX ON MIND: CRANIAL CONTOUR.

[47]—To say woman's skull is less than man's is ambiguous and misleading. Of course, it is *absolutely* less, otherwise woman's head and body would be disproportionate. I think male and female skulls relatively equal. Artists divide the male figure into eight heads, and the female into seven and a half. Grant, then, the female skull relatively somewhat larger than the male. "The brain diminishes and the nerves increase from man downwards; in the fœtus and child, the nerves are proportionately larger than in the adult." (Lawrence: "Lectures," chapter vi.) Walker states that the nerves connecting the brain with external organs of sense are comparatively larger in woman than in man. The eyes excepted, external organs of sense are smaller in woman than in man. Does anatomy support Walker's statement? If so, it helps to explain observed fundamental differences in male and female intelligence. Woman is perceptive; man reflective. Woman excels in sensibility and observation. Man is a being of the intellect; woman of instinct and emotion. Man reasons and reflects; woman perceives and feels. Man is active; woman passive. To man belongs the kingdom of the head; to woman the empire of the heart. Chamfort epigrammatically says:—"Woman has a cell less in the head, a fibre more in the heart."

The sexes cannot exchange sovereignties. Women is less guided by intellect than by feeling and impulse. Her movements are more

easy and prompt, though less sustained, favoured by ready obedience of muscular action and short stature. She is less combative than man. (This rule has serious exceptions.) She desires to please. Man's mission is to protect and defend. Her disposition to sustain mental and bodily exertion is much less than man's. She is fonder of change, and more fluctuating in opinion. *Varium et mutabile semper fœmina. La donna è mobile qual piuma al vento.* Not by her understanding or mental force, but by her prompt and easily-affected sensibility, is woman eminently adapted to surmount maternal suffering; through affection and pity, to be interested in children and household cares. Where her heart is touched, a woman will make incredible sacrifices for a lover, a husband, a child, a parent. She is constitutionally fitted to be wife and mother, to "guide the house," for minutiæ of details. A girl of sixteen makes a better housekeeper than a man of sixty. Woman is more sedentary than man. Her disposition is milder. She is less addicted to great crimes.

Woman's face resembles the child's in absence of beard, rounded form, smooth skin, and brilliant complexion. The infantile type of head appears in smallness of features relative to skull, and perpendicular forehead. Welcker says, woman displays orthometopy (perpendicular forehead) and a decided tendency to prognathism. I have seen both combined in the same skull, though a retreating forehead and projecting jaws generally go together; and orthometopy generally accompanies orthognathism. Camper's facial angle is a very uncertain measure of intelligence; according to it alone, the child would stand higher than the man. "If skulls are ranged according to Camper's angle, the infant's skull, contrasted with any animal's, occupies a higher place than the adult's; but if skulls are ranged according to the increasing angle of the sella, the series stand—man, woman, child, animal." (Welcker.) A front view of head and face is well defined by the outline of an egg. The big end represents the skull, the small end the chin. The egg's short diameter gives the position of the eyes, which bisect the oval. If in man, the part above be greater than that beneath the eyes, it is not objectionable, because we associate this excess of coronal elevation with intellectual qualities characteristic of sex. In woman's head, a similar excess in the superior region is a fault in beauty, for which the masculine qualities corresponding to this head-type do not compensate. Ancient Greek sculptors perfectly understood this important distinction in the cranial contour of the sexes. The female head contrasts well with the lofty, massive square brows of male heads. Of the Venus de Medici, Walker observes:—"The size of the head is sufficiently small to leave that preponderance to the vital organs in the chest, which makes the nutritive system peculiarly that of woman. This is the first and most striking proof of the profound knowledge of the artist, the principles of whose art taught him that the vast head was characteristic of a very different female personage. Phrenologists have told us that the head of the Venus is too small. They might as well have said that the head of Minerva or the Jupiter is too large, or a hundred other ignorant, inapplicable, or ridiculous pedantries" ("Analysis of Beauty"). The Gradgrind utilitarian school depreciate the fine arts. Independently of intellectual pleasure, what actual knowledge in these masterpieces! Ancient sculptors could learn nothing of beautiful forms from phrenologists. The latter may learn much from ancient sculptors of the influence of sex on mind.

Nov. 14. J. McGregor Allan.

SPEED OF ICE-YACHTS.—THE FIFTEEN PUZZLE.—CHEAP TELESCOPE AND MICROSCOPE. — TECHNICAL TERMS RELATING TO TELESCOPES.

[48]—There are two things in last week's Knowledge that I wish to mention to you:—1. In explaining the ice-yacht, you say the parallel wind HG (diagram) will increase the yacht's velocity, *which already exceeds that of the wind.* It seems to me that you do the very thing which you take exception to "Upsilon," doing, viz., take it for granted; but, perhaps, I may have missed your meaning.

The other thing (2) is the "Fifteen Puzzle." I am sure many of your readers would like, as well as myself, to hear more of this puzzle; what "the true won position" means; in fact, to explain what the puzzle means, as I candidly confess I have never heard of it before.

There is another matter I wish you would help me in. For some time past I have wished to become the possessor of a telescope and a microscope, but have not been able to see my way to doing so. I could afford to give about £5 each in purchasing them. With regard to the microscope, my ambition is to take, were it years hence, a degree, both in surgery and medicine. I know that a monocular is best suited for histological work; but I should like the time that I would spend at the microscope to materially serve me afterwards. Which should I purchase, a binocular or monocular? Could I get one to serve my purpose for £5, or would I require to pay more? Then, as for the telescope, I am puzzled at the various technical terms used in the magazines I come across, for instance:—(1), Equatorial telescope; (2), astronomical telescope; (3), terrestrial eyepiece; (4), reflector; (5), refractor; (6), 11-inch achromatic; (7), $6\frac{1}{3}$-inch object-glass; (8), $9\frac{1}{3}$-inch mirror; (9), altazimuth stand; (10), equatorial mountings and divided circles, &c. I shall feel obliged if you will tell me what to purchase and WHERE. I should like one that would do some good work for me, and repay me the cost in knowledge obtained. I may add that I have done my part by getting you another subscriber, and I sincerely wish Knowledge success.—Yours, &c., Twenty.

[The explanation at p. 36 shows that there remains a driving wind whose velocity is represented by HG when the yacht's velocity is represented by CE, or is greater than that of the actual wind FE. *A fortiori*, there is a driving wind for all smaller velocities. Starting from rest under the action of a wind in the quarter represented by FE, the yacht will travel with constantly-increasing velocity until the driving force is just balanced by frictional resistance, and it is shown at p. 36 that even when a velocity exceeding that of the wind has been attained, a driving wind remains, which may be quite sufficient to do more than merely maintain the speed attained. Suppose, for instance, that FE in Fig. 2, p. 36, represents a 40-knot breeze, then HG represents (it will be found, on measurement), a six-knot breeze. Now, an ice-yacht moves freely from rest under a six-knot stern wind, so that the velocity of the ice-yacht under the conditions illustrated in Fig. 2 would still increase, though CE corresponds to a velocity of more than 50 knots per hour.

I supposed every one knew the Fifteen Puzzle. It consists simply of a square space, within which are placed, first, sixteen square blocks, numbered in order from 1 to 16. Block 16 is removed. The rest are placed in any random position within the square space; and the puzzle is, by sliding the blocks successively into the vacant square which remains after each sliding motion, to get them into the order shown in the adjacent figure. A prize is said to have been offered in America to any one who should bring the blocks into this position—called the won position—starting from a position differing only from the "won position" in having the three blocks in the fourth line arranged 13, 15, 14, instead of 13, 14, 15 (a position which has been called the "lost position"), and thousands wasted hours on hours of their time in the attempt to do this impossible thing. Some said they had done it, but were assuredly mistaken. Others thought they had satisfied the conditions of the problem by getting some such arrangement as these:—

1	2	3	4
5	6	7	8
9	10	11	12
13	14	15	

	1	2	3
4	5	6	7
8	9	10	11
12	13	14	15

4	8	12	
3	7	11	15
2	6	10	14
1	5	9	13

But the true won position never can be obtained from the lost position. The problem, however, like squaring the circle, trisecting an angle, duplicating the cube, and finding the perpetual motion, has had a singular charm for many, and especially for those to whom the word impossible is as a red rag to a turkey.

The question relating to telescopes and microscopes I must leave others to answer; I have never possessed a £5 telescope, and have but little idea what an instrument can be made to do at that price.

The technical terms mentioned by "Twenty" are no more mysterious than the terms binocular (for two eyes), and monocular (for one eye), which he uses himself. An astronomical telescope is one which shows objects inverted (avoiding the loss of light which results from use of lenses for making the object appear upright); a terrestrial eye-piece is the tube (next the eye) containing such lenses; it is sometimes called an erecting eye-piece; an equatorial is one which, instead of turning round on an upright axis, and moving upwards and downwards round a horizontal axis, like ordinary terrestrial telescopes, is carried round an axis directed to the pole of the heavens, moving also on another axis, so as to be inclinable at any angle to the polar axis; and so forth. But any guide to the use of the telescope explains these points. We may presently publish in these pages some simple papers on such matters.—Ed.]

Queries.

19.—Comparative Anatomy of Birds and Animals.—Can you kindly tell me through your paper the corresponding bones in man and other mammals to the furcula of birds? I have consulted all the books to my hand, and cannot find out. Wishing all success to your interesting paper.—Charles Sherborn.

[20]—Orrery.—Can you or any of your readers kindly inform me where an Orrery can be seen?—Vulcan.

[21]—Deep-Sea Soundings.—Could you tell me where I can find an account of "Deep-Sea Soundings" taken off the coast of New Guinea?—Stanhope T. Speer.

[22]—Fomalhaut.—Is this visible in England at about 9 p.m.? My little daughter having decided, by a study of your star maps, in the affirmative.—Stanhope T. Speer.

[Yes, it is the star α, in constellation Piscis Australis, shown near the south-west horizon in map illustration No. 2.—Ed.]

[23]—Faure's Accumulator.—Would the editor oblige a reader of Knowledge by stating how De Faure's battery for the storage or accumulation of electricity is constructed, or in what publication such an account is to be found?—Nameless.

[24]—Dreams.—What position does science take on the subject of dreams? Does it deny positively that dreams ever have been sent as warnings, or that the warnings which dreams have apparently conveyed have ever been fulfilled except by accident? I have seen it stated that faith in dream warnings is as much out of date now as faith in astrology. Yet many accounts which have been given of dreams which have been apparently fulfilled, seem scarcely to be explained away so lightly.—A Dreamer.

[25]—Forms of Food.—Would not a short article, explaining the meaning of some of the terms used in Dr. Carpenter's interesting article on the "Relation of Food to Muscular Work," be of great use to many whose studies have not yet shown them the real meaning of such words as hydrocarbons, non-nitrogenous, and so forth?—Desdichado.

[26]—Training.—It seems to me that it would follow from Dr. Carpenter's theory of the "Relation of Food to Muscular Work," that the system of training followed by our athletes is unsound. Ought we not to return to the system of the ancients, who trained their athletes on barley cakes and oil?—Oarsman.

[27]—Effects of Marriage on the Death-rate.—I should be glad if the editor of Knowledge, or any reader who can give the desired information, could tell me how far it has been made out, or whether it has been made out, that marriage acts as a preservative. Is the death-rate of the married lower than that of the unmarried?—Benedict.

[28]—Stone on Rolling Wheels.—A long stone is rolled forward on wheels 2ft. in diameter, or say 75in. in circumference. There is no sliding. How far does the stone advance for each revolution of the wheels on the top of which it rests? A mathematical friend says the stone advances 150in.; but I cannot see how it can advance more than 75in.—Queensland.

A New Comparison of Poisons.—Comparative experiments with different poisons have often been made by injecting a given quantity of each into the veins of animals, and noting the effects. M. Richet has recently tried another method (which offers some advantages)—viz., poisoning the medium in which the animal breathes. If a fish be put in a poisonous solution, it dies sooner or later, according to the concentration of the poison. M. Richet adopts as the "limit of toxicity," the *maximum* quantity of poison (referred to one litre of water) allowing a fish to live more than 48 hours. This limit he has determined for various metals, always using the same acid radical—viz., chlorides. The limit of toxicity was calculated, not per weight of chloride, but per weight of combined metal. The figures show that there is no precise relation between the atomic weight of a substance and its poisonous power. Copper is 600 times as poisonous as strontium, though its atomic weight is less. Lithium, with an atomic weight only the twentieth of that of barium, is three times as poisonous, &c. Even with metals of the same family, no relation between the two things was discoverable. Cadmium (112) is only about half as poisonous as zinc (65); lithium (17) is 70 times as poisonous as sodium (23), &c. Nor could any relation be made out between the chemical function of a body and its toxical power. Thus, potassium and sodium, the chemical properties of which are so similar, have very unequal toxicity; one gramme of potassium is nearly 250 times as poisonous as one gramme of sodium. M. Richet means to prosecute the subject further.—*The Times.*

Replies to Queries.

[1]—Ultima Thule.—In answer to Query No. 1 in Knowledge, Nov. 11, "Alpha Sigma" will find "Ultima Thule" mentioned in Virgil's "Georgics," Bk. 1, line 30, in the sense of some remote country.—G. R. F.

This query is answered in a similar way by many correspondents.]

[1]—Ultima Thule.—The following account is given by Lemprière in his "Classical Dictionary":—"Thule, an island in the most northern parts of the German Ocean, to which, on account of its great distance from the continent, the ancients gave the epithet of *Ultima*. Its situation was never accurately ascertained, hence its present name is unknown by modern historians. Some suppose that it is the island now called Iceland, or part of Greenland, whilst others imagine it to be the Shetland Isles. *Stat.* 3. *Sylv.* 5, v. 20.—*Strab.* 1.—*Mela*, 3, c. 6.—*Tacit. Agric.* 10.—*Plin.* 2, c. 75, 1, 4, c. 16.—*Virg. G.* 1, v. 30.—*Juv.* 15, v. 112.—*Solin.* 20.—*Servius ad Virg. loco. cit.*—W. E. M.

[2]—A Fifteen Puzzle.—It is easier to solve the Fifteen Puzzle than to give the demonstration for which "Rusticus" seeks. The solution is as follows (or at least this is one solution): Let the fifteen girls be called A, B, C, &c., down to O, then the seven arrangements are these—

ABC	ADE	AFG	AHI	AJK	ALM	ANO
DIJ	BFH	BIK	BJL	BMO	BND	BGE
EFL	CKM	CLN	COE	CDF	CGI	CHJ
GKO	GJN	DHO	DGM	EIN	EHK	DKL
HMN	ILO	EJM	FKN	GHL	FJO	FIM

It is very easy for nine girls to go out on four days. The arrangement runs thus—

ABC	ADE	AFG	AHI
DGH	BFH	BDI	BEG
EFI	CGI	CEH	CDF

Mathematicus.

[4]—The Earth's Inclination.—Were the Earth to rotate on an upright axis, the greatest amount of heat would always exist at the equator, while the least would be at the poles. At all points of longitude between, the heat would be in proportion to the distance from the equator. I would express it so: perpetual summer would reign at the equator, as the sun would be always vertical to some point there, and perpetual winter at the poles, as the sun would always be on the horizon.—L. T. F.

[5]—Hot Winds, Cause of.—There is usually an inrush towards hot areas, but at times the action is reversed. There is no reason why at times a sandy region, like the Desert of Sahara, should not become a region of high pressure (especially when we remember the rapid radiation of heat at night), and in that case the flow of air would be from that region to surrounding regions, the air carrying before it the heated air. Again, the heated air which has flowed upwards may descend not far from the region of greatest heat, and travel as a hot wind from the hot, sandy region.—Meteorological.

Nature's Respirator.—The season of cold, raw, damp, and foggy weather is upon us, and many will be its victims. Among other causes, the inhalation of cold, and especially of dirty, air must take a high place. Man is, however, provided with a safeguard against this danger. The double passage to the lungs through the nose and through the mouth suggests some difference in use, and this become certain when we find such a difference in the two routes as actually exists. The air passing into the lungs through the nose in quiet respiration is warmed as it passes over the lower turbinated bone, with its very vascular mucous membrane, while, as the cavity is so narrow, it is also to a great extent filtered, and in this way deprived of its two dangerous characters even before it reaches the larynx. Those, therefore, who in the cold and in the fogs wish to avoid catarrhs should be careful to inspire only through the nose. With most this will require some practice, but it will be well repaid. Some, too, will find the impure air of a London fog very irritating to the nasal mucous membrane, and thus a demonstration of the irritant properties of the suspended matter which in breathing through the mouth gets free access to the lungs may be obtained. Those who are specially anxious to preserve their voice—as preachers, singers, and judges—stand in special need of this precaution, which is as effective as it is simple, and has many and great advantages over all the artificial respirators yet invented.—*Lancet.*

ANNIVERSARY MEETING OF THE BIRMINGHAM AND MIDLAND INSTITUTE UNION OF TEACHERS AND STUDENTS.

By W. Mattieu Williams.

In the first number of Knowledge I communicated a sketch of the history of the Scientific Department of the Midland Institute, so far as the classes are concerned. But there is another element, viz., the Students' own Associations for Mutual Improvement, which is well worthy of notice and imitation.

One of these, the Institute Scientific Society, has been remarkably successful. It possesses a scientific library of no mean character, and its members read admirable papers and carry on discussions of considerable interest. Some of these papers or lectures on the Birmingham trades, written by practical workers who, at the classes, have attained sufficient scientific knowledge to discuss the philosophy of their daily avocations, supply a kind of information not easily attainable from books or the lectures of ordinary professors.

The Union of Teachers and Students, another and larger society, held its anniversary gathering on Tuesday evening, Nov. 22. The programme included a tea-party, the whole arrangements of which were conducted by the female students, without external aid of contractor or purveyor. This was followed by a meeting in the new theatre, under the presidency of the Mayor, where an address was read by the retiring President of the Union, Mr. C. J. Woodward. The subject was the history of the institute. I must not be tempted to quote any more than the following—viz., that when Mr. Rickard commenced the Penny Arithmetic Classes, he had six pupils to the first lessons. This session the attendance to the first lesson was two hundred, and there are now held every week no less than sixty-five "Penny" classes on different subjects in the central institute and its branches, besides all the other classes.

Then followed a general *conversazione*, distributed through the various class-rooms and lecture-theatres, including an exhibition of microscopes contributed by the members of the Institute Scientific Society, scientific experiments by students of the Chemistry and Physic Classes, vocal music by members of the Singing Classes, German recitations, a French play—"Un Quartier Tranquille"—by members of the Institute French Dramatic Club, with the usual social and loyal conclusion of "Auld Lang Syne" and "God Save the Queen" by everybody.

Criticism would be out of place here, and description of details possibly tedious. I need only add that the whole programme was successfully carried out.

The attendance, which commenced with 450 at the tea-party, grew to above a thousand later in the evening, *i.e.*, after working hours.

The feature to which I wish to direct particular attention is the spontaneous, self-originating, and self-supporting character of these proceedings, and of all the other doings of these student associations. They constitute what appears to me to be a most important adjunct to the classes and public lectures of the Midland Institute, and one which may be very advantageously introduced in other kindred institutions, especially those of London. One of the most shallow and mischievous of popular delusions on the subject of education is the supposing that when we have completed a certain prescribed course of study, and passed our examinations on any subject, we have completed that part of our education—the fact being that all class teaching and all book reading is but the first stage of true, comprehensive education; self-teaching, original thought, the digesting and co-ordination of such school knowledge, must follow, to render it truly fruitful, and social co-operation in such supplementary work is most desirable. The meeting of old students with their younger successors, the revisiting of the old teachers, and sustaining of the old friendships between them and their former pupils, gives vitality and moral warmth to the whole institution, prevents the possibility of that decay which too often falls upon such institutions, when their existence is allowed to depend upon the efforts of outside patrons and the beneficence of mere endowments. Besides all this, the governing body is kept justly informed of the real requirements of the students—those who have good reason to be grateful to it, and know its workings by their own experience as former pupils, remain attached to it, join in its management, and otherwise substantially express their gratitude.

The genuine enthusiasm and hard-working efforts in carrying out the evening's programme, the genial friendship and high moral tone which I witnessed as pervading all the proceedings of Tuesday's meeting, convinces me that if such unions and friendly gatherings of teachers and students, old and young, male and female, should become one of the essential elements of all our literary and scientific institutions, their general prosperity and practical effectiveness would be greatly promoted.

Our Mathematical Column.

PRACTICAL USE OF LOGARITHMIC TABLES.

Let us now take a few examples of the practical use of a table of logarithms, noting that the former paper was intended to explain all that is necessary to be known respecting the theory of logarithms. I did not then think it necessary to draw any distinction between the logarithms of our tables and logarithms to any other base than 10; for the computers who mostly employ logarithms, use the decimal notation.

Let us first take the example afforded by Mr. Harding's calculation at p. 55, noting that the result, corrected for a "printer's error," is

$$= 21{\cdot}9912 . \frac{143303}{153664}$$

We have to take out the logarithms of these three numbers. Take first 21·9912. We turn to the number 2199 in the table and run our eye to the second column above which is the next digit, 1, getting the logarithm 3422450. (The first three digits of this are shown in the first column, the other columns only giving the next four for each number). But we still have to provide for the last digit, 2. Now we might do this from the part of the tables already used. Thus they show:—

logarithm of 2·1991 is 3422450,
and logarithm of 2·1992 is 3422647, or 197 more.

Now, we see that 2·19912 is only two-tenths of the way from 2·1991 to 2·1992, so that we should add only two-tenths of 197 to the logarithm of 2·1991 to get the logarithm of 2·19912, assuming that the logarithm increases, for such small differences, proportionately with the number of which it is the logarithm—which is shown to be true by the circumstance that we have the difference 197 or 198 (*often er the latter*) for several logarithms on either side of the one we are using. Manifestly if in passing from 21990 to to 21991 and thence to 21992, 21993, and so forth, we have the same difference*, the logarithm is here growing in the same proportion as the anti-logarithm (that is, as the number of which it is the logarithm). Hence, we take two-tenths of 198 (note italicised words above), or 39·6 (the nearest whole number to which is 40), and add this to 3422450, the logarithm of 2·1991, to get the logarithm of 2·19912. Thus

$$\log. 21{\cdot}9912 = 1{\cdot}3422490.$$

But we are saved even this slight labour by good tables. All tables give the difference as 198 in our example; but in good tables there is shown on the right the table of proportional parts, giving the amount to be added for digits 1, 2, 3, 4, 5, &c., respectively, and opposite 2 is set 40, the amount to be added.

Let us proceed in the same way with 143303 and 153664. We find in the tables, logarithm 1·4330 is 1562462, the "difference" is 303, and 3-10ths of this are 91, which added to 1562462 gives us

$$\log. 143303 = 5{\cdot}1562552.$$

Again, we find in the tables, logarithm of 1·5366 is 1865608, "difference" is 283, and 4-10ths of this are 113, which added to 1865608 gives

$$\log. 153664 = 5{\cdot}1865721$$

Thus, according to the principles on which logarithms are used, our "sum" is worked thus:—

$$\begin{aligned} \log.\ 21{\cdot}9912 &= 1{\cdot}3422490 \\ \log.\ 143303 &= 5{\cdot}1562552 \\ \hline \text{Sum} &= 6{\cdot}4985042 \\ \log.\ 153664 &= 5{\cdot}1865721 \\ \hline \text{Difference} &= 1{\cdot}3119321 \quad [=\log.\ 20{\cdot}508]. \end{aligned}$$

We now turn to the tables, and looking first along the left hand column of logarithms (next to the column of numbers) for the part 311 of the logarithm we have found. This comes next the number 2047, but running along this part of the tables for the remaining part, 9321, or what comes nearest to it, we find it opposite 2050 under the ninth column, corresponding to digit 8 (shown at the top of this column). The logarithm given here is 3119233 while that next larger is 3119445; the former is nearest to the logarithm above obtained, 3119321. Thus, if we are content with this degree of approximation to the result we want we write down 20508 as the digits representing that result, but as in the bracketed part of above computation we set the decimal point after the *second* digit, because our logarithm has 1 on the left of the decimal point. As a matter of fact, it would be absurd not to be content with this degree of approximation, simply because we cannot get more out of the

* The same difference, for though the actual difference alternates hereabouts between 197 and 198, this is only due to the circumstance that the last digit has to be the *nearest* to the true value, and cannot represent the exact value.

numbers we are using. Having 21·9912 correct only to the last place but one (the real number runs 21·991485, &c.) our result cannot be correct to a greater degree of approximation. Therefore, we set down only the third decimal figure.

If, however, our *data* would allow us to take more out of our logarithmic result than what was *put in* would permit, we could easily do it. Thus:—

log. of result = 1·3119321
log. of 20·508 = 1·3119233

Difference = 88

But we see from the tables that difference between the logarithms to 20509 and 20508 is 212, so that we must add to 20·580 $\frac{88}{212}$ ths of ·0001, or ·00088 divided by 212, or ·00012, making our answer 20·50812.

But it may be said all this is very long and complicated: might one not as well multiply 21·9912 by 143303 and divide the product by 153661. It will be found, however, that with a little practice it takes but a few moments to take out a logarithm, or to find the number corresponding to one. A few points have to be carefully attended to, which we shall discuss with further illustrations of more difficult examples in our next.

Our Chess Column.

LEAVING to next week the further analysis of the "Two Knights' Defence," we give this week an illustrative game in that opening, and a singularly pretty end-game which occurred in actual play with "Mephisto."

GAME No. 2.

Played by "Mephisto" and Mr. Marriott, of Nottingham.

Two Knights' Defence.

White. Mr. M.	Black. "Mephisto."
1. P. to K.4.	1. P. to K.4.
2. Kt. to K.B.3.	2. Kt. to Q.B.3.
3. B. to Q.B.4.	3. Kt. to K.B.3.
4. P. to Q.4. (a).	4. P. takes P.
5. Castles (b).	5. Kt. takes P. (c).
6. R. to K.sq.	6. P. to Q.4.
7. B. takes P.	7. Q takes B.
8. Kt. to Q.B.3.	8. Q. to K.R.4.
9. Kt. takes Kt.	9. B. to K.3. (d).
10. Kt. to K.Kt.3. (e).	10. Q. to Q.4.
11. B. to K.B.4.	11. Castles.
12. R. to Q.B.sq.	12. B. to K.Kt.5.
13. Kt. to K.4.	13. P to K.R.3.
14. P. to K.R.3.	14. B to R.4.
15. P. to Q B.4.	15. Q to K.B.4. (f).
16. Kt. to K.Kt.3.	16. Q. takes B.
17. Kt. takes B.	17. Q to K.B.4.
18. Kt. to K.Kt.3.	18. Q. to Q.2.
19. Q. to Q.R 4.	19. P. to K.Kt.3.
20. Kt. to K.4.	20 P. to K.B.4. (g).
21. Kt. to K.B.6.	21. Q. to K.Kt.2. (h).
22. R. to K.6. (i).	22. B. to Q.3. (j).
23. P. to Q.B.5.	23. Q to K.B.2.
24. Q.R. to K.sq.	24. B. to K.4.
25 Q.R. takes B. (k).	25. Kt. takes R.
26. Q. takes R.P.	26. P. to B.3. (l).
27. Q. to R.sq.(ch.)	27. K. to B.2.
28. Q. to R.4 (ch.)	28. K. to Kt.sq.
29. R. takes Kt.	29. Q. takes Kt.
30. R. to K.2.	30. P. to Q.6.
31. R. to Q 2.	31. K.R. to K.sq.
32. P. to Q Kt.4.	32. Q to B.6.
33. Q. to Kt 6.	33. Q. to B.5. (m).
34. P. to Q.R.4.	34. Q to Q R.3.
35. Q. takes Q.	35. P. takes Q.
36. K to B.sq.	36. R. to K.5.
37. R. to Q Kt.2.	37 P. to K Kt.4.
38. P. to K.Kt.4. (n).	38. P to K.B.5.
39. Kt. to Q 2.	39. R. to K.7.
40. Kt. to B.4.	40. P. to Q 7. (o).
41. R. to Q Kt sq.	41. P. to K.B.6.
42 Kt. to Q 6.	42. R to Q.2.
43. P. to Q Kt.5.	43. Q R. to K.2.
44. P. takes R.P.(ch.)	44 K. to R sq.

White resigns.

NOTES BY "MEPHISTO."

(a) Although, strictly considered Kt. to K Kt 5 is the stronger move, P. to Q.4 nevertheless gives the first player a good attacking game; it mostly leads to interesting variations of the Giuoco piano kind.

(b) If, instead of 5. Castles, White plays 5 P. to K 5, the game is transformed into a Giuoco piano. Black replies with 5 P. to Q.4, and by proper play he will withstand the subsequent attack of White, and retain the Pawn.

(c) If, instead of 5. Kt. takes P., Black plays 5. B. to Q B.4, then we have the Giuoco piano variation known as Max Lange's attack, from the name of its author, in which White again proceeds with P. to K.5, as before.

(d) 9. B. to K.2 is given as best for Black, but we prefer the move in the text.

(e) Here 10. B. to K Kt.5 is given by the books, with the object of preventing Black from Castling, if then P. to K.B.3, B takes P. The move in the text, however, shows a steady determination to attack the Queen's side, as shown in his 11th and 12th move.

(f) 15. P. takes P. *en pass.* would have been correct; Black had nothing to fear from White's attack; *e.g.*, if, instead of Q. to K.B.4, 15. P. takes P. *en pass.* 16 $\frac{\text{Q takes Q.}}{\text{R. takes Q.}}$ 17. $\frac{\text{Kt. takes P.}}{\text{B. takes Kt.}}$ 18. $\frac{}{\text{P. takes B.}}$ (if Kt. takes R. then B. takes Kt. with a far better game). 18. $\frac{\text{R. to K.B.4}}{}$ 19. $\frac{\text{R. to K.8 (ch.)}}{\text{K. to Q.2}}$ 20. $\frac{\text{R. to K.4.}}{\text{P. to K.Kt.4.}}$ with a good game.

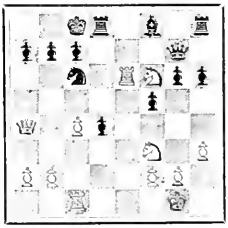
(g) This is weak. Black, by P. to K.Kt.3, tried to prevent the entry of the Knights into his game. The move in the text allows the Knight to play to K.B.6, and, if so inclined, afterwards to the commanding position on Q.5. Black ought to have played 20. B. to K.2, in order to prevent this, followed, perhaps, by P. to K.B.4.

(h) 21. Q. to K.B.2 was the correct move. White would then have obtained a very fine game by playing his Kt. to Q.5, followed by the advance of the Queen's Pawns.

(i) This is a very fine move, and initiates a combination belonging to the highest order of Chess play. We give a diagram of the position.

Position after White's 22nd move.

Black.



White.

(j) The position is one of great difficulty for Black. If Black plays 22. $\frac{}{\text{Q. to K.B.2}}$ 23. $\frac{\text{R. takes Kt.}}{\text{P. takes R.}}$ 24. $\frac{\text{Q. to R.6. (ch.)}}{\text{K. to Kt.sq.}}$ 25. Kt. to K.5 winning.

(k) In reply to 25. R. takes Kt. instead of Q.R. takes B. Black would play 25. B. takes Kt.

(l) The only move to save the game. If, instead, Kt. takes Kt. (ch.), with the idea of winning another piece, then Black loses his Queen, *e.g.*, 26. $\frac{}{\text{Kt. takes Kt. (ch.)}}$ 27. $\frac{\text{P. takes Kt.}}{\text{P. to B.3}}$ 28. $\frac{\text{R. takes P. (ch.)}}{\text{P. takes R.}}$ 29. $\frac{\text{Q. takes Q.}}{}$; whilst the move in the text wins.

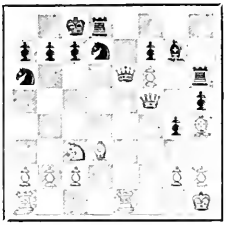
(m) White still tries for a favourable chance by pushing his Queen's Pawns on to the Black King, which plan Black, however, frustrates by forcing the exchange of Queens.

(n) Played to stop the advance of the Knight's Pawn; the Pawn cannot be captured, as, after P. takes P., and R. takes P., White plays Kt. to K.5.

(o) Threatening to Queen or win a piece if Kt. takes P.

Ending which occurred in an actual game played by "Mephisto" on Nov. 15, 1881, at 18A, Regent-street.

AMATEUR.
BLACK.



WHITE.
MEPHISTO.
Black to move.

White. MEPHISTO.	Black. AMATEUR.
. . . . .	1. Q. takes Q.
2. P. takes B.	

This fine move, as will be seen, wins the game. If White had played, B. takes Q. instead, then B. takes P., would give Black the better game.

2. Q takes B.

Best! for if the Queen should retire on B.4. or R.4., then White would play B. takes R., and his Pawn on Kt.7. could not be prevented from Queening, which would leave White with a Rook ahead and a winning position.

3. B. takes R.

This again is right, as will be seen

White.	Black.
	3. Q. to R.2.
4. R. to K.8. (threatening mate).	4. Kt. to B.3. If Black plays 4. P. to Kt.3. then White Queen's.

If White would play 5. B. to Kt.5, discovering check, and on the Black King moving to 5. Kt.2. 6. B. takes R. 6. Kt. to B.3. (best.) 7. R. to R.sq. 7. Q. to B.4. 8. Kt. to K.4. 8. Q. takes Kt. 9. B. to Kt.5. 9. Kt. to Kt.sq. (best.) Q. to R.; and White would have a very unsatisfactory game.

White.	Black.
5. B. takes Kt. ch.	5. K. to Q.2.
6. Q.R. to Q.sq. (ch.)	6. K. to B.3. (If K. takes R. 7. R. to Q.8. mate.)
7. R. to K.R. 8.	7. Q. to Kt.3.
8. R. takes R.	8. Q. takes R.
P. Queen's	Q. takes B.

And White mated in two moves.

CHESS QUERIES.—[2]—A few errors occur on page 20 of KNOWLEDGE which may confuse young chess-players, viz.:—*Game No. 1*: White's 17th move should be Kt. to Q.2; Black's 31th move should be R. to Q.R.5. *In Notes to Game No. 1*: (b) White's 18th move should be P. to Q.6; variation A, Black's 19th move should be B. to R.2; (l) White should be if K. to Kt. 3; (n) This note I do not understand; *In Notes on "Chessikin"*: Black's 7th move should be Q. takes R.P.(ch.); Black's 8th move should be K.Kt. takes P.—WHITE PAWN.

[3]—I beg to call your attention to what I think is an error in Mephisto's note (g), KNOWLEDGE, No. 1, page 20. Having 16. $\frac{\text{B. takes P.}}{\text{B. takes K.B.P.,}}$ it says if White takes B. Black would win his Queen by P. takes P. should this not be R. takes Q.?—Yours, G. N. SHERBORN.—[Mr. Sherborn misunderstands Mephisto's note. It is at move 15 that White cannot take B. Of course, if White played 16. B. takes B., Rook would take Q.—CHESS EDITOR.]

BLACK PAWN.—Your game is very interesting, and shall appear, with some additional notes.

ILL-HUMOUR.—A great deal of what we commonly call ill-humour springs from the ill-conditioned state of the body. We familiarly talk about people "rising from bed on the wrong side," and there is a very suggestive meaning in the phrase. They may have been sleeping all night in a very badly-ventilated room, or have gone to bed after taking a heavy and indigestible supper. The consequence is, that in the morning, from want of pure oxygen, or from overloading the stomach, the whole organism is out of order, the nerves are on edge, and they rise fretful and impatient, and continue so throughout the day.—Paterson's "Health Studies."

Our Whist Column.

By "FIVE OF CLUBS."

SO soon as we have accepted the general principle that in whist each player is to consider his partner's hand as well as his own, and that for this purpose each must inform his partner by every legitimate means of the nature of his own hand, we are at once able to decide on the proper way of conducting whist strategy. Were it otherwise, the first consideration of each player would naturally be the nature of his own hand. He would play so as either to make all his strong cards at once, or to adopt the course which seemed to him best for making them in the long run. If he had a short suit, he would try to get rid early of the cards of that suit, in order presently to trump the remaining good cards of the suit. And he would play his trumps solely with the object of making as many of them as he could. If every player followed such a course as this, the fortunes of the different hands would run very much as they do with good play, but the game would not be whist. It would be simply a chance game, each player's success depending on the number of good cards which happened to fall to his share, or on the fortuitous occurrence of short suits with opportunities for trumping them. The advantage of the scientific game is that it requires skilful strategy, and calls into action many useful faculties.

To tell my partner anything about the constitution of my hand, I must in the first place follow a systematic and generally understood method of selecting a suit to lead from, and, in the second place, I must open a suit so selected in the correct way.

Now, considering first the selection of a suit, we note that there is only one quality which, being common to all hands, can be adopted for systematic guidance. A player tells his partner nothing useful by playing out his good cards, even if he made the best use he could of them for himself by showing them at once. Leading from a short suit again is not only bad in itself—especially the atrocious lead from a single card which young players affect.—but it is not a method of leading systematically available, for not every hand possesses a suit of fewer than three cards. But every hand must possess a suit of four cards, at least—that is, a long suit. If, then, for no other reason, still for this, that, by so opening the hand, partner learns that one holds four, at least, of that suit (save in a few exceptional cases), the long suit would be a good one to lead, if that were always understood to be the meaning of the lead. But, apart from this, there is a manifest advantage—other things being equal—in leading from the long suit. This suit always has an element of strength, even though every card be small. Suppose, for instance, I have 2, 3, 4, 5 of a suit, an opponent have Ace, King, Queen, and the remaining six cards equally divided between the other players. Then, though we by no means advise a lead from 2, 3, 4, 5, if the holder of Ace, King, Queen drew three rounds, I should remain with 5; and, when trumps are drawn, that small card, if I get a lead, is as good as a trump; or, if I obtain a lead before all the trumps are drawn, that small card would either make a trick or draw a trump from the enemy as well as an Ace or a King. By leading from a long suit, and getting that suit so far exhausted that I have commanding strength in it, I secure an element of strength for my hand which comes next in efficiency to strength in trumps.

For the double reason, then, first and chiefly that in that way I can tell my partner the chief constituent of my hand; secondly, that by so playing I am likely to strengthen my hand, my first lead should be from my longest suit.

Of course, this rule, like all rules relating to a game so varied and complex as Whist, is not without exceptions.

I showed just now that a hand of four very small cards has a certain element of strength, which is wanting in a suit of the three highest cards; yet the latter has, of course, the greater strength. If you have two suits thus constituted, one long, but very weak, the other a three-card suit of great strength so far as the individual cards are concerned, you would be showing your partner best the chief constituent of your hand by leading from the shorter very strong suit, than by leading from the other. But a three-card suit must be very strong, or a four-card suit very weak, for the former to be preferred in this way. There are several reasons for this, besides the general reason that long-suit leads, followed systematically, instruct the partner best. A suit which is short with you is likely to be long with one or other of your opponents; and, if so, you are playing their game by leading it. Again, commanding cards of a short suit are more useful as cards of re-entry, that is to give you a lead later in the game, than they can possibly be if used early in the play of the hand. If your long suit is very weak, your partner will very soon find that to be the case, and by showing you where his strength lies, can serve your game, as a rule, better than you can serve his by opening a three-card suit, unless it be of absolutely commanding strength.

I — WHIST QUERY.—I have read your introductory article on Whist in this week's KNOWLEDGE, and was much interested by its good sense, but allow me to point out to you what seem to be two errors in the only illustration you give. You say, "I lead Ace, and follow with Queen of my best suit. My partner knows that I have the Knave and a small card left." This is by no means a necessary inference. It is a necessary inference, according to the best play, that the leader has not Ace, Queen, Knave, to five; and the play would be proper if he had Ace, Queen, Knave, to four; but he may equally have Ace, Queen, Knave alone, in which event he would also play Ace, followed by Queen.

You say also (and for this purpose I must assume the leader to have Ace, Queen, Knave, and a small one, and the partner to have King and two small ones, as in your illustration), "suppose that instead of following the recognised line of play for such cards, I lead the second round with my small card. My partner plays his King, and, let us suppose, wins the trick. So far as he can understand me at all he thinks I have three small cards of the suit left, and that the Queen lies with one of the adversaries." It is true that the lead would have simulated a lead from five, but why is it a necessary inference that the Queen is not with the leader. It is the commonest thing at Whist to lead from Ace, Queen, to five, Ace and then a small one. The only inference a partner from such play could draw (as the leader ought not to have both Queen and Knave, is that one or the other of these two cards was with one of the adversaries. Excuse my pointing out to you what seems to me to be worth your re-considering, if you ever collect your article for re-publication. I am, yours, &c.

FRED. H. LEWIS.

[Our correspondent is, of course, quite right in his criticisms. What the partner of leader in the case supposed would *know*, if lead were made correctly, is only that *Knave* remains with his partner. If, subsequently, he finds that his partner has not been forced to lead from a short suit, he knows that a small card of the suit remains; but otherwise he cannot be certain of this. In the case where the play is incorrect, a small card being played after Ace, the inference would be that three cards remain, and that Queen and Knave are certainly not *both* among the three. It is two to one (nearly) that Queen lies with an adversary, but it is not certain.

"FIVE OF CLUBS."

Answers to Correspondents.

*** *Communications which are to receive early attention should be addressed to the Editor of* KNOWLEDGE, *74 and 75, Great Queen-street, London, W.C.*

HINTS TO CORRESPONDENTS. 1. *No questions asking for scientific information can be answered through the post.* 2. *Letters sent to the Editor for correspondents cannot be forwarded; nor can the names or addresses of correspondents be given in answer to private inquiries.* 3. *No queries or replies savouring of the nature of advertisements can be inserted.* 4. *Letters, queries, and replies are inserted, unless contrary to Rule 3, free of charge.* 5. *Correspondents should write on one side only of the paper, and put drawings on a separate leaf.* 6. *Each letter, query, or reply should have a title, and in replying to letters or queries, reference should be made to the number of letter or query, the page on which it appears, and its title.*

DIXON K. Thanks; your letter being marked private, we assume you do not wish your remarks in *Field* to be touched on here.—MARTIN HINTON. See reply below to "Old Christian," in so far as they relate to the general subject on which you have written.—M. B. ALDER. Later.—J. C. H. W. You ask whether the remark of Huxley's, quoted at p. 4, is not incompatible with his views as a materialist. Of one thing you may be well assured—it represents his real views. Prof. Huxley's views as a materialist are indicated in his admirable lecture on Descartes' "Discourse." He says there: "When the materialists stray beyond the borders of their path, and begin to talk about there being nothing else in the universe but Matter, and Force, and Necessary Laws, and all the rest of *their* grenadiers, I decline to follow them." Perhaps if you carefully ascertain what *are* Prof. Huxley's views as a materialist, you may find them to be less inconsistent than you suppose with his opinion on "True Science and True Religion."—JOHN HAMPDEN. We are quite "aware that you take a position antagonistic to all the professional astronomers and geographers of the present day." We are not, however, "disposed to earn the everlasting gratitude of the upholders of the Newtonian theory by showing the impropriety of" your "charges against it." You say "if the professors are too cowardly to defend their own system, they are not competent to write upon any subject." When the Newtonian system needs defence, it will be time enough to undertake the work. How could reasoning avail with the followers of Parallax? They believe that the earth is shaped like a flat wheel about 25,000 miles in diameter, and that the sun travels in a path always parallel to this flat disc, and some 4,000 miles above it. On a spring or autumn day the sun's path, they tell us, is appreciably a circle, vertically above the equatorial circle, from which circle no part of the flat earth is more than 6,250 miles away! Reasoning would be thrown away on any one who could even for a moment imagine this to be the case. Repeatedly they have been shown what should be perfectly obvious—that, were this the case, (i) the sun could never set; (ii) that he would vary greatly in apparent size as seen from different places, or from the same place at different hours, or on different days; (iii) that all the phenomena of the heavens would be entirely different from those we see; (iv) that there could be but one pole of the heavens, and that many other consequences would follow which are utterly unlike any actually observed. Their answer to this has always been simply vituperation and abuse of those who happen to possess some reasoning power. It would be utterly contrary to the dictates of common sense to find room for any arguments against the absurdities of Parallax and his followers, in any journal intended for sensible beings. We will not insult our readers so grossly. To yourself personally, as undoubtedly believing what you profess to believe (incredible though such credulity may seem), we would remark (though the rudeness of many of your letters would justify absolute silence on our part) that the phenomenon which you said we should never see—viz., the sun to the south of the east and west points—in Australasia, we saw repeatedly, and that on every day the sun moved in the southern skies precisely as he should move on the accepted theory, and as he never could move were the theory of Parallax true. But we must ask our readers' forgiveness for making so unnecessary a statement, even in this corner of the paper, and in the smallest type our printers use.—LOMBARDIAN. We know of no Woman's Rights Association. There is a Society for the Protection of Women and Children, Secretary, Rev. J. G. Roberts, 85, Strand.—J. J. G. Portions of your letter appear in the "Correspondence" columns.—B. H. T. (F. C. S.) Thanks for your encouraging note.—W. C. T. Have not yet seen the work you mention. Doubtless the communications you promise will be of interest to our readers.—G. G. I can well believe it.—H. AMBROSE SMITH. The article on the Fifteen Puzzle to which you refer appeared in the *Gentleman's Magazine*, but the date and number of the magazine we do not know. It must have been some time last spring. We were in Australia when the article was written. It forms one of a series of "Familiar Science Essays," which Messrs. Chatto & Windus are about to publish.—M. Y. W. One tail omitted in engraving. The editorial note at foot of left hand column of page 36 related to a letter which was at the last moment removed to make room for another relating to the November meteors. The note should have been removed too.—G. G. D. and L. T. F. Your replies to "Tyro's" query in letter 9, involve the same difficulty which has perplexed him in the text books and primers. These all speak of the axis remaining in the same direction (they all use the knitting-needle illustration also); but "Tyro" finds a difficulty, it seems, in understanding what is meant by "the same direction." The expression, "moving always parallel to itself," is equally perplexing to many beginners. Your answers are, of course, quite right.—A. HART. "To settle a dispute," we inform you that though the planets are often called stars, a star is not a planet. We do "get light from a planet;" but it is not the planet's own light; it comes from the sun, and is reflected by the planet.—A. H. SWINTON. The general idea that, after all, the sun's heat may be some form of electricity, without confirmation by facts or reasoning, would hardly justify us in publishing so long a letter as yours.—AN OLD CHRISTIAN. With some regret, we find ourselves obliged to decline publishing your letter on science and religion. So far as we are concerned, we should have been glad to do so, because it chiefly relates to our own remarks on the same subject. But you emphasise the differences which you believe to exist between "the notions of modern scientists, and the plain statements of the Bible," and we emphatically decline to discuss the question whether such differences even exist. We are only concerned here with the question whether scientific statements are correct or not. Any scientific opinion expressed in these pages you are free to oppugn on scientific grounds. When you find a line here suggesting the opposition of scientific opinions, sound or otherwise, to any religious opinion whatsoever, then—and not till then—you, or any man, may express or defend religious views in these columns. "In such a then, I write a never." To use the words of Professor Huxley, "Science and Philosophy, within the range of which lie all the topics on which we can venture to speak here, are neither Christian, nor un-Christian, but are extra-Christian." I entirely agree with you that "no intelligent Christian" (or Buddhist, or Mahometan, you might have added) "can raise an objection to modern science on the ground that it enlarges unduly our ideas of the vastness of God's domain in space, or of the immensity of the time periods during which He acts;" but the objections raised by many well-meaning persons to modern scientific teachings amount in effect to this. They do not see that to believe in the evolution of a solar system or of a system of such systems, no more amounts to setting the Almighty on one side in the name of universal evolution, than does belief in the growth of a plant, or even of the tiniest bud or leaflet. It may save correspondents, and it will save ourselves some trouble, to define our position precisely, which we may do as follows:—"With religion, properly so called, all the truths of science are in necessary accordance; and so far, what is taught in these pages will be essentially religious; but this journal is intended to spread scientific knowledge; and DOGMATIC RELIGION SHALL HERE BE NEITHER ATTACKED NOR DEFENDED."—BIRD'S EYE VIEW. You may be very right, but this is hardly the place for advocating your views. *Remaining letters unavoidably crowded out.*

Contents of KNOWLEDGE *No.* 1.

Contents of KNOWLEDGE *No.* 2.

Contents of KNOWLEDGE *No.* 3.

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AN ILLUSTRATED
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PLAINLY WORDED-EXACTLY DESCRIBED

LONDON: FRIDAY, DECEMBER 2, 1881.

CONTENTS.

OUR FIELDS.

By E. W. Prevost.

IT is not probable that very many of the readers of KNOWLEDGE have ever thought, when walking through the fields, how those fields came to be in the condition in which they now are; not as regards the crops which they bear, but as regards the fine powdery state of the earth, when below are only to be found hard rocks. We do not propose to enter into the question why the rocks below occupy their present position, but rather to consider some of the means whereby rocks generally have yielded, and still do yield, a material different in character to themselves, and one suited to the growth of plants. For simplicity's sake we will assign the origin of a soil to two rocks known as Granite or Trap; for although a soil is not wholly nor directly derived from these two, yet the changes, such as those experienced by granite and trap, are the same in character, though differing in degree, in whatever be the rock under consideration.

Granite, a substance produced by the agency of heat in the early geological ages, is a mixture of three different minerals—quartz, a white lustrous mineral, also known in certain forms as rock-crystal, felspar, and mica. Felspar, of which there are several kinds, is a silicate of the metal aluminium, together with the silicates of potash, lime, or soda, according to the variety of felspar. Mica appears in the form of small bright shining scales, and is a silicate of potash or magnesia. Together with these three minerals, which may be called the elements of the soil, there are always present small quantities of other substances, such as iron, &c.

The trap rocks, divisible into two great classes, called diorite and dolerite, contain soda, lime, magnesia, and potash; and phosphoric acid is also present, but not to a large extent. Here, then, we have the materials necessary for the formation of soil, but its quality is dependent not only on the composition of the minerals contained in the rocks, but also upon the proportion in which they have contributed their constituent parts. The first agency to which the disintegration of a rock is due, reducing it from the original massive condition to that of a powder, is frost in conjunction with water. Most persons must be well aware of the fact that when water freezes, it expands to a considerable extent at the moment of solidifying, and that this expansion is irresistible, the vessels enclosing the water being destroyed. This same irresistible force is continually at work during the winter, destroying the cliffs on the sea-shore and inland; for there are always a number of clefts or fissures that admit of the entrance of water, which, when the temperature is low enough, is there frozen, and in freezing expands and splits the stone; and then, if the position be favourable, this splinter from the main rock falls down as soon as the ice melts, in consequence of a rise of the temperature. Again, the alternations of heat and cold, without the intervention of water, produce rifts, because the rock, being composed of substances which are not affected to the same extent by the same rise of temperature, expands unequally, and the result is the separation of the parts from one another. It is thus that the beds of torrents, rushing down glens, are in part filled by stones of various sizes, which, raising the level of the bed at the same time by their removal from the banks, alter the character of the glen. Even before the boulders become detached from the cliff, they are subjected to a slow but sure process of destruction, for every drop of rain which beats against the bare stone has its effect in wearing the surface away. In these processes of destruction we recognise mechanical agencies: but this is not all; chemistry plays also a very important part in reducing a block of stone to powder. The rain, in falling through the air, dissolves some of the oxygen and carbonic acid gas which it contains, and then, when this rain, holding carbonic acid in solution, comes in contact with the rock, a part of the more soluble portions is dissolved. It is, therefore, evident that the rain is a very powerful destroyer of stone, in that it works in two distinct ways—mechanically, whereby the rock is powdered, and chemically, whereby a part is carried away in solution.

Now, let us see what is the result of this partial solution of an apparently insoluble material. In the case of granite, bearing in mind that there are at least three different substances present, which are firmly and solidly mixed together, simple powdering would result in a separation from one another of the quartz, felspar, and mica; and if a current of water be passing over the rock at the time, the heavier particles would be left behind and the lighter ones would be swept away. This is what actually occurs: the quartz, finally known as sand, is left behind, while the lighter particles of felspar and mica are removed; now, seeing that the water, whether rain or river, contains carbonic acid, the felspar is slowly acted upon by the acid, is decomposed into silicate of alumina, and potash, which passes into solution, while the silicate of alumina remains behind or suspended in the water, and is now known as clay. As for the mica, it is not easily affected chemically, and may either be left behind with the clay, or, if the stream be rapid, it is carried away and deposited elsewhere. Such a process as is here described explains the destruction of a rock, and the consequent formation of beds of sand and clay, which are ill-fitted for the growth of plants, and to explain this we pictured to ourselves the rock in its original position, and undergoing all the processes at one time. To account for the production of loam, it is not necessary that every process should occur at once; but rather that, firstly, the boulder, removed by frost from the main rock, should be further broken up in the bed of a stream, and the particles so produced be then carried down by the stream and deposited where the current is less swift; this deposit, in time, becomes bare of water, as the stream alters its course, and finally becomes dry land. Now our new soil is not yet fit for plants, and as it consists of particles of the

original rock, it will, if subjected to the same influences, yield the same products when decomposed; lying exposed, as it does, to the rain and air, the slow changes above described take place, and we finally have a mixture of sand, clay, potash, lime, &c. To aid this slow action, which at the present day is not yet complete, the farmer ploughs his field, which, bringing up fresh soil from below, exposes it to the air and rain. We have before stated that the fertility of a soil is dependent upon the minerals of the soil and the extent of their decomposition. If, therefore, no aid is given to the solvent action of rain by ploughing, no great amount of fertility can be expected. The variety of felspar, whether it be a soda or a lime felspar, is of great importance—the second kind, when decomposed, producing tolerably fertile land, the former not so. When mica is present in large quantities, extreme infertility is the result, and for reasons before stated. It would occupy too much space to allude to all the various circumstances productive of the various classes of soils, and we therefore proceed to account for the production of the humus, or vegetable matter, which generally gives the dark colour, for a soil destitute of humus would be of varying shades of yellow and red. It has been proved by experiment that plants can grow without humus, or vegetable mould, and we need only refer to the growth of lichens on stones, &c., as an example; therefore, the first crop which grew on the new soil, whatever that crop or its origin might be, would flourish to a certain extent, and, dying, leave its remains behind; these would render the growth of the next generation of plants more vigorous; these, in their turn dying, the supply of humus would gradually increase. It might be supposed that the formation of humus would in time become excessive, but, as a rule, this is not so, because humus gradually decomposes, and in decomposing supplies carbonic acid to the soil, so aiding in the decomposition of the minerals present. In all these changes we see the marvellous usefulness of that gas which, under certain circumstances, is so fatal to man; but here we find it as a most useful servant, for not only does it supply air-food to the plant, being absorbed by the leaves, but, entering into the earth with the rain, it there prepares that food which the plant must have, and which it absorbs by its roots.

COMETS AND COMETS' TAILS.

By the Editor.

BEFORE we proceed to consider the theory by which alone, so far as can be judged at present, the phenomena of comets' tails can be explained, it may be well that we should consider the evidence derived from other comets than those hitherto considered.

In the first place we would direct special attention to the comet of 1811. In this comet, as may be seen from its picture in Fig. 1, the various parts of the comet and its tail could be distinguished by the naked eye. There was the condensed part, called the *nucleus*, which in this case was apparently globular in form; the nebulous envelope which surrounds the nucleus, the so-called *coma*; the bright side parts of the tail where it seems to be swept away from the coma, leaving a comparatively dark region behind the head, and the tail, widening and growing fainter with distance from the head. No one, we think, who considers this picture will for a moment imagine that the comet is a mere lens, and its tail merely the track of light condensed by this lens along the region behind the head. Here, again, the hollow structure of the tail seems indicated by the bright tracks on either side, though, as we shall endeavour to show later, the exceedingly well-defined nature of the dark track behind the nucleus in many comets seems to force upon us a different interpretation of this singular and characteristic feature.

In some respects the comet of 1811 tells us more of cometic possibilities, so to speak, than any other comet that has ever yet been observed. Discovered on March 26, 1811, this comet remained visible for a longer time than any yet seen, viz., for 16 months, 22 days. It had a tail 120 millions of miles in length, and 15 millions of miles in diameter at the widest part. The diameter of the nucleus was about 127,000 miles, that of the envelope round the head about 643,000 miles. But what was so remarkable about this comet was, that it obtained this remarkable development without approaching the sun, as other comets have done. The usual rule with comets is that the nearer they approach to the sun, the more their heads and tails are developed. But the least distance of the comet of 1811 from the sun was little less than 100 millions of

Fig. 1. Comet of 1811.

miles. Again, although it made so remarkable an appearance, as seen from the earth, the distance of that comet from us was at no time less than 110 millions of miles. Its true magnitude, therefore, as Professor Kirkwood well remarks, "has probably not been surpassed by that of any other comet which has yet been observed." If its path had carried it nearer to the sun, its appearance would probably have been terrible in the extreme. If we consider the enormous volume occupied by this comet and its tail, its

Million cubic miles of head,
Ten billion leagues of tail,

we shall see that the phenomena we have to interpret ought not to escape us in virtue of minuteness of scale.

Next consider the great comet of 1861. This comet was discovered on May 13, by Mr. John Tebbutt, jun., of New South Wales, and first accurately observed at the Sydney Observatory, on May 26. It passed northwards from the southern skies, and first became visible in Europe in the last week of June, 1861. The first recorded obser-

vations were made on the evening of June 30, nineteen days after it had passed its point of nearest approach to the sun. We remember well observing it on the morning of July 2, 1861. For some reason, we found it impossible to sleep that morning, and getting up about three in the morning (the exact hour we do not remember, but it must have been very early), we saw in the east what looked at first like the rays of an aurora borealis. But presently we noticed that these rays proceeded (unlike those of the aurora) from a bright centre, which had been hidden by clouds when our observations began. We used at that time to keep a four-inch telescope, mounted on a three-legged stand, in our bedroom. This we had quickly ready for action (noting that the object, owing to the approach of sunrise, was getting fainter every minute), and turning it on the comet, we drew a picture of the nucleus and coma so closely resembling that which appeared a week or two later in the *Illustrated London News*, that we might have supposed our picture had been surreptitiously sent to the office of the *Illustrated*, had we not found it resting just where we had put it in our scientific portfolio.

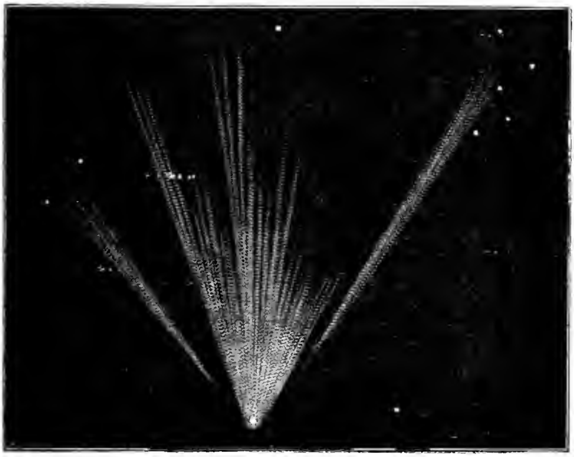


Fig. 2.

The comet appeared to the eye as shown in Fig. 2. Sir John Herschel, who observed it at Collingwood, in Kent, remarked that it was far more brilliant than any comet he had ever seen, not even excepting those of 1811 and 1858. The Padre Secchi, at Rome, found that in the clear skies of Italy the tail was fully 118° in length, corresponding to nearly one-third more than the distance between the horizon and the point overhead. This comet, by the way, though only favourably visible for a very short time, remained within the range of telescopic vision much longer. Hind remarks that the number of separate observations for the determination of its orbit exceeds 1,150, and extend over a period of 11½ months. It travelled on a course favouring observation, coming from remote distances south of the plane in which the earth travels to the northern side of that plane—and as it chanced, crossing the plane (about five-sixths of the way from the sun to the earth's orbit) just when the earth lay in the same direction from the sun, so that for a time she was within the bounds of the comet's tail-like appendage—and then travelling northwards on a path almost at right angles to the plane of the ecliptic. Thus the comet could be tracked on its retreat until, finally, distance concealed it from our view.

Now, the tail of the comet of 1861, as seen in Fig. 2, had something of the fan-like expansion observed in the tail of the comet of 1744; but what was known of the comet's position at the time when this fan-like form was seen, explained the peculiarity, and showed the necessity of taking into account the position of a comet before attaching undue importance to the apparent figure of its tail. For the fan-like form seen on this occasion was a mere effect of perspective. The end of the tail appeared very much wider than the part near the head—not that it really was so, but simply because it was very much nearer to the observer on earth. When we were actually immersed in the tail, the part nearest to us, being all round, had, to all intents and purposes, an infinite extension. But even when the comet was beyond that position, or a few days earlier, before it had reached it, the end of the tail was much nearer to us than the comet's head, and thus appeared far more proportionately widened than was actually the case.

Such considerations must always be taken into account in dealing with cometic phenomena. Comets, more than any other celestial objects (the Milky Way, regarded as a whole, being, perhaps, alone excepted), are affected in shape, and apparently, even in their very nature, by position, and consequent foreshortening.

SOLIDS, LIQUIDS, AND GASES.

By W. Mattieu Williams.

PART III.

THAT the solid and liquid states of matter are not distinctly and broadly separable, but are connected by an intermediate condition of viscosity, which is more or less common to both, has, I think, been sufficiently shown in the previous papers, and the proofs of this are familiar enough.

We now come to the question whether there is any similar continuity between liquids and gases. Ordinary experience decidedly suggests a negative answer. We can point to nothing within easy reach that has the properties of liquid and gaseous half-and-half; that stands between gases and liquids as pitch and treacle stand between solids and liquids.

Some, perhaps, may suggest that cloud-matter—London fog, for example—is in such an intermediate state. This, however, is not the case. White country fog, ordinary clouds, or the so-called "steam" that is seen assuming cloud-forms as it issues from the spout of a tea-kettle or funnel of a locomotive, consist of minute particles of water suspended in air, as solid particles of dust are also suspended. It has been called "vesicular vapour," on the supposition that it consists of minute vesicles, like soap-bubbles on a very small scale, but this hypothesis remains unproven. London fog consists of similar particles, varnished with a delicate film of coal-tar, and interspersed with particles of soot.

In order to clearly comprehend this question, we must define the difference between fluids and gases. In the first place, they are both fluids, as already agreed. What,

then, is the essential difference between liquid fluidity and gaseous fluidity? The expert in molecular mathematics, discoursing to his kinematical brethren, would produce a tremendous reply to this question. He would describe the oscillations, gyrations, collisions, mean free paths, and mutual obstructions of atoms and molecules, and, by the aid of a maddening array of symbols, arrive at the conclusion that gases, unless restrained, are liable to indefinite or vast expansion, while liquids, of their own accord, retain definite limits or dimensions.

The matter of fact experimentalist demonstrates the same by methods that are easily understood by anybody. I shall, therefore, both for my own sake and my reader's, describe some of the latter.

In the first place, we all see plainly that liquids have a surface, *i.e.*, a well-defined boundary, and also that gases, unless enclosed, have not. But as this may be due to the invisibility of the gas, we must question it further. The air we breathe may be taken as a type of gases, as water may of liquids. It has weight, as we may prove by weighing a bottle full of air, then pumping out the contents, weighing the empty bottle, and noting the difference.

Having weight, it presses towards the earth, and is squeezed by all that rests above it, and thus the air around us is constrained air. It is very compressible, and is accordingly compressed by the weight of all the air above it.

This being understood, let us take a bottle full of water and another full of air, and carry them both to the summit of Mont Blanc, or to a similar height in a balloon. We shall then have left nearly half of the atmosphere below, and thus both liquid and gas will be under little more than half of the ordinary pressure. What will happen if we uncork them both? The liquid will still display its definite surface, and remain in the bottle, but not so the gas. It will overflow upwards, downwards, or sideways, no matter how the bottle is held, and if we had tied an empty bladder over the neck before uncorking, we should find this overflow or expansion of the gas exactly proportionate to the removal of pressure, provided the temperature remained unaltered. Thus, at just half the pressure under which a pint bottle was corked, the air would measure exactly one quart, at one-eighth of the pressure one gallon, &c.

We cannot get high enough for the latter expansion, but can easily imitate the effect of further elevation by means of an air-pump. Thus, we may put one cubic inch of air into a bladder of 100 cubic inches capacity, then place this under the receiver of an air pump, and reduce the pressure outside the bladder to $\frac{1}{100}$th of its original force. With such atmospheric surrounding, the one cubic inch of air will plump out the flaccid bladder, and completely fill it. The pumpability of the air from the receiver shows that it goes on overflowing from it into the piston of the pump as fast as its own elastic pressure on itself is diminished.

Numberless other experiments may be made, all proving that all gases are composed of matter which is not merely incohesive, but is energetically self-repulsive; so much so, that it can only be retained within any bounds whatever by means of some external pressure or constraint. For aught we know *experimentally*, the gaseous contents of one of Mr. Glaisher's balloons would outstretch itself sufficiently to occupy the whole sphere of space that is spanned by the earth's orbit, provided that space were perfectly vacuous, and the balloon were burst in the midst of it, and the temperature of the expanding gas were maintained.

Here, then, in this self-repulsiveness, instead of self-cohesion, this absence of self-imposed boundary or dimensions, we have a very broad and well-marked distinction between gases and liquids, so broad that there seems no bridge that can possibly cross it. This was believed to be the case until recently. Such a bridge has, however, been built, and rendered visible, by the experimental researches of Dr. Andrews; but further explanation is required to render this generally intelligible.

Until quite lately it was customary to divide gases into two classes—"permanent gases" and "condensable gases" or "vapours." Gaseous water or steam was usually described as typical of the latter; oxygen, hydrogen, or nitrogen of the former. Earlier than this, many other gases were included in the permanent list; but Faraday made a serious inroad upon this classification when he liquefied chlorine by cooling and compressing it. Long after this, the gaseous elements of water, and the chief constituents of air, oxygen, hydrogen, and nitrogen, resisted all efforts to condense them; but now they have succumbed to great pressure and extreme cooling.

We thus arrive at a very broad generalisation, viz., that all gases are physically similar to steam, (I mean, of course, "dry steam," *i.e.*, true invisible steam, and not the cloudy matter to which the name of steam is popularly given,) that they are all formed by raising liquids above their boiling-point, just as steam is formed when we boil water and maintain the steam above the boiling-point of the water.

But some liquids boil at temperatures far below that at which others freeze; liquid chlorine boils at a temperature below that of freezing water, and liquid carbonic acid below even that of freezing mercury, and liquid hydrogen far lower still. These are cases of boiling, nevertheless, though it seems a paradox according to the ideas we commonly attach to this word. But such ideas are based on our common experience of the properties of our commonest of liquids, viz., water.

When water boils under the conditions of our ordinary experience, the passage from the liquid to the gaseous state is a sudden leap, with no intermediate state of existence that we are able to perceive; and the conditions upon which water is converted into steam—the liquid into the gas—while both are at the bottom of our atmospheric ocean, are such as to render an intermediate condition rationally, as well as practically, impossible.

We find that the expansive energy by which the steam is enabled to resist atmospheric pressure is conferred upon it by its taking into itself, and utilising for its expansive efforts, a large amount of calorific energy. When any given quantity of water is converted into steam, under ordinary circumstances its bulk *suddenly* becomes above 1,700 times greater—a cubic inch of water forms about a cubic foot of steam, and nearly 1,000 degrees of heat (966·6) disappears *as temperature*. Otherwise stated, we must give to the cubic inch of water at 212° as much heat as would raise it to a temperature of 212 plus 966·6, or 1178·6° if it remained liquid. This is about the temperature of the glowing coals of a common fire; but the steam that has thus taken enough heat to make the water red hot is still at 212°—no *hotter* than the water was while boiling.

This heat, which thus ceases to exhibit itself as *temperature*, is otherwise occupied. Its energy is partly devoted to the work of increasing the bulk of the water to the above-named extent, and partly in conferring on the steam its gaseous speciality—that is, in overcoming liquid cohesion, and substituting for it the opposite property of internal repulsive energy which is characteristic of gases. My reasons for thus defining and separating these two functions of the so-called "latent" heat will be seen in the next paper, when we come to the philosophy of the interesting researches of Dr. Andrews.

GERMS OF DISEASE AND DEATH.

By Dr. Andrew Wilson, F.R.S.E.

Part II.

Turning now to the *Charbon* or *Splenic Fever*, we witness another veritable triumph of Pasteur's industry and research. In 1850, certain observers noted the interesting fact that minute, rod-like bodies, which appeared to be lower forms of plant life, existed in the blood of animals affected with this disease. The "rods," it was observed, originated from particles which might, with perfect accuracy, be called "Germs." And as we watch the "rods" in turn, we see that, sooner or later, microscopic specks appear in their substance: these grow to form regular bead-like rows within the "rods;" and when finally the "rods" themselves break up and fall to pieces, these beads are liberated as the "germs," which in time will grow into new rods. Thus countless myriads of rods and germs grow and are reproduced within the body of the animal suffering from *splenic fever*. The fever, in a word, is the result of the growth and development within the living soil, of these rod-like plants. But exact demonstration of the truth of the latter statement can be had.

If we grow and cultivate in a proper fluid—such as the aqueous humour of the eye of an ox—the "rods," we may inoculate with our "rods" the body of a healthy animal. We may *sow* in that animal's body the germs of splenic fever. Thus a drop of a solution containing the "rods" sown within the body of a guinea-pig, produces splenic fever in that animal. And more wonderful still, it has been shown that the dried blood taken from an animal affected with this fever will reproduce the fever, even after an interval of four years, if the dried particles of blood with their "rods" be introduced into the body of a healthy animal.

Pasteur, armed with knowledge of the kind just detailed, set himself to ascertain the "reason why" splenic fever should suddenly appear in districts which knew it not, and wherein only healthy animals lived. Obviously, if the germ theory were true, such sudden and apparently isolated outbreaks must be capable of being explained on this hypothesis. The idea of the "spontaneous," or *de novo*, or *ex nihilo* origin of the disease would, if supported by facts, prove fatal to the "germ theory." Here, then, was a typical case for scientific investigation. Let us see how the genius of Pasteur overcame the difficulties of the situation.

The localities in which splenic fever seemed to burst out suddenly and without warning were, as Pasteur learned, former seats of the disease. But the interval between the visitation was to be measured by years. How, then, could the new outbreak be accounted for? It seemed, in truth, as if the one outbreak had little or nothing to do with the other. The infected animals which had died, or had been killed, whilst suffering from the fever, were duly buried, and that very deeply, in the soil. Such a method of interment would seem to obviate all risk of infection. But the possibilities of nature are illimitable, and no man knew this better than Pasteur. If the poison had been buried in the soil, why should it not be there still? And, further, why should it not be conveyed upwards to infect the fresh flocks that fed on the graves of their predecessors? With a gift of scientific divination, Pasteur sought in the earthworm, the type of the "middleman" betwixt the living and the dead. He now examines the bodies of the worms which live in the soil wherein the bodies of the animals infested with splenic fever, years before, were entombed. By experimental means, he solves his problem. He makes a preparation of the contents of the digestive system of the worms. This he administers in the food of healthy animals, entirely removed from the pastures. And once again a scientific principle dawns in view. The rabbits and guinea pigs which devoured the matter obtained from the worms at once developed splenic fever, whilst in their blood the rods were seen developing in full force. Once again Pasteur had sown the fever, and had argued thus from the result, backwards to the cause. It has also been proved that even grain may convey the subtle "rods" to healthy animals, and may in this way engender splenic fever. Following close upon the heels of the discovery of the germ-origin of this fatal malady comes the gratifying announcement that, as small pox is modified by vaccination, so splenic fever may be modified by an analogous process. Pasteur has proved that we can inoculate sheep and cattle with a mild form of the fever which protects the animal from a recurrence of the disease; and this protective influence, as we write, is being practically utilised by the breeders of France.

Such is a brief recital of a new step towards a perfect knowledge of the nature of the diseases which decimate, not merely animal life, but human existence as well. It may not be inappropriate if, by way of close, we remind our readers of two very noteworthy points in connection with this all-important topic, bearing, as it does, in the most intimate manner upon the physical welfare of man.

The first of the points to which we refer concerns the apparently trivial origin of an all-important subject. It was in the city of Florence, some two hundred or more years ago, that a certain physician, Francesco Redi by name, demonstrated to the Florentine wiseacres that the maggots in meat do not arise from the dead meat by "spontaneous generation," but were produced from the eggs of the flesh-flies. This result he achieved by covering over the meat with gauze, so that whilst the meat-decay proceeded, there was likewise a convincing absence of maggots. Childishly simple as was Redi's experiment, it laid the basis and method of all succeeding research; for from his day down to ours the progress of the "germ theory"—or of that doctrine which holds that all life, however mysteriously generated, must spring from pre-existing life—has been uniform and triumphant.

But the second point to which attention is worthy of being directed, exists in the statement that the practical and actual benefits which have flowed to human health, and which are likely to flow in the future as well—the saving of life by the prevention and extermination of disease—arise from a simple study in natural history. So-called "practical" minds are often given to loudly express their disapproval of any science which deals with what, to them, seem mere abstractions. Doubtless, to such minds the study of the development of the "rods" of splenic fever under a watch-glass must seem a piece of scientific *dilettantism*; just as information respecting the solar system may seem despicable enough, because its results cannot be measured by a profitable currency, or, in plain language, because it "doesn't seem to pay." The best answer to such foolishness is found in a recital of the results to human and animal life to which natural history study seems likely to lead. Just as two hundred years ago, in Florence, Redi began the good work by a simple study in zoology, so to-day we are reaping the reward of the earnest work of the botanists and zoologists who toil and labour to spread abroad their saving knowledge.

BRAIN TROUBLES.

Impaired Memory (*continued*).

The following case is one of those in which sudden failure of memory implied serious cerebral mischief. "An eminent provincial surgeon, of large and anxious practice, was seized with a sudden failure of memory. He forgot all his appointments, and to such a degree was the faculty of retention impaired" (so far as the names and cases of patients were concerned) "that he was obliged to make memoranda of every trifling and minute circumstance which it was important for him to remember, and to these he was constantly referring in order to refresh his memory. This attack was preceded by headache, of which he had complained for nearly a fortnight." Up to the period of the case being brought to the attention of Dr. Forbes Winslow, who treated it, no suspicion had been entertained of the existence of any prior state of cerebral ill-health, sufficient to account for the patient's sudden loss of mental power. Dr. Winslow ascertained, however, that "about eight weeks, or nearly three months previously" (not a very clear way of putting the matter, by the way), the patient had been seized, whilst in the act of applying a stethoscope to the chest of a patient, with severe epileptic vertigo. For about a second he lost consciousness. This had been succeeded by an attack of distressing sick headache. "Three days subsequently he had a second paroxysm of giddiness, and nearly fell out of the carriage in which he was sitting at the time. His spirits subsequently became much depressed, but in a few days he again rallied, flattering himself that he had quite recovered. He made no mention of these attacks to any member of his family, and carefully avoided all conversation on the subject of his health with his medical brethren." "When I saw this gentleman," says Dr. Winslow, "the only appreciable mental symptom was inability to retain in his mind, for many consecutive minutes, any recent impressions. His pulse was feeble, face pallid, and general health shattered. His spirits were, however, at times buoyant, and the prognosis which he formed of his own case was favourable." The result showed that he was a false prophet. Two weeks later he had an epileptic fit. He then became rapidly worse, and ten months after he died "in a deplorable state of mental imbecility."

But against such a case as this, which was obviously exceptional, may be set the following case, in which, under similar conditions, so far as appearances were concerned, a complete cure was effected:—A barrister complained to Dr. Winslow of occasional attacks of enfeebled memory. "He attributed this mental impairment," says Dr. Winslow, "to the fact of his having been engaged as counsel the previous year in several anxious and severely-contested election cases. I advised an entire cessation from all professional work, but had great difficulty in persuading him to recognise the necessity for a complete abstinence from mental occupation. He promised a guarded acquiescence in my strict injunctions, but finding himself relieved after an interval of a few weeks, he returned, in opposition to my solicitations, to his chambers, and recommenced active practice. As I predicted," proceeds Dr. Winslow, "he soon broke down, and I was once more conferred with. He then acknowledged it to be a matter of vital necessity that he should give his mind prolonged rest, and agreed unreservedly to do so. I kept him for a period of *two years* from all anxious and severe mental occupation, and by that time his powers of mind had rallied to a surprising extent; in fact, they became, according to his own impression, more vigorous than they were prior to his attack of illness. For many years this patient has continued steadily at work, never having had a return of loss of memory. I should premise that I exacted from him a promise that he would read no briefs after dinner. He has rigidly adhered to this understanding, but being an early riser and a man of remarkable quickness of apprehension, he is enabled to master a large amount of work before breakfast. I also made it a *sine quâ non* that he should go abroad every year for a period of two months, thus ensuring for him a complete diversion and relaxation of mind from all injurious pressure. He has scrupulously complied with my instructions, and the result is an entire freedom from all symptoms of mental impairment and cerebral disorder." A case such as this is full of encouragement, because here it would seem that at the outset overwork had seriously injured the brain, yet attention to a few simple rules resulted in a complete cure.

Apart from actual injury to the substance of the brain, transient loss of memory seems to be usually caused by a deficient supply of blood to the brain, whether through loss of blood generally, or owing to defective circulation. This is illustrated by the following case: A lady had been reduced to a state of such extreme prostration by hæmorrhage, that for nearly a week she seemed simply lingering between life and death. After this she remained for a long time in a state of extreme mental depression and vital prostration. When she was able to articulate, her husband was astonished to find that her memory was paralysed. "She had forgotten where she lived, who her husband was, how long she had been ill, the names of her children, and, in fact, her own name was obliterated from her recollection. She was unable to call anything by its right name. In attempting to do so she made the most singular mistakes. She had been in the habit, before her illness, of speaking in French, her husband being a Frenchman; but while in the state of mind described, she seemed to have lost all recollection of the French language. When her husband spoke to her in French, she did not seem to understand in the least what he was saying, though she could at this time speak English without difficulty. Seven or eight weeks elapsed before her memory began to improve, and months passed before her mind regained its original strength."

Intense cold seems to have the power of paralysing the memory. During the retreat from Moscow, many of Bonaparte's officers and men found their memories greatly enfeebled. Bonaparte himself was affected, especially as to dates and names. "For a time he was constantly confusing one person with another, and making odd mistakes in dates." In his case the affection of the memory lasted only a few days;* but one of Bonaparte's *aides-de-camp* lost his memory for several years.

Instances such as these enable us to understand the true meaning of those comparatively slight attacks of failure of memory which most of us experience from time to time. In the first place, we do not find much evidence enabling us to assign to one or other of the two classes of memory-failings above indicated a greater or less degree of importance, whether such failings occur in a marked or slight

* During this time Bonaparte's mind seems to have been affected. "He merely made some gestures of melancholy resignation on every occasion when," during the battle of Semenowska, the *aides-de-camp* sent by Ney "informed him of the death of his best generals. He rose several times to take a few turns, but immediately sat down again. Everyone looked at the Emperor with astonishment. Hitherto, during these great shocks, he had displayed an active coolness; but here only a dead calm, a mild and sluggish inactivity." Count Segur, referring to Napoleon's state at this time, says: "The Russian autumn had triumphed over him."

degree only. Inability to commit new matter to the memory with customary facility seems as likely to be a sign of mischief as inability to recollect matters forming (ordinarily) a part of our stock of familiarly known facts. Again, it is clear we need not fear that mind is necessarily going astray because for a time the memory fails in slight degree. We see that very serious failures of the power of memory may occur where the brain has suffered no irreparable mischief. But since we see that much overwork will cause serious temporary mischief of this particular kind, we learn that where a slight lapse of memory is noticed, the indication may be taken as a sign that rest is needed. But there are, as we have seen, other ways in which this special power may come to be affected; so that if the memory should show signs of failure where we have no reason to believe that overwork has caused the mischief, we may infer that some one or other of the causes which, as we have seen, may affect the memory seriously have operated injuriously in slight degree. Nor in general need we be in much doubt as to the true nature of the cause, simply because we cannot fail (usually) to recognise in the circumstances preceding the attack the origin of the mischief. Thus, although a serious failure of the memory considered apart from the circumstances preceding it might leave the physician in doubt whether depletion or plethora (to mention two possible causes) had produced the mischief, yet the physician, apart even from an examination of the patient's condition, could learn at once from him whether either of these two opposite conditions had existed before the attack. In like manner, any person whose memory suddenly seemed weakened could, as his own physician, ascertain (unless, indeed, his memory failed to remind him how he had passed the hours or days preceding the attack) whether the mischief resulted from deficiency or excess in the amount of food or stimulants he had previously taken, whether the proper remedy would be, on the one hand, some such medicine as a glass of wine and a chop, or, on the other hand, a diminution during two or three days of the amount of food consumed or the avoidance of some of the more stimulating articles of diet. Here, however, we are considering rather those mental troubles which are produced by mental work, whether relating to subjects of great difficulty or carried on too long. We would notice also that in dealing with other indications of mental mischief we need not be careful to show how the more serious cases of each kind suggest the significance of the slighter and far commoner mental troubles which form our real subject of inquiry; for this reason, simply that what we have here said about failure or loss of memory applies equally to other signs of temporary mischief.

MAN A FRUIT-EATER.*

MAN'S nearest of kin among the animals is the ape. This is shown not only by those outward features which all can recognise, but more clearly and more certainly by the structure of the nervous system. The animal in which this system resembles most closely the nervous system in man is the ape, and of all apes, that which comes nearest to man in this respect is the orang. The brain convolutions, which in rodents (gnawing quadrupeds—rats, squirrels, &c.) and edentates (toothless quadrupeds—ant-eaters, ground-hogs, &c.), are very simple, in the flesh-eating animals are more developed, and in the apes, especially the orangs, they are developed still more fully. "We are authorised in concluding," says Professor Mivart, that "the difference between the brain of the orang and that of man, as far as yet ascertained, is a difference of absolute mass; it is a difference of degree, and not of kind."

Starting from this relationship, Miss Kingsford, in the book before us, proceeds to indicate the bearing of man's kinship to apes on the vexed question of man's proper or natural food. Carefully studying the entire digestive apparatus of animals and men, and especially comparing this apparatus in men and apes, she is led to the conclusion that man approaches nearest in this respect to those animals which are eaters of fruits and herbs. "If," she says, "we have consecrated to this sketch of comparative anatomy and physiology a paragraph which may seem a little wearisome in detail, it is because it appears necessary to combat certain erroneous impressions affecting the structure of man, which obtain credence, not only in the vulgar world, but even among otherwise instructed persons. How many times, for instance, have we not heard people speak with all the authority of conviction about the 'canine teeth' and 'simple stomach' of man as certain evidence of his natural adaptation for a flesh diet? At least we have demonstrated one fact, that if such arguments are valid, they apply with even greater force to the anthropoid apes—whose 'canine' teeth are much longer and more powerful than those of man—and the scientists must make haste, therefore, to announce a rectification of their present division of the animal kingdom in order to class with the carnivora (flesh-eaters) and their proximate species all those animals which now make up the order primates (men and apes). And yet, with the solitary exception of man, there is not one of these last which does not in a natural condition refuse to feed on flesh!" Pouchet says that all the details of man's digestive apparatus, as well as his dentition, are proofs of his frugivorous (fruit-eating) origin. Professor Owen agrees that the close analogy between apes and man demonstrate his frugivorous nature. So Cuvier, Linnæus, Lawrence, Bell, Gassendi, Flourens, and a host of other authorities.

Yet another belief is as common as it is erroneous, viz., that "flesh food contains the elements of physical force, and that to be strong, robust, and endowed with muscular energy it is necessary to partake largely of animal food." Yet no flesh-fed animal rivals in strength the herb-eating rhinoceros, in endurance the horse, the mule or the camel. A gorilla feeding on fruits and nuts is more than a match for the far heavier lion. "The buffalo, the bison, the hippopotamus, the bull, the zebra, the stag, are types of physical power and vast bulk, or of splendid development of limb. Only in ferocity are flesh-eating animals superior (?) to those who find their food in fruits and herbs."

As regards man himself, the idea that the flesh-eaters are the most powerful, is erroneous, as is the cognate idea that to acquire strength, a man should eat daily large quantities of flesh meat. "In the palmy days of Greece and Rome, before intemperance and licentious living had robbed those kingdoms of their glory and greatness, their sons, who were not only soldiers but heroes, subsisted on simple vegetable food, rye meal, fruits, and milk. The daily rations of the Roman soldier were one pound of barley, three ounces of oil, and a pint of thin wine. It was no regimen of flesh that inspired the magnificent courage of the Spartan patriots who defended the defiles of Thermopylæ, or that filled with indomitable valour and enthusiasm the conquerors of Salamis and Marathon." Among the nations of

* "The Perfect Way in Diet; a Treatise advocating a return to the natural and ancient fruit of our race." By Anna Kingsford, Doctor of Medicine of the Faculty of Paris. (London: Kegan Paul, Trench, & Co.)

to-day, also, we find the fruit eaters and herb eaters as enduring, to say the least, as the flesh eaters, and healthier.

Are we then to infer with our author that a diet of fruit and seeds, preferably uncooked, is the best for the human race? Or, if we infer this, may we conclude that all would do well to adopt such a diet? It might be unsafe to accept the latter inference, for habit and custom count for something in such matters. But we may very safely adopt the opinion, now generally prevalent among experienced physicians, that fruit and seed, herbs and vegetables, should form a larger proportion of our food than they do. Precisely as many who do not accept, in its entirety, the views of Dr. Richardson about alcoholic stimulants, yet hold that these stimulants, if taken at all, should be taken in much smaller quantity than is customary, so, many who would not agree with Miss Kingsford, that animal food should be entirely displaced (which is Dr. Richardson's opinion also), yet see that it would be well if flesh meat were taken in much less quantity than at present.

How much custom has to do with the use and effects of flesh meat is shown by cases such as Miss Kingsford mentions, in which persons unaccustomed to flesh meat have been actually intoxicated by its use. Dr. Dundas Thompson tells us of some Indians accustomed to vegetable food, who, dining luxuriously on meat, showed an hour or two later, by their jabbering and gesticulations, that the same effect had been produced upon them as if they had taken some intoxicating spirit or drug.

Apart from the special doctrine which Miss Kingsford advocates, her little treatise is well worth studying for its clear and correct account of the various forms of food used by man. There is much matter for reflection, also, in what she says about slaughter-houses, fox-hunting, pigeon-shooting, and the cruelties of the fur trade.

THE SPECTROSCOPIC ANALYSIS OF LIGHT.—We may illustrate this process by a similar one, which we might imagine mankind to perform. Suppose Nature should loan us an immense collection of many millions of gold pieces, out of which we were to select those which would serve us for money and return her the remainder. The English rummage through the pile, and pick out all the pieces which are the proper weight for sovereigns and half-sovereigns; the French pick out those which will make 5, 10, 20, or 50-franc pieces; the Americans the 1, 5, 10, and 20-dollar pieces, and so on. After all the suitable pieces are thus selected, let the remaining mass be spread out on the ground according to the respective weights of the pieces, the smallest pieces being placed in a row, the next in weight in an adjoining row, and so on. We shall then find a number of rows missing—one which the French have taken out for 5-franc pieces; close to it another which the Americans have taken for dollars; afterwards a row which have gone for half-sovereigns, and so on. By thus arranging the pieces, one would be able to tell what nations had culled over the pile, if he only knew of what weight each one made its coins. The gaps in the places where the sovereigns and half-sovereigns belonged would indicate the English, that in the dollars and eagles the Americans, and so on. If, now, we reflect how utterly hopeless it would appear, from the mere examination of the miscellaneous pile of pieces which had been left to ascertain what people had been selecting coins from it, and how easy the problem would appear when once some genius should make the proposed arrangement of the pieces in rows, we shall see in what the fundamental idea of spectrum analysis consists. The formation of the spectrum is the separation and arrangement of the light which comes from an object on the same system by which we have supposed the gold pieces to be arranged. The gaps we see in the spectrum tell the tale of the atmosphere through which the light has passed, as in the case of the coins they would tell what nations had sorted over the pile.—*Newcomb's Popular Astronomy.*

A MILD NOVEMBER.

THE weather of the month that has just elapsed has been so unusually mild, that a few statistics respecting mild Novembers in general, and the past one in particular, may not be without interest.

It appears that in London the average temperature of the first twenty-five days of the past month has been as much as 7° above the mean of twenty years' observations; and, if it is fair to compare the London temperatures with those of Greenwich, the past November has been decidedly the warmest experienced during the present century. Relatively warm Novembers were observed in the following years: 1806, 1817, 1818, 1821, 1822, 1824, 1845, 1846, 1847, 1850, 1852, 1857, 1863, 1865, 1866, and 1877. The warmest of these was 1852, when the mean temperature in London was 48·9°, or rather more than a degree lower than that of the month that has just passed away. The maximum readings observed during the month under review have been exceptionally high, and those of the 5th and 10th, when the thermometer in the shade rose to 64°, have only been exceeded once during the present century. This was on Nov. 8, 1847, the reading then being as high as 67°.

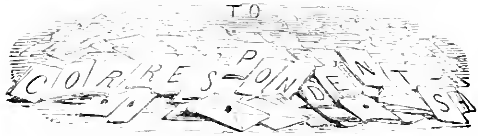
On looking over the meteorological returns from several English stations, we find that, while the thermometer has been unusually high throughout, there have been three periods of especially warm weather. The first of these occurred on the 5th, the second about the 13th or 14th, and the third about the 20th or 21st. In the subjoined table, the temperature on each of these occasions has been compared with the mean for the corresponding day of thirteen years:—

STATIONS.	5th.		13th or 14th.		20th or 21st.	
	Temp.	Difference from mean.	Temp.	Difference from mean.	Temp.	Difference from mean.
	deg.	deg.	deg.	deg.	deg.	deg.
North Shields	51·0	+ 6·2	55·0	+12·2	50·0	+ 8·5
York	51·5	+ 6·7	55·0	+12·0	50·0	+ 8·4
Yarmouth	52·5	+ 6·5	52·5	+ 8·9	50·0	+ 7·6
Cambridge	58·0	+12·8	54·5	+11·3	52·5	+10·5
Nottingham	56·0	+10·5	57·0	+13·7	52·5	+10·7
Leicester	54·6	+ 9·2	55·3	+12·0	50·1	+ 9·3
Birmingham	56·1	+10·6	56·6	+13·2	50·0	+ 7·4
Hereford	56·6	+ 9·8	57·9	+13·6	51·0	+ 8·1
Oxford	57·0	+10·6	54·5	+10·6	54·5	+12·3
London	59·5	+19·7	56·0	+11·1	54·5	+11·6
Marlborough	51·2	+ 7·5	55·5	+11·2	49·6	+ 6·8
Dover	54·5	+ 6·9	52·0	+ 6·8	53·0	+ 9·7
Hastings	54·5	+ 6·9	52·7	+ 7·5	52·5	+ 9·2
Silloth (Carlisle)	49·7	+ 4·7	54·1	+10·8	49·7	+ 7·1
Barrow-in-Furness	50·5	+ 5·2	51·5	+ 8·3	50·0	+ 7·2
Manchester	50·2	+ 5·0	55·0	+11·9	47·6	+ 5·7
Liverpool	53·0	+ 6·8	56·5	+12·3	51·5	+ 8·7
Holyhead	54·5	+ 7·5	53·0	+ 7·5	52·5	+ 8·3
Pembroke	54·0	+ 6·2	54·0	+ 7·7	53·0	+ 7·3
Plymouth	56·8	+ 7·2	51·8	+ 7·5	53·8	+ 7·7

It will be seen that on the 5th, the average temperature was from 5° to 7° above the mean in the north of England, and also on the south coast, but that in the Midland counties it was between 9° and 11° above, while at Cambridge and in London the excess was nearly 13°. On the 13th or 14th—for in some places the greatest heat was on the former date, and in others it occurred on the latter—the excess was between 7° and 8° on the west and south-west coasts, but between 11° and 13° at most of the inland stations, while at Nottingham and Hereford it amounted to more than 13½°. On the 20th and 21st the weather was not quite so warm, but even then the temperature was more than 10° above the mean in many parts of central England, and as much as 12° in excess at Oxford.

On examination, it appears that a warm November is usually associated with great storms, and frequently accompanied by disturbance of an electrical nature in the shape of thunderstorms and displays of aurora borealis. The month under review has certainly been distinguished for these phenomena, although their influence has been chiefly confined to the more western and northern parts of the kingdom.

F. J. B.



Letters to the Editor.

[The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with their writers. He requests that all communications should be as short as possible, consistently with full and clear statements of the writer's meaning.]

All communications should be addressed to the Editor of Knowledge, *74, Great Queen-street, W.C.*

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∴ All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.

All Letters or Queries to the Editor which require attention in the current issue of Knowledge, *should reach the Publishing Office not later than the Saturday preceding the day of publication.*

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

Our Correspondence Columns.

THE INVISIBILITY OF LIGHT—INCLINATION OF THE EARTH'S AXIS—THE ZOETROPE.

[49]—Without entering—at all events for the present—into the discussion of the question "Is the Sun hot?" I should like to suggest to "Tyro" (letter 6, p. 35) a simple experiment which will give him ocular demonstration that light is invisible. Let your correspondent, then, make a pasteboard tube, ten or twelve inches long, and three inches in diameter. Close the bottom with a disc of card, and line the whole with black velvet. This done, let him cut two holes, say half an inch in diameter, opposite to each other, two-thirds of the way towards the closed end of the tube; and between these a third hole. If he will hold this simple piece of apparatus so that the sun shall shine across the tube, *i.e.*, so that sunlight shall enter in at one of the side holes and pass out at the other, and will look into the tube through its open end, he will see —nothing; the interior being totally dark. Now, let him introduce a strip of writing paper through the hole between the two others (which should be at the lower side of the tube), and push this paper up until it enters the beam of sunlight. Instantly the interior of the tube will become illuminated, because the previously invisible light will be reflected from the paper, and so become perceptible. "Tyro" has presumably noticed that a beam of sunlight entering a darkened room through a hole or chink in the shutter is traceable as it crosses the apartment. This, however, arises from the reflection of the light by the particles of dust which fill the atmosphere of the apartment. Were it practicable to eliminate these by burning, or otherwise, the path of the ray would be quite invisible, and the round spot of light on the floor or opposite wall, would be the only indication we should receive of the entry of sunlight into the darkened chamber at all.

I do not know whether the "Tyro" of letter 9 (p. 36) is the same "Tyro" as he to whom I have just been essaying a reply. At any rate, I will suggest another experiment as a means of clearing up this second difficulty. It is this. Let your querist obtain a lamp, an apple, and a knitting-needle. Furnished with these, he must thrust the knitting-needle diametrically through the apple, and place the lamp in the middle of the table. The lamp will stand for the sun, the apple for the earth, and the knitting-needle for its axis. Now, he must incline the needle 23°, and we will suppose that, in doing so, he causes the top of it to point to the north wall of his room. Then he must carefully preserve the direction of the needle constant in this position, and, doing so, carry it round the lamp. An instant's reflection will show him that, should he start from the north side of the lamp, the upper or northern end of his needle (and obviously the northern half of his apple-earth) will be inclined from the lamp; and that when he brings it round to the south side of the lamp, the top of the axis, still pointing to the north wall of the room, must be inclined towards it. The end of "Tyro's" query is not quite so intelligible as might be desired; but he seems to conceive in some occult way that the earth's rotation must affect the position of her axis. If, while studiously keeping his knitting-needle axis parallel to itself during its revolutions round the lamp, he twists it so as to make the apple rotate on it, he will at once see how the two movements may be independent of each other.

As "Moonstruck" (query 1, p. 38) has a question on a cognate subject, I may say that perpetual spring, rather than perpetual summer, would reign at the poles of the earth, were its axis perpendicular to the plane of the ecliptic. Summer would, of course, be continuous at the Equator, where the sun would be always vertically overhead.

If "Zulu" (query 10, p. 38) will reflect, *imprimis*, how much what we call seeing is a matter of inference, and, in the next place, remember that the image of any object is retained by the retina for something like 0·1 second after such object has disappeared, he will get some idea of the way in which the images in the Zoetrope are caused, as it were, to shade into each other, the mind unconsciously supplying the intermediate steps. The real images, in the case of an actually moving object, must, so to speak, overlap in a way which must render it impossible to predicate definitely that any one given instantaneous attitude of the body in motion has been actually observed.

A Fellow of the Royal Astronomical Society.

THE MISSING LINK.

[50]—Dr. Andrew Wilson does good service in making known to your readers the erroneousness of the widespread notion that man is descended from monkey. Concerning the question to which he makes excellent reply, I think the following remarks in Professor Huxley's Preface to Hæckel's "Freedom of Science" (p. xiii.) will be serviceable.

"All the real knowledge which we possess of the fossil remains of man goes no further back than the quaternary epoch, and none of these remains present us with more marked pithecoid* characters than such as are to be found among the existing races of mankind. But then the equine quadrupeds of the quaternary period do not differ from existing *Equidæ* in any more important respect than these last differ. Yet it is a well-established fact that in the course of the tertiary period, the equine quadrupeds have undergone a series of changes exactly such as the doctrine of evolution requires. Hence sound analogical reasoning justifies the expectation that when we obtain the remains of pliocene, miocene, and eocene *anthropidæ*, they will present us with the like series of gradations." —Edward Clodd.

THE 1-INCH ORDNANCE MAPS.

[51]—In the first numbers of Knowledge, attention has been called to the fact that the 1-inch to the mile Ordnance maps are not trustworthy. A very good case in point has come to my knowledge. From Handcross, in Sussex, three roads run to Brighton, one through Crabtree and Henfield, one through Cuckfield, and the other through Hicksted and Bolney. The one through Crabtree leaves the other roads about a mile south of Handcross Gate, and is shown in the right place on the map. The one through Cuckfield leaves the one through Bolney about a furlong lower down; but on the map it is a good mile and a quarter, and, consequently, for about three miles run is shown entirely in the wrong place.—I am, &c.,

G. W. Buckwell.

PALIZSCH AND HALLEY'S COMET.

[52]—Allow me to point out that, in an article on Comets in the first number of Knowledge (p. 10), you have inadvertently adopted an oft-repeated error, that when Palizsch re-discovered Halley's comet on Christmas Day, 1758, he found it "without telescopic aid." A complete account of his discovery, in his own words, will be found in the "Berliner Jahrbuch" for 1828, by which it appears that it was made with a telescope of 8 ft. focal length, and was the result of a search for the comet in the part of the sky where he expected it to appear. With this he noticed, about six o'clock on the evening in question, a nebulous-looking object, between ε and δ Piscium, which he had not seen there before; and subsequent observations, on Dec. 26 and 27, proved that it was indeed a comet. Thus did Palizsch (who, though certainly a farmer, was not a peasant, for he was a man of education, and an amateur in botany and other sciences, besides astronomy) first observe the predicted return of a comet nearly a month before anyone else; Messier being the next, and observing it at Paris, with a 4½ ft. Newtonian, on Jan. 21, 1759. I presume the mistake that Palizsch found it casually, and without telescopic aid, was founded on a misunderstanding of a note in Herschel's "Outlines," which certainly conveys that impression, but does not actually state it, so that it is difficult to be sure whether Sir John really thought so. But at any rate, it has been repeated in many books on astronomy, and Madler thought it

* Ape-like.

necessary to contradict it in his "Geschichte der Himmelskunde" (vol. II., p. 460). Palitsch was less fortunate in 1780, when he thought he had discovered another comet in Cancer, which proved, however, to be a now well-known nebula. He died at Lungraa, near Dresden, on Feb. 22, 1788, in the 65th year of his age; his memorable discovery on Christmas Day, 1758, was made at Prohlis, between Dresden and Pirna. Yours, &c.,

Blackheath, Nov. 11. W. T. Lynn.

DO COMETS OBEY GRAVITY?

[53]—As I am probably the victim of ill-written text books, such as you allude to in your introduction to the correspondence columns, perhaps you will kindly permit me to be set right in regard to a little matter which has occasionally troubled me, and which is again suggested by the article on comets.

It is stated in that article that comets obey the law of gravitation; and also that the matter of which they (or at least their tails) are composed is of inconceivable tenuity.

What puzzles me, then, is how these flimsy concerns can possibly be obeying the laws of gravitation, which I understand to mean that bodies attract each other in proportion to their mass. How is the sun's attraction, which fixes the *massive planets* to their present paths, to be reconciled with the fact that these *airy bodies* wander round orbits so gigantic?

If you cannot afford space for my difficulty, a reference to what you consider a reliable work will greatly oblige, yours, &c.,

Comet.

THE CRIMSON-CIRCLED STAR.

[54]—Will you kindly inform me to what star Tennyson refers in "In Memoriam," LXXXVIII., verse 12—

"And last, returning from afar,
Before the crimson-circled star
Had fallen into her father's grave."

The use of the feminine possessive pronoun would naturally suggest Venus. But why "crimson-circled"? Would this be effect of refraction on the horizon? If so, though only a dilettante astronomer, I do not remember having observed it.

I wish every success to your new Magazine, whose motives and virtues I diligently proclaim everywhere.

Quite apart from punning, I think you have taken as your motto, "Sublimi feriam *sidera* vertice."

Yours faithfully, M.R.C.S.

[I have always supposed crimson-circled here to mean surrounded by the crimson sunset glory. Is it not LXXXIX.?—Ed.]

TABLES OF MERIDIANAL PARTS.—THE FLAT EARTH.

[55]—Can you tell me how it is that the Table of Meridianal Parts in Riddle's Book of Tables differs so much in some parts from the corresponding table in Chambers' and Norie's books? The fact is the more strange, because the formula which Riddle gives for calculating that same table yields results identical with those tabulated in the two latter works. Which is likely to be the more trustworthy, Chambers' book or that of Riddle?

Have not you, and men of science in general, a right to ask your impassioned friend and determined enemy "Parallax" to prove that he has some clearly-defined, or, at all events, some intelligible theory of the motions of the heavenly bodies, and some conception of what that theory involves, by (1) constructing a set of tables which would enable us to find our way about on the earth; or (2) by forecasting for us such events as eclipses, planetary transits, &c.; or (3) by showing how it comes to pass that if the upholders of the Newtonian theory be the fools, or impostors, or empirics which he asserts them to be, their forecasts are so invariably justified by results?

And if he refused, or were unable to give an account of himself in some such way, to forbid him, on pain of a writ "*De lunatico inquirendo*," to open his mouth, or write, or ever send telegrams on this subject as long as he lived, to any living man, saving only Mr. Newton Crosland. Your obedient servant (and admirer and well-wisher),

Winter.

P.S. It would be interesting to discover what would be the result of the meeting and intermingling of chaos with chaos (in the way hinted at above)—whether it would be order or annihilation.

[It was a favourite idea of the late Professor De Morgan's to set paradoxer against paradoxer. It is singular that they only agree in attacking the theories which men of science agree in accepting. They never agree among themselves. Each may have a follower or two, or even ten or twenty; but they do not follow each other.—Ed.]

PYRAMID MEASURES.

[56]—With reference to your remark about the coincidences, I may remark that my mathematical knowledge does not enable me to prove that it is possible to produce the same coincidences by the use of any other values of the diameters and distances of the three bodies, and of the scale of reduction, than those I have given; if, however, it can be proved to be possible, then I admit my conclusions will not be entitled to the consideration which at present I claim for them. And further, if the next transit of Venus be observed, as it ought to be, and the resulting value of the sun's distance proves to be identical, or very nearly so, with the value I have derived from the pyramid measures, will it be quite satisfactory to a mind of average intelligence to say that the agreement is a mere coincidence? Of course, you may reply that this argument has no present value; nor am I inclined to attach much weight to it, because, looking at the results of past transits, it is scarcely to be expected that the next will finally settle the question of the sun's distance, and yield a result in which all astronomers will concur, and which will be more reliable than the one derived from the pyramid measures. Probably the results of Professor Winnecke's new method of determining the sun's distance from Venus observations will be more accurate than any of the results obtained by other methods.

I think a much greater interest is now being taken in the pyramid than you seem to be aware of. An active correspondence has been going on this week in one of the Manchester papers, in which, however, I have taken no part; and a lecture was delivered there on Wednesday evening. Lectures are also being given in other towns and districts, and the pyramid is apparently fast becoming a household word.

I notice printer's errors in four of my equations which render them unintelligible. I enclose a list of corrections of four of the formulæ in my paper.—Yours, &c., Joseph Baxendell.

Corrections of formulæ in paper on "The Great Pyramid Measures, and the Diameters and Distances of the Sun, Earth, and Moon."

9.	for	$\frac{se^3}{9ms}$	read	$\frac{se^3}{9mS}$
20.	,,	$\frac{\sigma^2\sqrt[3]{\pi^3}}{e^3 2\sqrt{10^5}}$	,,	$\left(\frac{\sigma^2\sqrt[3]{\pi^3}}{e^3 2\sqrt{10^5}}\right)^2$
31.	,,	$\frac{8\pi}{1\pi}$	,,	$\frac{8\pi}{1\eta}$
33.	,,	$10\sqrt{\frac{Se}{s}}$	,,	$10.\sqrt{\frac{se}{m}}$

INTELLIGENCE IN ANIMALS.

[57]—Some years ago my father, who was a medical practitioner in Somersetshire, had a valuable horse, which eventually he was obliged to part with, as it was vicious, and not always safe to drive. During the time my father drove it, he had occasion to visit daily for several weeks an old gentleman who had met with a serious accident. His patient lived at the bottom of a steep lane, which branched off at right angles from the main road, at about 3½ miles from the town where my father lived. This horse was always used for visiting this patient, and during the first two or three weeks, when there were dangerous symptoms, was frequently driven down the lane twice a day.

The farmer to whom my father sold this horse lived at a distance of several miles beyond this turning on the same road, and attended regularly the market in the town where my father lived, and necessarily passed this sharp turning both going and returning therefrom. Some three or four years after purchasing this horse, he had occasion to drive into the town to fetch my father to attend his wife. As the case was urgent, he got into the gig, and was driven by the farmer towards the farm where he lived. Suddenly, without the slightest warning, the horse turned down the lane he knew so well, nearly capsizing them.

As soon as they had recovered themselves, the farmer exclaimed that "he had never known the horse do such a thing before all the years he had had it." My father was surprised, and said, "Not when you have driven this way to and from the market?" The farmer replied, "that the horse never even so much as looked at the turning, whilst he had driven it, until now." "Well," said my father, "he must associate me, knowing that I am in this gig, with the many visits he used to pay with me down that lane, when I attended my poor old patient at the bottom, after his accident. I patted his nose before starting, and he knows by my voice that I am behind him. His memory has served him well, and he concluded that I must be going the same journey we performed together so many years ago." My father always considered this fact evidence of reasoning powers in the

horse, and although I incline to the same opinion, I will not comment upon it, but content myself with simply relating this anecdote. A. H.

Nov. 19, 1881.

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[58]—A singular instance of *apparent* prescience in a dog occurs in an account given Nov. 21 of a father shot by his son. Here is the evidence of the wife and mother :—

"We heard nothing to disturb us after retiring to bed until about half-past two o'clock next morning. About that time a little dog which belonged to my husband, and was a great favourite, came upstairs, and jumped upon our bed. My husband tried to make the dog go away, but he could not do so, as the little thing seemed so 'fussy.' At last he thought the best thing to do would be to take the dog downstairs, and by shutting the door at the bottom, prevent it from returning. My husband got out of bed, and took the dog in his arms for the purpose of carrying it away. In about half-a-minute, and when he was on the stairs, I heard a loud report, as if a pistol or a revolver was being fired. This was repeated twice, and the deceased then shouted out at the top of his voice, 'I am shot!' "

The peculiarity here is that the coming danger, of which the animal appeared cognisant, could only have been imparted by the footsteps or other movements of *a member of the family;* this, under ordinary circumstances, could have given no such premonitions of danger to the dog. Has any similar case been observed?

Bark.

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[59]—In the article on "Intelligence in Animals," in No. III. of Knowledge, p. 46, the writer speaks of animals possessing the power of practical reasoning, but not abstract. Does not the following anecdote show a power of more than merely practical reasoning? I do not know whether it has been quoted by anyone else in this relation to Darwin's theory.

Two students were in the habit of visiting each other at their respective rooms; each had a dog. On one occasion both dogs were left outside the room while their owners were inside. The dogs began to fight, which ended in their being admitted into the room and kept at a distance from each other. On a subsequent visit one dog was absent, and the other dog was put outside as before. The two students were soon after surprised by hearing what they thought was their two dogs outside fighting. On opening the door the dog walked into the room, which was the end he desired. He had remembered the reason why, on a previous occasion, both he and the other dog had been admitted into the room, and he had acted accordingly.—Yours, &c. Frederick G. Abbiss.

REASON IN ANIMALS.

[60]—In support of the view that animals possess a certain amount of reasoning power, I would contribute the following instance, which has not hitherto been published.

During my boyhood my father had the shooting over some property adjoining a deer-park, and we owned at this time a very intelligent setter, which used also to retrieve. One day my father shot and wounded a hare, which made its way through a hole in the park-paling. The dog leaped the paling, caught the hare, and brought it back to the fence in its mouth. It then tried several times to return by leaping the paling, but the weight of the hare prevented it from reaching the top. After resting awhile it bethought itself of the hole through which the hare had come, and, taking the hare to this hole, it pushed it through, then leaped the park paling, and brought the hare to my father.—I am, sir, your obedient servant, R. Clement Lucas, B.S., F.R.C.S.

Dec. 18, 1881.

ARE WOMEN INFERIOR TO MEN?

[61]—I read with much pleasure your remarks on the question, "Are women inferior to men?" It occurs to me that if we are inferior in brain capacity, the reason may be that sufficient care is not taken to develope the brain. We all know the size of the hand is increased by constant work—at least of a particular kind. And constant use of the other parts of the body usually leads to an altered and probably heavier formation. Now, in a girl's education the brain is but slightly exercised. Music and needlework, which occupy so much of her time, scarce exercise it at all. And even the part of her education which does require brain-work seldom calls it forth vigorously. She has fewer rewards to look forward to if she succeeds, and fewer punishments if she idles. Many idle boys require whipping—and get it. Many idle girls require whipping—and do not get it. Then, just when the boy is about entering on the most active part of his brain-work in a University, the girl is taken from school, and sets out on a new course of *flirting* and *husband-catching*. She is sometimes at this work, and perhaps even married at an age when the boy who idles would be whipped—as she ought to be. Again, in her after-life, she has but rarely much brain-work to do, and the development of the brain is probably not maintained. But I should be surprised to hear that the brain development of George Eliot, or George Sand, for instance, was inferior to that of the average male, and if it was, it would prove that the quality, not the quantity, of brain was the really important point. If girls were as well taught at schools as boys, got as much brain-work to do, and were then sent to universities, and did not begin to look for husbands until they were twenty-two or twenty-three years old, I have no doubt their brains would be much improved. I know my views on whipping will be unpopular, but if the sexes are equal, as I contend, why not treat them alike?—I remain, &c., Susan G.

EFFEMINACY OF APPEARANCE.

[62]—Can any of your readers give me a description of the characteristic difference between the physiognomy of the male and female of the human species? You may sometimes see men whose features might be called effeminate; now in what does this peculiarity consist?

The article on the question "Are women inferior to men?" brought this to my mind. Physiognomist.

OPTICAL ILLUSION.

[63]—In regard to the optical illusions, there may be added to the instances adduced in Mr. Foster's article, one of circles.

If, say, a 2-inch hole be cut in a piece of brown paper and an inch periphery left round it, and, again, another 2-inch hole be cut with a three inch periphery, the two holes, although exactly the same size, will appear not to be so.—I am, &c.,

Percival A. Fothegill, F.R.A.S.

A "LUNAR ILLUSION."

[64]—I have frequently noticed what your correspondent M. calls a "Lunar Illusion" (No. 23, p. 57), but I have always supposed a different reason for it, and I hardly think that M.'s arrow hits the mark.

Let us suppose a lighthouse on our horizon; we know that a straight line from it drawn through the moon would bisect it very differently from a line drawn from the setting sun (below, in fact), though the sun would appear in the same place as the lighthouse, and a line from it would, of course, seem as if it ought to bisect it in the same way. The *enormous comparative distance* of the sun seems to me the true reason; and I have sometimes thought that a rough idea of his distance might be made by taking the angle of M.'s sagitta when the moon is near the zenith, knowing, of course, the distance of our satellite from the earth.—I am, sir, yours, &c.,

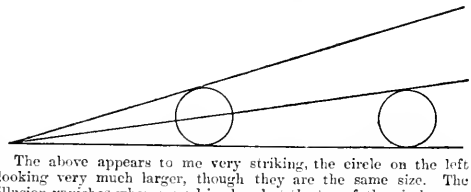
Cleveland Lodge, Sydenham. S. H. W.

TELESCOPES.—OPTICAL ILLUSION.

[65]—Would it be in accordance with your plan to give a popular description of the different kinds of telescopes, with the apparatus, giving a few hints to amateurs in obtaining them, explaining among other things the following :—

The Terrestrial Telescope.
„ Astronomical „
„ Newtonian „
„ Equatorial „
„ Huyghenian Eye-Piece.
„ Pancreatic „ „
„ Kitchener's Pancreatic Eye-Piece, &c., &c.
„ Different ways of mounting.
„ „ kind „ stands.
„ Power that object glasses will bear, &c.

As the circulation of Knowledge increases, as I am sure it will do, you may be able to spare a portion of space in each number to the above subject.



The above appears to me very striking, the circle on the left looking very much larger, though they are the same size. The illusion vanishes when a card is placed at the top of the circles.

F.H.S.

SOLAR HEAT.

[66]—Referring to Letter 2 (page 15), I am of opinion that whatever conditions apply to this earth apply also to the sun, only in a greater degree. The matter of which the earth (and all the planets and satellites) is composed, once formed part of the sun, which has gradually condensed and left each planet behind in its turn. Geologists tell us that at a period inconceivably remote our globe was in a state of intense heat. This is practical proof that the sun is in a similar state. Referring to Letter 28 (page 58), Mr. Newton Crosland endeavours, by one fell blow, to annihilate the great law of universal gravitation. In promulgating this great law, Newton cleared away heaps of the cumbrous rubbish with which the old schools of astronomy were hampered. Emerson says: "The tendency of all science is to simplify." Mr. Crosland endeavours to complicate. Yours, &c., John Thomson.

IS THE SUN HOT?

[67]—It may interest your readers to know that this subject was discussed in *Design and Work*, Vol. VII., under the question, "Does the Sun Warm the Earth?" The leader of that discussion endeavoured to prove that we receive our heat from the earth, and not from the sun! Under the title of "The Great Primordial Force," it has also received the attention of Henry Raymond Rogers, M.D., of Dunkirk, N.Y., in a paper published in the September number of *Progress of Science*. In this paper, Dr. Rogers propounds a similar opinion to that expressed by your correspondent, "Cogito," in letter 24. G. E. Bonney.

THE SUN'S HEAT AND THE EARTH'S RADIATION.

[68]—I have read the replies to Anti-Guebre's question (which, like yourself, I have frequently encountered before); but in all of them, including your own, one important factor is omitted—viz., the difference between the obstructive or absorbent power of atmospheric vapour to the rays of heat from different sources. The sun-rays, as several correspondents have stated, pass through our ordinary atmosphere with but little absorption; but this is not the case with "obscure rays," or rays from a less heated and non-luminous source. Hence the earth warmed by the sun does more to warm the air than the sun itself does, because the heat it radiates is absorbed by the air, and also because it warms the air by direct contact and the commingling of the portion of air thus heated with that above, or "convection." It appears to me that even our best treatises give too much credit to this latter action, and too little to the first. The following experiment which I tried many years ago on Mont Blanc, at an elevation of between fourteen and fifteen thousand feet, is instructive. My coat was wetted by falls on the sloppy surface of the glacier below, and at the elevation above-named, I noticed that the coat-tail opposite the sun was thawed, while that in the shade was frozen and stiff. By slowly turning round like a joint of meat before a roasting fire, I alternately thawed and refroze all sides of my coat-tails in about one minute, as nearly as I can remember. The direct rays of the sun were painfully scorching, and skinned my face and ears completely, but in the shade, the thin and highly-dried air permitted a far greater degree of radiation of obscure heat to take place than down below. Hence the freezing. I do not write this to controvert the other explanations given by yourself and correspondents, but merely to supply an additional factor.

Thus, the cooling effect of night radiation is far more effective above than below, seeing that the amount of resistance to the passage of the obscure rays is so greatly diminished there. This is strikingly shown by the sudden freezing of the rills and streamlets, which are such remarkable features of a glacier surface during the summer's day. They all stop as the sun sets, and the sloppy surface of ice is dried, as was the shady side of my wet coat.

W. Mattieu Williams.

LIGHTNING IN NOVEMBER.

[69]—Last night (Nov. 4) at 10.30 p.m., in a perfectly cloudless sky, the flashes of sheet lightning were vivid and incessant. Is not this very unusual at this time of the year? A strong gale was blowing in the Irish Channel at the time.—Yours, &c.,

Liverpool, *Nov.* 23, 1881. E. S.

LOGARITHMS.

[70]—Mr. Grundy asks for information on an important item in connection with Logarithmic work, viz., the tables. Many students give up in disgust this simple process of calculation because they have been unfortunate in their choice of books.

I have been in the habit of using Logarithm Books for many years, for work of every description, and with pleasure give him my experience.

In a long series of logarithmic calculations, the book to use will depend on the number of figures we require in the desired result. For instance, in doing 4-figure work, it would be a loss of labour and time to use a 7-figure table.

The following are the tables I have used with great comfort:—

For 4-figure Work:—

Table of Logs and Anti-Logs, published by Layton, price 1s. (Very useful.) This requires simple interpolation.

Table of Logs and Anti-Logs (Hannyngton), published by Layton, price 5s. A comfortable table, but rather dear.

For 5-figure Work:—

Table of Logs, published by Smith, Elder, & Co., price 1s. 6d. A good book, and well worth the money.

Table of Logs by Gauss. Better than the above, but requires a little more practice. It contains many other useful tables. Can be had of Trübner, price about 1s. 6d.

For 7-figure Work:—

Table of Logs by Bruhns. The best book published; I strongly recommend it. Can be had of Trübner, price about 4s. or less.

Table of Anti-Logs by Filipowski. This is a useful book when many Anti-Logs are required. Published by Bell & Daldy.

Abacus.

THE PRIMARY COLOURS.

[71]—In the little Treatise on Optics, forming part of the course of "Natural Philosophy," published in 1862, by the Commissioners of National Education in Ireland, a statement is put forward with respect to the constitution of the solar spectrum, which differs from that set forth in other similar works upon chromatics.

The correctness of that statement is borne out by what I myself conceive to be the appearance of the character of the colour of violet light, yet I confess I am puzzled to make out how, upon the explanation offered, the colour of the light in question is to be accounted for.

The commissioners, after describing in detail Newton's experiment of breaking up solar light into its seven coloured constituents, continue their statement as follows:—

"In reality, blue, red, and yellow are the only colours present, the rest being combinations of them. For the spectrum consists of a layer of each of these colours, superimposed on the other—the blue, the red, and the yellow appearing distinctly at those points at which they are most vivid in the superimposed and corresponding layers."

"The three colours of which the spectrum is really composed are thus divided among the seven which it contains, calling red rays R, yellow Y, and blue B.

White	Red.	Orange.	Green.	Blue.	Indigo.	Violet.
20R + 30Y + 50B	8R	7R + 7Y	40Y + 10B	7Y + 12B	12B	16B + 5R."

Thus far the commissioners.

As I have said, the impression produced on my eye bears out distinctly the statement as to the occurrence of *red* light in the violet exhibited by a really pure prism of dense flint glass. Yet if the spectrum consists simply of the layers of the three colours in question superimposed, it is difficult to conceive what appearance each of those spectra would present, could we manage to obtain it isolated. In what form, for instance, would the red spectrum present itself? From the above table we learn that no red exits in the yellow, green, blue, or indigo, while a considerable amount of red is met with again in the violet. Would the red spectrum exhibit a wide, blank interval, extending from the yellowish end of the orange up to the violet end of the blue?

An explanation of this matter, "plainly worded, exactly described," will doubtless be an easy task for you, and will probably prove a boon to others besides,

Yours, &c.,

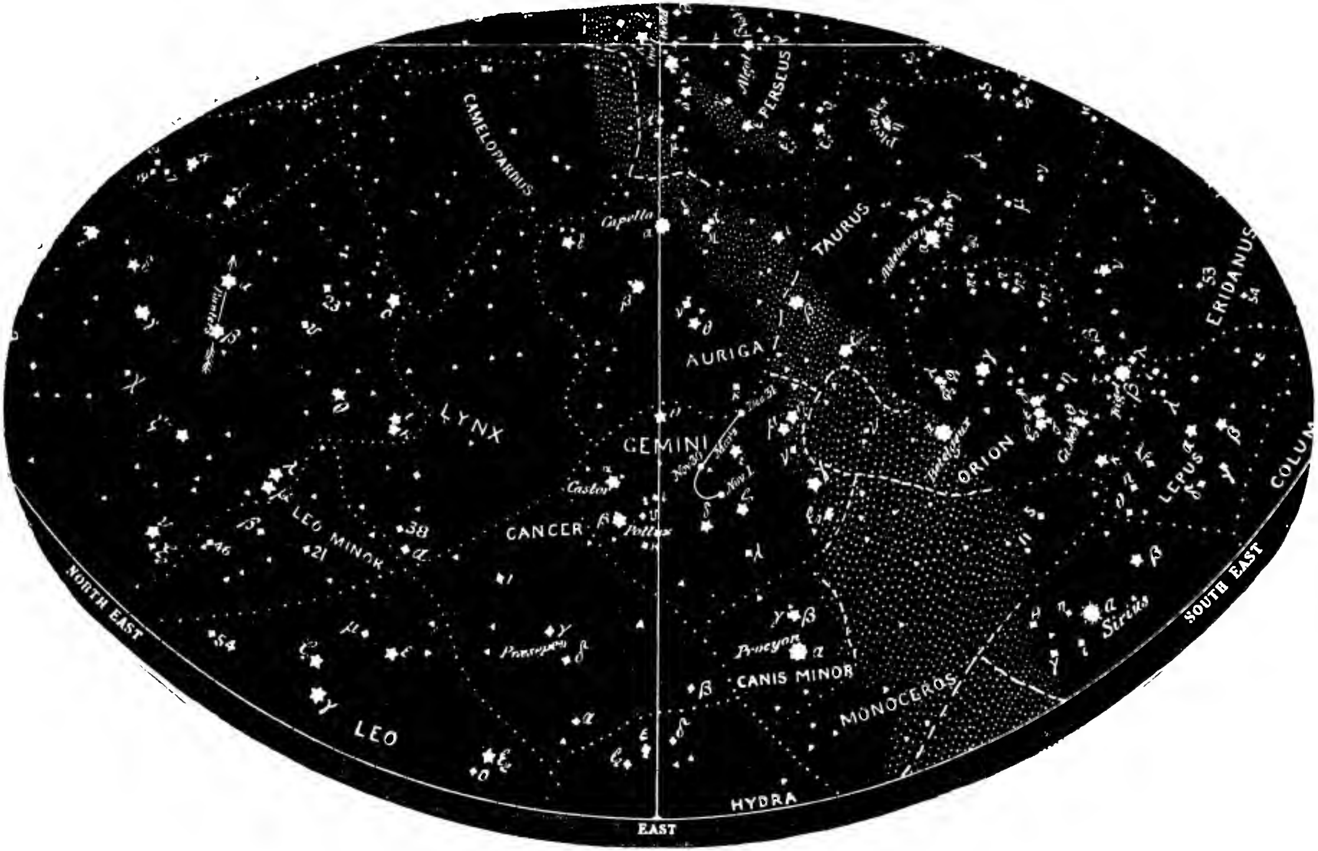
Mabel Winfred Lain.

PHRENOLOGY.

[72]—Will you allow me to make a few observations, in answer to the first query contained in letter 32, p. 59, entitled "Phrenology"? "G. P." says: "Assuming that phrenology is all wrong, what are the causes that determine the shape of the head?" I would observe at the outset that if we assume phrenology to be "all wrong," there can be no satisfactory explanation given of the diversities in the shapes of heads. A common answer to the query "Why are no two heads alike?" is, "Why are no two faces or hands alike?" The simplest answer to this last seems to be, that no two human natures are alike, and this will apply to the first also, when we consider that the varieties we discover in the

[Continued on page 99.

THE EASTERN SKIES IN DECEMBER.



This Map shows the position of the stars in the Eastern Skies. —

On November 30, at $10\frac{1}{2}$ o'clock.	On December 8, at 10 o'clock.	On December 16, at $9\frac{1}{2}$ o'clock.	On December 23, at 9 o'clock.	On December 30, at $8\frac{1}{2}$ o'clock.
On December 4, at $10\frac{1}{4}$ o'clock.	On December 12, at $9\frac{3}{4}$ o'clock.	On December 19, at $9\frac{1}{4}$ o'clock.	On December 27, at $8\frac{3}{4}$ o'clock.	On January 3, at $8\frac{1}{4}$ o'clock.

The movements of the planet Mars, which is travelling in this part of the stellar heavens at present, are indicated by the curved line marked with this planet's name. We show its path during the last month as well as December. He is now retrograding at his greatest rate. He passes his opposition, or is exactly opposite the sun, so as to come due south at midnight, on December 27. The stars in the Eastern skies are passing upwards from the horizon, moving aslant, from left to right.

⁂ Stars of the first magnitude are shown with eight points, those of the second with six, of the third with five, of the fourth with four, of the fifth with three.

Continued from page 96.]
shape of the head are the same varieties that exhibit themselves in the whole external form of man. Now, if these differences of bodily structure are marks of diversity of nature, the inequalities of the skull (which cannot be exempt from the process) will also indicate the same, or to come to the point, will indicate diversity of brain conformation, which is just the doctrine of phrenology. Assuming it to be established, then, that the "cause that determines the shape of the head" is peculiar conformation of brain, the pertinent remark of Professor Lawrence forms a fitting supplement :—"If the mental processes be not the function of the brain, what is its office? In animals, which possess only a small part of the human cerebral structure, sensation exists, and in many cases more acute than in man. What employment shall we find for all that man possesses over and above this portion—for the large and prodigiously developed human hemispheres? Are we to believe that these serve only to round off the figure of the organ, or to fill the cranium?"

With regard to the opinion of Mr. G. H. Lewes, that the brain acts as a whole, and that its functions are not localised, one merely requires to point to Dr. Ferrier's researches in cerebral physiology, by which it has been proved that the *individual convolutions are separate and distinct organs.* A. B.

POPULAR ELECTRICITY.

[73]—I write to suggest that now that electricity is becoming of such vast importance in the world, it is very desirable that a sound elementary knowledge of its principles should be widely disseminated, and that KNOWLEDGE might be an available medium for contributing to this result. Would it not be a good thing that a series of papers on this subject (including magnetism) should be published in KNOWLEDGE? The papers should be simple, and should suggest experiments which any person of intelligence might try for himself without elaborate or expensive apparatus. Something in the style of Tyndall's "Lessons on Electricity," but extending, of course, to current electricity. I venture to think that such a series, if produced by a competent hand, would be extremely popular with young people, whose desire for information in this direction is very great.—Yours, &c., DANIEL JONES.

COMETS' TAILS.

[74]—I have read with great interest your excellent paper on Comets, but am sorry to find no other theory can be introduced to account for the tails always pointing away from the sun, except that of solar repulsion. Such a theory, I am sure, should not be adopted without the most conclusive and perfect evidence of its existence; because, if we introduce a resisting medium into the solar and stellar system, we see in it the germs of its own destruction, which, although for a time making no effect upon the movements of the ponderous planets, must, in the end, make itself felt, even though that medium be of the rarest tenuity. I once heard a theory propounded, that comets were simply lenses, and the sun shining through them produced the appearance of a tail; that might apply to telescopic comets, without any indication of a nucleus, but not otherwise, for Sir John Herschell tells us the nucleus of a comet is opaque. The theory is, as far as I know, a novel one, and the readers of KNOWLEDGE must judge for themselves of its value.—Yours, &c., J. D.

THE SUN'S DISTANCE.

[75].—The latest determination of the velocity of light, by Michelson, gives 186,305 miles per sec. *in vacuo.* Supposing that the time interval for the passage of light across the major axis of the earth's orbit, which is given as 986·6 secs., may be relied on to the $\frac{1}{10}$ part of a second, as is likely, considering the numerous and accurate observations on Jupiter's satellites; may not the deduced semi-axis of the earth's orbit, which is 91,904,256 miles, be trusted as far as the first four figures? The corresponding parallax would be 8″·895, round which all the best determinations seem to centre, such as 8″·88 from transit of 1871, 8″·91 by Hansen's lunar method, 8″·894, and 8″·855 from observations on Mars. It appears that Michelson's method, which is based on an extension of Foucault's, gives a result which may be relied on almost down to the unit's place: for the interval to be measured was the passage of light over a distance of about 1,000 yards, and the direct deviation obtained was about 150 times that obtained by Foucault.—Yours, &c.

Marlborough College, Nov. 19, 1881. H. L. CALLENDAR.

[Considering the somewhat wide discrepancies between the observed time of appearance and disappearance of Jupiter's satellites, according to the telescope employed and the observer's eyesight, we should be disposed to doubt whether the method referred to can be trusted as likely to give results to the degree of accuracy you mention. Aberration is regarded as more trustworthy. ED.]

THE METRICAL SYSTEM.

76.—Sir W. Thompson, in his address at York, referring to the metrical system, says, "to it we are irresistibly drawn in all scientific and practical measurements, notwithstanding a dense barrier of insular prejudice most detrimental to the islander."

It would be superfluous to enlarge here upon the many advantages of the metrical system of weights and measures which are set forth and explained in so many elementary treatises; but if you could find space in your columns for two or three examples, illustrating the saving of labour that their adoption would introduce into the computations of every-day life, I cannot but think that you would make converts, and by facilitating calculation, promote accuracy, the stepping-stone to all knowledge.

Take an ordinary example from agriculture :—

A field of 6 hectares, 41 ares, 66 square mètres, sown with wheat, at 250 litres to the hectare, would require

$6·4166 \times 250 = 1,543·8500$ litres,
$= 15$ hectolitres, 44 litres of seed.

If the wheat is of specific gravity 0·80, the whole quantity would weigh $1544 \times ·80 = 1235·2$ kilogrammes.

Again, the seed required would be
2·5 litres for every are,
or 0·25 litres for every square mètre.

Let the same field be ploughed up to the depth of 2 decimètres (nearly eight inches), and suppose that the specific gravity of the soil is 2, every square mètre of the soil ploughed up will weigh $2 \times 2 \times 100 = 400$ kilogrammes, and the whole soil of the field ploughed to the depth of 2 decimètres will weigh

$64,166 \times 400 = 25666400$ kilogrammes,
$= 25666·4$ metrical tons.

It then becomes very easy to compute the proportion of any manure to the available soil.

Let the amount of manure applied be 1,000 kilogrammes per hectare :—

1000 kilogrammes per hectare
= 10 kilogrammes per are
= 100 grammes per square mètre
= 1 gram per square decimètre, or 2 cubic decimètres of soil.

$= \frac{1}{4000} = ·00025.$

If the amount be 600 kilogrammes per hectare
= 6 kilogrammes per are
= 60 grammes per square mètre
= ·6 gram per square decimètre

$= \frac{·6}{4000} = ·00015.$

When great accuracy is required, the use of logarithms will, of course, much facilitate the calculation, but they may be dispensed with for ordinary practical purposes. Now, let anyone try to work out such simple problems with a field of the same size expressed in English measures, viz., 15 acres, 3 roods, 17 poles, and the seed sown at 3 bushels per acre. He will see what an amount of labour they involve. Indeed, at the outstart, we are met with such an anomaly as this, that while the yard is our standard measure of length, few persons could give the length of the side of a square statute acre, such side being 69 yards and a long fraction (69·5701). Nor is the matter much facilitated when we find that $30\frac{1}{4}$ square yards go to one pole.

Should you think these remarks not too technical, I shall recur to the subject with some other examples in a future number, and beg to subscribe myself, yours, &c.,

MICROCRITH.

[We thank "Microcrith" for his excellent illustration of the value of the metrical system. It is actually easier in dealing with the second form of the above problem to convert the English measures into the metrical system, then to work the problems as "Microcrith" has done, and to turn the answers into English measures, than it is to work them throughout with the English measures.—ED.]

CAUSE OF GRAVITY.

[77]—In your note to letter No. 31 (page 59) on this subject, it is stated as a "demonstrated fact that the action of gravity is communicated far more rapidly than light travels." I beg to ask an explanation of this, as I was not aware that the action of gravity was "communicated" at all in the sense of "travelling" (or of crossing over a space during a greater or less lapse of time). As far as my present knowledge goes, the statement reminds me (I give

to illustrate my point, I may cite the remark of a friend who said he knew something which "travelled" quicker than light, and on being asked to explain, answered *sight*. I had some difficulty in explaining to him that sight was merely a mental interpretation of sensation produced on a *passive* organ by the light which had already "travelled" from the distant object to the organ in question.

Unless I have been misinformed, the present tendency of scientific thought is to do away with the idea of "attraction" altogether, as a figment of the intellect. What we call gravity, or any other form of so-called attraction, is merely the tendency to fall in the direction of least push. Thus, suppose a railway truck between two engines tending to push it in opposite directions. It cannot move both ways at once. It therefore moves in the direction of the weaker engine. Something analogous to this is the modern explanation of attraction so-called. A stone falls to the earth because the pressures exerted on it are least in the direction of the earth—no attraction at all in nature—all push and strife.—Yours, MUDDLERSBROUGH.

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[78]—I was much gratified to find this question raised in the last number of KNOWLEDGE. It is an exceedingly interesting subject, and one I would like to see treated in a competent manner. In an article by yourself, entitled "The Mystery of Gravity," you intimated that you might at some future time describe a method by which gravity might be generated and propagated, founded on the views of Le Sage. I write from memory, and therefore cannot be certain of the exact words. Will you kindly say whether this promise has ever been fulfilled? If not, would you kindly favour the readers of KNOWLEDGE and myself by stating your views on this truly great question.—Yours, &c., T. J. HICKIN.

ELECTRICAL BELLS.

[79]—As a subscriber to your promising weekly, I have come across the extract from the *Times*, No. 1, page 14, wherein the writer recommends electric bells for private houses.

I beg to say from long experience, electric bells are very troublesome in private houses, however they may answer in hotels, where there is generally a *yearly charge* to keep them in order.

There is a better system in use in Government offices, and that system is the pneumatic, which is so simple and durable, that I wonder the *Times* should ignore "pneumatic" while writing up "electric."

I have pneumatic in my house, and the excellency of the whole arrangement is, beyond question, worth bringing to the front, certainly quite as much so as electric is by the *Times*. There is no battery nor anything whatever to attend to.—Yours truly,

M. TESTER.

THE FIFTEEN PUZZLE.

[80]—I enclose a *short* proof, which I gave in the *Brighton Herald* for May 22, 1880.

Take 15 tickets, numbered from 1 to 15, and arrange them in a row in any order. Let every instance in which a lower number is further on in the series than a higher number be called a disarrangement. Note whether the number of such disarrangements is even or odd. A cyclic interchange of any odd number of the tickets will make an even difference in the number of disarrangements, and, therefore, no combination of such interchanges can convert an order with an odd number of disarrangements into an order with an even number of disarrangements.

Now arrange the 15 tickets in a square, and bring the blank space to the place it is to occupy finally. Then after this, the game consists of the travels of this blank space over the board, finally ending where it started. The route pursued consists partly of tracks followed and again retraced, which make no ultimate difference to the number of disarrangements and partly of closed paths travelled round, which are cyclic interchanges of an odd number of tickets. No number of such operations can make the number of disarrangements zero if it happened originally to be odd.—Yours, &c., ARTHUR BLACK.

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[81]—As you invite correspondence, I send you the enclosed very remarkable arrangement of figures. A year or two ago I sent them to the publisher of juvenile books. I believe it was not attended to. If so, I am of opinion that it is unknown.

At first sight it seems a confusion of figures; but, on examination, the arrangement will be found to be very simple.

You will perceive that the unit 1 is placed under the central square; if you follow the figures to the end, all confusion will vanish. To understand it is a very different thing.

West Brompton, Nov. 11, 1871. H. S.

The square of *all odd* numbers may be so arranged that the totals of all the columns—perpendicular, horizontal, and diagonal—shall be the same. And the totals will be the *larger** half of the square multiplied by the odd number. Thus the totals of

5 × 25 ÷ 2 = 13 × 5 = 65, the total.
7 × 49 ÷ 2 = 25 × 7 = 175 ,,
9 × 81 ÷ 2 = 41 × 9 = 369 ,,
11 × 121 ÷ 2 = 61 × 11 = 671 ,,

SQUARES OF "ODD" NUMBERS.

5 *totals* = 65.

11	24	7	20	3
4	12	25	8	16
17	5	13	21	9
10	18	1	14	22
23	6	19	2	15

Square 25 ÷ 2 = 13 × 5 = 65, the totals.

7 *totals* = 175.

22	47	16	41	10	35	4
5	23	48	17	42	11	29
30	6	24	49	18	36	12
13	31	7	25	43	19	37
38	14	32	1	26	44	20
21	39	8	33	2	27	45
46	15	40	9	34	3	28

Square $\frac{49}{2}$ = 25 × 7 = 175, the totals.

9 *totals* = 369.

37	78	29	70	21	62	13	54	5
6	38	79	30	71	22	63	14	46
47	7	39	80	31	72	23	55	15
16	48	8	40	81	32	64	24	56
57	17	49	9	41	73	33	65	25
26	58	18	50	1	42	74	34	66
67	27	59	10	51	2	43	75	35
36	68	19	60	11	52	3	44	76
77	28	69	20	61	12	53	4	45

Square of 9 × 81 ÷ 2 = 41 × 9 = 369, the totals.

This is a known method of making magic squares. We find in the magic square for the numbers 1 to 9; thus:—

4	9	2
3	5	7
8	1	6

in which also other methods of solution are indicated.—ED.

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* The *larger* half means the actual half plus ½.

Queries.

[29]—Garden Tripod Stand for Telescope.—Could any of your readers tell me how to make a cheap garden tripod stand for a 3-in. glass, and where to get the several parts? The cheapest I can buy is £1. 1s., and that seems so dear for so simple a thing. Ignoramus.

[30]—Solar Heat.—With respect to the sun being hot, will you kindly explain to me why some countries are warmer than others? If it is because they are nearer the sun than the cold ones [but it is not—Ed.], why, then, should not the top of a mountain be warmer than the bottom? On a man going into a tropical country, he naturally gets brown-burnt by the sun (?). Does the sun act directly on the man, or does it first enter the earth, and the man, as it were gets the reflection?—Warmus.

[31]—Intensity Coils.—Can you inform me how to estimate the maximum battery-power which may be used with any given intensity coil, without incurring the risk of destroying the insulation? Can you afford me any practical hints as to the best way of restoring one so damaged without having recourse to a coil-maker? I presume it will be necessary to replace the secondary coil with a new one—but how?—Δ.

[32]—Engraving on Copper.—Want to know the usual method of engraving on copper, what the plate is covered with, and the materials used? Is it sulphate of copper and salt, or what? Engraver cannot get any cheap handbook bearing on the subject.—Engraver.

[33]—Astronomical Slides.—Can any of your readers give me a few hints how to prepare the above for a lecture on popular astronomy; also, how to make a good opaque black for same?—C. J. S.

[34]—Three-handed Chess.—Can any of your readers inform me of a way of playing chess with three players only? The four-handed game is well known to me.—Geo. H. Verney.

[35]—With reference to the calculations concerning the four asteroids and the magnitude of the third satellite of Jupiter, have there been found any great differences since 1846?—S. S. S. S.

[36]—Laplace's Theory.—Do you consider Laplace's conclusions with reference to the physical forces and evolution sound, as far as we now know?—S. S. S. S.

[37]—Vestiges of Creation.—Who is the author of "Vestiges of the Natural History of Creation," and do you consider it reliable?—S. S. S. S.

[38]—Density of Neptune.—What is the density of the planet Neptune, and how many of his satellites have been discovered?—S. S. S. S.

[39]—The Moon's Rotation.—The moon turns once on its axis in exactly the same time that it takes to revolve once round the earth. Is there any law or theory as to the cause of this, or is it a mere coincidence?—C. O. K.

[40]—Fine Drilling.—Professor Edward C. Pickering, of Harvard College, says that in undertaking to measure the intensity of the light of the satellites of Mars, he had occasion to need an extremely small hole. Among the artisans who essayed to furnish what was required was one who had succeeded in making a hole edgewise through an old-fashioned three-cent piece, and another who had pierced a needle through from end to end. A hole about the twenty-five-hundredth part of an inch in diameter was finally secured.—(*Public Opinion*, Nov. 19, 1881, p. 658.) Have you any knowledge of the above? Assuming that there is no mistake, an account of the mechanism employed, and of the boring instrument, would be of interest.—A. T. C.

[41]—The Planet Vulcan.—Is anything more known of the planet Vulcan; and is it believed to be certainly in existence?—S. S.

[42]—Magnetoscope.—Is there any instrument made for the purpose of measuring the amount of magnetism, and also electricity in the human body? I think I have heard of a "magnetoscope," but know no particulars.—S. S.

[43]—B.Sc. and D.Sc. Examinations, London University.—I am desirous of obtaining such a knowledge of science as would enable me to answer such questions as are set in the B.Sc., and even the D.Sc. examination papers of the University of London. What books would you recommend for this purpose? I shall be extremely obliged for an answer to this.—An Enthusiast.

[44]—Illumination of the Solar System.—May I, as a reader of Knowledge, request you to give me and my fellow readers some sound instruction on the subject of the new theory started by Mr. Collyns Simon, on the "Solar illumination of our system"? I have seen attention called to it by several foreign periodicals, but none by the English. Are we to accept it as possible, or reject it as impossible?—V. A. T.

[45]—Cause of Gravity.—What is the cause of gravity? There is an interesting article, if I recollect rightly, in the old "Encyclo. Lond.," giving the views of Sir Richard Philips on this matter. Philips believed that all the phenomena attributed to attraction might be accounted for by the laws of motion. Meter.

[46]—Planetary Rings. Are these not evidences of matter more or less plastic beyond the solid planetary nucleus, the phenomena presented by the denser and cloud portions of the atmospheres of planets, ranging themselves centrifugally, in belts? This globe, in my view, would appear belted in other planets. This explanation might be illustrated experimentally. Meter.

[47]—Optical Illusions. Are these not all due to the simple law of contrast?—Meter.

[48]—Trisection of an Angle.—I shall be glad if any of your readers can inform me how to trisect *any* angle. A small diagram will greatly assist.—Euclid.

[49]—Lifting Man's Weight.—Could you kindly inform me how it is that four men appear to lift a fifth so easily when they all inspire their breaths together? Is it simply a trick, or is there some scientific explanation of it? I should esteem it as a favour to have an answer in your next issue.—D. H. B.

[50]—Wasted Energy?—A certain amount of energy is supplied to an electric lamp in the form of current electricity. A small part reappears as heat, the rest as light. What form does the energy radiated as light vibrations take as the light is constantly absorbed by surrounding objects?—A. H. H.

[51]—Botany Lectures.—Can the Editor or a reader of Knowledge tell me of any first-class lectures on Botany, embracing such subjects as are not included in the ordinary lectures given at the various science schools, but are required for the S. and A. Honours Exam.? Do not any of the masters of the subject teach, either by lecture or correspondence?—Nemo.

[52]—Microscope.—Can anyone oblige by recommending what they consider from experience to be the best microscope for botanical and biological work; cost, with all necessary appliances, not to exceed £10.?—Nemo.

[53]—Suggestion.—Botanical Papers.—If the Editor of Knowledge should see fit to introduce into his useful and interesting paper a series of articles on botanical subjects, I think many readers would welcome them with pleasure, as the subject is one that is now so widely studied, and the new discoveries and theories of the learned do not appear in the text-books till long after date. A column devoted to this subject would surely be as approprioté to Knowledge as a chess or whist column. Nemo.

[54]—Chemical Questions.—1. In testing a solution of ferrocyanide of potassium with chloride of ammonia, ammonia in excess, and hydrosulphuric acid, no precipitate was obtained. Could you explain why no precipitate was thrown down, since iron is one of the third group of metals? 2. Strong solutions of bisulphate of potassium and potassic iodide were separately treated with tartaric acid. No effect, even when stirred on a glass surface. Why was there not a white precipitate following the course of the stirring-rod?—Castor and Pollux.

[55]—Future of Greek Verbs.—Is there any rule for the lengthening or otherwise of the vowel when forming the future of a pure verb in Greek. For instance:

φιλεω	makes	φιλησω
καλεω	,,	καλεσω
τιμαω	,,	τιμησω
ἐραω	,,	ἐρασω

Why should the ε be lengthened to η in φιλεω and not in καλεω?—Castor and Pollux.

[56]—Can you kindly recommend a book covering the chemistry for the South Kensington Honours Syllabus?—Castor and Pollux.

[57]—The Great Bear.—In what latitude would this constellation become invisible for three months of the year, by dipping below the northern horizon? Is the upper pointer (Dubhe) the most western star of the constellation, and the tail star (Benetnasch) the most eastern? What is the difference of celestial longitude between the most eastern and most western stars of the constellation—*i.e.*, how many hours and minutes of longitude does the Great Bear spread itself over? The Súrya Siddhánta, a textbook of Hindu astronomy, which seems to teach a modification of the Ptolemean system, attributes to this constellation a cycle of 2,700 years. The translator (Rev. Ebenezer Burgess) is at a loss to know what this refers to. Can you solve the difficulty?

Has the Great Bear been made use of by any people for measuring time, in connection with the Dog-star or any other? Bentley, in his Hindu Astronomy, says that the Hindus had the following method of measuring the amount of *precession*: They assumed an imaginary line, or great circle, passing through the poles of the ecliptic and the beginning of one of their lunar asterisms called *Maha* (instead of twelve solar signs, they had twenty-seven lunar asterisms). This great circle, which was called the line of the Rishis, was supposed to cut some of the stars of the Great Bear, and the precession was noted by stating the degree, &c., of any *movable* lunar asterism cut by that fixed line or circle as an index. That is to say, their asterisms shifted, precessionally, just as our "Aries" has got into Pisces, and they measured the amount of precession by the passage of the asterisms over the assumed fixed line. This fact suggests to me to ask whether the Egyptians made out any similar connection between the Great Bear and Sirius, or between the Pole-star and Sirius. I know that a line drawn through "the pointers" would not pass through Sirius, because the Right Ascension is so different; but is any sort of connection known to have existed? GEORGE ST. CLAIR.

[58]—SIRIUS AND ORION.—I have met with a statement that Sirius is in the shoulder of Orion. Knowing that Sirius was in the Great Dog constellation, I was puzzled. Did the Orion constellation at any time include Sirius? GEO. ST. CLAIR.

[59]—MERCURY'S REVOLUTION.—The "Science Primer on Astronomy" (by J. Norman Lockyer, F.R.S.), p. 62, says that Mercury is 84 days in traversing its orbit. Most books which I have looked into give Mercury's sidereal revolution as 87 days 23 hours 15 minutes 43 seconds. Is the sidereal revolution something different from the "traversing of the orbit?"—GEORGE ST. CLAIR.

[60]—SOUND.—How does sound penetrate through a brick wall?—S.

[61]—Will the Editor, or anyone else, inform me the name of the great comet that appeared in our northern sky this last midsummer; also, how long it takes to make one journey round the sun?—S. C. H.

[62]—ALGOL.—Where abouts in Persius is the variable star Algol? If this is the same star as in the head of Medusa, I suppose the latter constellation is a part of the former. Is it so?—F. H. S.

[63]—ALGOL AND MIRA.—When are Algol and Mira at their brightest?—F. H. S.

[64]—STAR LETTERS AND NUMBERS.—Not having examined a stellar map before those that appear in KNOWLEDGE, will you kindly explain the following? I have been under the impression that the stars of all constellations are named after the Greek letters, and when these are exhausted, the numerals are then resorted to, beginning, of course, at 1? I find this is not the case, *e.g.*, take Lynx, as marked in the map on No. 1. The brightest star is of the fourth magnitude, and is α; there are only two others marked in this constellation, but instead of their being β, γ, or 1, 2, they are called 31 and 38. F. H. S.

[65]—NEW STAR IN CASSIOPEIA.—I am told that a periodical star in Cassiopeia will shortly appear, and it will be so bright as to be seen at noonday. Is that so? F. H. S.

[66]—Has Venus ever been seen in the daytime?—F. H. S.

Replies to Queries.

[11]—LOGARITHMS.—Without knowing the extent of accuracy desired by Mr. Grundy (query 11, p. 60) in his computations, it is difficult to advise him as to the class of tables he requires. Copies of the ordinary "Seven-place" tables of Hutton, Babbage, Sang, and others are easily procured second-hand (the first for preference); German tables to six places, by Ursinus and Bremiker, can be purchased at Nutt's; and the best of several five-place logs. will be found in Galbraith & Haughton's series, now published by Cassell. This includes a table of "Sum and difference" logs. by Gauss. The last table is made specially applicable to Life contingencies and the general formation of tables by a continuous method, by P. Gray, who inserts it in his "tables and formulæ," tabulating for each log. that of the function $1+x$. An error in Babbage may here be noted. In the eight-figure series attached to his larger table, the log. of 103 (an important figure, as being the initial 1, plus its interest at 3 per cent.) should be 01283722, not 01283723. The most complete table of Anti-logarithms is the old folio of Dodson, frequently to be picked up at the stalls; see also Shortrede's tables to seven places in the usual form. Yours faithfully,

H. S. A.

[19]—COMPARATIVE ANATOMY OF BIRDS AND ANIMALS. Charles Sherborn asks, What are the corresponding bones in man and other mammals to the "furcula" (*furculum*) of birds? I am surprised Mr. Sherborn has not discovered the answer to his query in any good manual of *zoology*. The "furculum" of a bird (*anglicè*, the "merry-thought") is composed of the two collar-bones or clavicles of the bird, which are, in most cases, firmly united to form one bone. This bone, in turn, is joined to the breast-bone, and forms a strong arch, or, rather, keystone, of the shoulder. It should be remembered that in a bird's shoulder there are three typical bones—scapula, or shoulder-blade; clavicle, or collar-bone; and coracoid bone—on each side. The coracoid bone, developed as a distinct bone in birds, reptiles, and lower vertebrata, is represented in all mammals, except the lowest, as a mere process (coracoid process) of the shoulder-blade. In the lowest mammals (*e.g.*, *Ornithorhynchus* and *Echidna* of Australia) the coracoid, as in the bird, is articulated with the breast-bone. In this respect, as well as in the absence of sutures or distinct lines of union between the skull-bones, and in other joints of internal anatomy, the lowest quadrupeds present a striking affinity to birds. ANDREW WILSON.

[25]—FORMS OF FOOD. As explanatory of the terms relating to food in Dr. Carpenter's paper, which puzzle "Desdichado," I would advise him to read a simple elementary treatise wherein foods at large are treated. Such a book as Corfield's "Laws of Health," 1s. 6d. (Longmans), will assist him. Foods are divided into (1) Nitrogenous (containing carbon, hydrogen, oxygen, and nitrogen, and sometimes sulphur and phosphorus) and (2) non-nitrogenous. Of the latter, there are four chief varieties:—(1) Water (H_2O); (2) fats and oils (CHO); (3) sugars and starches (amyloids or hydro-carbons) (CHO, but differing from fats in the amount of O they contain); (4) minerals (*e.g.* lime, salt, iron, &c.) "Desdichado" will obtain all necessary information concerning foods from any primer of physiology, and such knowledge should form, indeed, part of every system of school training.—ANDREW WILSON.

TRAINING.

WE have before now spoken of the singular views which have prevailed with regard to the diet best suited for men who were desirous of developing their physical powers to the highest degree, and of the harm which has been done by the empirical, and in many cases ridiculous, rules which were laid down. Some of the most objectionable of these are now happily set aside, and the opinions of those intelligent persons who taught that mutton was better than beef for "wind," that all fluids should be avoided by men who wish to "get into condition," that meat was to be eaten without salt, and that pedestrians should drink sherry and boxers port, would be laughed at, even by the most ardent fanatic in an Eight; but, though a good deal of nonsense has been got rid of, a bad system still prevails, and there is no exaggeration in stating that harm is still done by the regulations respecting diet which, even in these days, are unhesitatingly obeyed. At one time, no doubt, some of these rules appeared to have a certain scientific sanction; but it has now been well established that the views on which this sanction was based were not only erroneous, but directly opposed to the truth. In so far, therefore, as rules which are in accordance with them have any effect, they must have a bad effect. It is true that they do not work so much ill as might be expected, but this is because the men who follow them are usually very young, very vigorous, and lead, apart from diet, a most healthy life. Still, unless modern physiological teaching is altogether wrong, even the modified system now followed must cause some evil, and the sooner it is swept away the better. If it be said that the men who train steadily often attain very "high condition," the answer is that this is due in no way to their food, but to constant and fitting exercise in the open air, to regular hours, to strict temperance with regard to alcohol, and to abstinence from, or great moderation in, smoking. Strength is attained not by diet, but in spite of diet.

That erroneous views should at one time have been held is not wonderful, for in support of them the great name of Liebig could be quoted by those who had sufficient energy and intelligence to attempt a scientific study of the question. It is now, however, well established that in some of his conclusions Liebig was wrong, and notably that he was wrong in thinking that muscular or mechanical effort was entirely supported by nitrogenous food, and that the heat-giving foods sustained the process of combustion which is constantly going on in the body, but did nothing more. If he was right, of course, the more muscular work a man did, the more nitrogenous food he would require; and trainers were therefore not mistaken in favouring meat, and in looking with great dislike on those foods which are commonly thought to produce fat; but then, unfortunately, Liebig has been shown to have been in error, and any system of diet which is in accordance with his views cannot be a good one, and is in all probability a very bad one. The

erroneous nature of views based on Liebig's doctrine is well known to physiologists; but, nevertheless, is hardly as yet so generally known as it ought to be, and very likely on the banks of Isis and Cam there is no suspicion of the truth. Possibly every year a certain number of men break down in training, with more or less injury to their constitutions, owing to a faulty diet. Instruction on this subject is therefore anything but superfluous, as a deeply-rooted error is not by any means eradicated; and all who are interested in athletic sports should welcome the appearance of two articles which a writer on physiology of the first eminence has contributed to Mr. Richard Proctor's new magazine, Knowledge—a periodical, we may observe, which promises to satisfy a want that has long been felt. In this magazine Dr. Carpenter has come forward to protest against the belief in Liebig's views which appears unfortunately still to exist. As need hardly be said, he does not write specially on training or diet, but generally respecting food material and physical effort, his articles being on "The Relation of Food to Muscular Work." They are written with all his accustomed clearness and powerful simplicity, and we hope to aid in calling attention to them, as they cannot fail to do much good if they reach those who habitually misfeed young men with a view to producing "high condition."

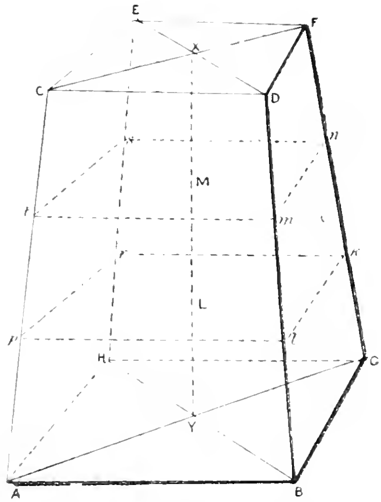
It is not necessary to reproduce here the careful and minute account which Dr. Carpenter gives of the process involved in muscular effort. Those who wish to understand this must seek the pages of Knowledge, and they will be very well repaid for their pains. His summing-up, however, which states briefly the views of modern physiologists, should be given in his own words, which are as follows:

"The *mechanical working* of the body of a living animal is as directly dependent as its *heating* upon the oxidation of the hydro-carbons of its food; and these may be most economically supplied by non-nitrogenous substances. On the other hand, the mechanism can only be kept in working order by the continual renovation of its substance (its very existence as a living whole involving the continual death and decay of its component parts); and for this renovation a supply of *proteids* is essential, with a certain admixture of *fat* to serve as a material for protoplasm."

Now it is scarcely necessary to point out how entirely these truths are opposed to the system followed in training, which did seem to receive some kind of sanction from Liebig. It is true, no doubt, that nitrogenous food is required for the renovation of the muscle, which wears out as all the tissues of the body wear out; but the consumption of muscle caused by effort which, as we have said, has been likened to the wear and tear of a machine—is small when compared with the consumption of the non-nitrogenous substances, which represent the fuel that is burnt to maintain the force developed. It is therefore clear that when there is severe and continuous physical effort, a large supply of the latter kind of food-material will be required to make good the loss occasioned by that effort, while of the former only a slight increase will be made necessary. The principle followed in training is exactly to reverse things. It is true that men are no longer encouraged to gorge themselves with underdone meat and to avoid sweets as if they were poison; but still, in the main, the trainer favours meat, and watches with some jealousy and restricts the other kinds of food. He ought to do just the opposite. We do not, of course, mean to say that the resources of the French *cuisine* should be placed at the disposal of young men in training, as it is sufficiently obvious that those who desire to attain a high state of health must confine themselves to simple and digestible food; but of simple food it is the non-nitrogenous kind that is the most needful, and it is about as reasonable to fear a large proportion of nutriment of this class as it would be to fear the oxygen of the air. Nay, more harm may be done by abstaining from the food indicated. Natural laws cannot be disobeyed with impunity; and when nature points distinctly to one kind of diet, and men choose to adhere to a diet of precisely the opposite kind, evil of some sort is not unlikely to follow. The argument that the diet is right because men who adopt it do get into "high condition" we have already answered, and there can be little doubt that the ailments which assail men in training and the occasionally serious results of training are in part due to a vicious system of diet which, in so far as it has any scientific basis, is founded on a doctrine which is now thoroughly exploded. Much, therefore, do we hope that Dr. Carpenter's valuable contributions to Knowledge will be read at the Universities and other places where there is devotion to the severer kind of athletics. If these and some other writings are studied, we doubt not that, before long, the foolish rules which still remain will be swept away, and that the happy young athletes who are able to enjoy good and wholesome dinners, which tend to produce, not to retard, the much-desired "condition," will fervently revere the name of the man of science who released the victims of training from an odious thraldom.—From the *Saturday Review*.

Our Mathematical Column.

Mathematical Query 1[?].—A mason has a block of stone as in figure, square at top and bottom. Perpendicular height, XY = 10 feet; AB, side of base, = 8 feet; CD, side of top, = 6 feet. He desires to cut it in three parts of equal solidity. Will one of your mathematical readers kindly tell him an easy way of doing it?—Cymro.



The practical answer to this question is simply to mark off Bq, Gk, Hr, Ap each equal to 2·783 ft., or to about 2 ft. 9⅜ in.; and qm, kn, rs, pt, each equal to 3·269 ft., or about 3 ft. 3¼ in., and cut off by planes parallel to $ABGH$, the pyramidal frustra pG and tF.

Presumably the block is to be cut by planes parallel to the top and bottom.

The best way of treating this question is, perhaps, the following:

The block EB may be regarded as part of a pyramid, having $ABGH$ as base. The height of this pyramid would be 40 ft., for its linear dimensions narrow one-fourth in 10 ft. of ascent, and reduce, therefore, to nought (or the vertex is reached) at four times this height.

Of this pyramid, the part or frustum EB has a volume bearing to the volume of the pyramid the ratio $(40)^3-(30)^3 : (40)^3$

$$=64-27 : 64 = 37 : 64$$

In other words, putting V for volume of pyramid, volume of block $EB=\frac{37}{64}\cdot V$. Now we require to determine two points L and M in YX, so that planes through L and M parallel to $AHGB$ may cut off from the lower part of the pyramid one-third and two-thirds respectively of the volume EB. The volumes thus cut off will, therefore, be respectively

$$\frac{1}{3}\cdot\frac{37}{64}V \text{ and } \frac{2}{3}\cdot\frac{37}{64}V, \text{ or}$$

$$\frac{37}{192}V \text{ and } \frac{74}{192}V$$

Now, if L and M be respectively distant x and y feet from the vertex of the pyramid, we have the volumes cut off from the lower part of the pyramid by the planes through L and M, respectively equal to—

$$\frac{(40)^3-x^3}{(40)^3}V \quad \text{and} \quad \frac{(40)^3-y^3}{(40)^3}V$$

$$\text{Hence } \frac{(40)^3-x^3}{(40)^3}=\frac{37}{192}; \quad \text{and} \quad \frac{(40)^3-y^3}{(40)^3}=\frac{74}{192}$$

$$\text{or } x^3=\frac{64000\times 155}{192}=\frac{155000}{3}=51666\cdot\dot{6}$$

$$\text{and } y^3=\frac{64000\times 118}{192}=\frac{118000}{3}=39333\cdot\dot{3}$$

We can [illegible] approximate solutions by logarithms, thus For [illegible], we have

$$[illegible]\ 51466\cdot67 = 4\cdot7132101$$
$$\text{cube root of which} = 1\cdot5710701 = \log.\ 37\cdot245$$
$$\text{whence } x = 37\cdot245 \text{ feet,}$$
$$\text{and } YL = 40 - x = 2\cdot755 \text{ feet.}$$

For [illegible], we have

$$[illegible] = 39333\cdot33 = 4\cdot5947608$$
$$\text{cube root of which} = 1\cdot5315869 = \log.\ 34\cdot008$$
$$\text{whence } y = 34\cdot008 \text{ feet,}$$
$$\text{and } YM = 40 - y = 5\cdot992 \text{ feet.}$$

Thus

$$YL = 2\cdot755 \text{ ft.};\quad LM = 3\cdot237 \text{ ft.};\quad \text{and } MX = 4\cdot008 \text{ ft.}$$

In practice, I suppose it would be more convenient to know whereabouts the planes of section respectively cut the lines *DB*, *FG*, *EH*, *CA*. For this purpose all we have to do is to divide each of these four lines in the same proportion as the line *XY*. This, whether it is done by construction or by computation, is too simple to need further explanation. I may just note, however, that

$$BD^2 : YX^2 :: (40)^2 + (YB)^2 : (40)^2$$
$$:: 1632 \qquad : 1600$$
$$:: 51 \qquad : 50$$

so that, for the divisions along *DB*, *FG*, *EH*, and *CA*, the above values for *YL*, *LM*, and *MX* have all to be increased in the ratio $\sqrt{51} : \sqrt{50}$ or approximating as 101 to 100. Increasing them by one-hundredth, we get, supposing *lm*, the required points of division along $DB - Bl = 2\cdot783$ ft.; $lm = 3\cdot269$ ft.; and $mD = 4\cdot048$ ft.

Our Chess Column.

The Two Knights' Defence.

We take next a sounder form of this opening, where Black, instead of retaking the Pawn at his fifth move, plays away his Queen's Knight to Queen's Rook's fourth. The game opens thus:—

1. P. to K.4 / P. to K.4
2. Kt. to K.B.3 / Kt. to Q.B.3.
3. B. to Q.B.4 / Kt. to K.B.3.
4. Kt. to K.B.5. / P. to Q.4.
5. P. takes P. / Kt. to Q.R.4.

White has now two lines of play; he may either check with the Bishop or play P. to Q.3, defending the Bishop while leaving it to protect the forward Queen's Pawn. The first, which is the better play, will now occupy our attention. Note, that whatever play is adopted, White's attack is, for the moment, over. He has to provide for the safety of his King's Knight, in attacking which Black can develope his game. The opening proceeds thus:—

6. B.Q.Kt.5ch. / P. to Q.B.3.
7. P. takes P. / P. takes P.
8. B. to K.2 / P. to K.R.3; or 8. B. to Q.R.4 / — (A)
9. Kt. to K.B.3 / P. to K.5; or 8. Q. to K.B.3 / — (B)
10. Kt. to K.5 / Q. to Q.5; or 10. — / B. to Q.3.
11. P. to K.B.4 / B. to Q.B.4. — 11. P. to Q.4 / Q. to B.2.
12. R. to K.B.sq. / Q. to Q.3. (Pos. 1.) — 12. P. to K.B.4 / Castles. (Pos. 2.)

Position 1.

Black.

White.

Position 2.

Black.

White.

In position 1 White seems to be the better off. Black has his game more open, and in fact, the attack rests for the moment with him. But the attack does not seem sufficient to compensate for the Pawn which Black has lost. It will be observed that at move 12 Black had to provide against the move P. to Q.B.3, attacking Queen, followed by P. to Q.Kt.4 attacking both Knight and Bishop. He might have done this by playing Bishop instead of Q. to Q.'s third. In this case White would still push Q.B. Pawn, attacking Q., and on her retreating to Q.'s fourth, her best move, would play P. to Q.Kt.'s fourth, with decided advantage. Or Black might have played away his Q.Kt. to Q.Kt.'s second, leaving, however, his Q.B.P. undefended, and on White taking it, Black Queen has to retreat to Q.'s third.

On the other hand, in position 2, Black's game seems as good as White's. He is a Pawn short, but his forces are better situated.

Next week we shall carry on the variations A and B, arising from White's playing 8. B. to Q.R.4, or 8. Q. to K.B.3, instead of B. to Q.2.

Young players will find it a useful exercise to play several games from each of the positions 1 and 2, taking the attack and defence alternately, and, in every case, playing through the opening moves till either position 1 or position 2 is reached.

Instead of 11. P. to K.B.4, by which position 1 is reached at twelfth move, White may play 11. Kt. to K.Kt.4. On this Black takes the Kt. with his Q.'s Bishop, White retakes with Bishop, and Black plays Kt. to Q.B.5. This leads to an even game.

Several of our readers appear to find considerable difficulty with Mr. Healey's little problem at p. 41, two declaring flatly that there must be some mistake. The solution is simply

White.	Black
1. R. to Q.Kt.7 (ch.)	1. K. to B.sq. (If K. to R. sq., there follows perpetual check, unless K. goes to B. sq.
2. R. to Q.Kt.5.	2. P. Queens.
3. R. to Q.B.5 (ch.)	3. Q. takes R., stalemate.

The other position, and Ponziani's position in our last number, are easy enough. Black's choice is divided between perpetual check and stalemate.

We give, in conclusion, a little problem of our own invention. It presents no difficulty whatever for those who are at all proficient in problem-solving; but may amuse, for ten minutes or so, those who have not given much attention to that department of Chess. (The very feature which makes the problem interesting for these directs the former at once to the solution.)

No. 3.—Problem by the Editor.

Black.

White.

White to play and mate in two moves.

Our Whist Column.

By "Five of Clubs."

Again we give a simple whist game, taken from actual play. It is borrowed from the "Westminster Papers" for 1877. The first player, *A*, was our correspondent (and friendly critic), Mr. Lewis, whose Double Dummy problems long formed so marked a feature of the "Westminster Papers." We give this time *B*'s inferences, with notes on the play as before. The game is interesting as showing how a good player may be led by the fall of the cards in the first two or three rounds to lead out trumps even when short in them, and when one of the adversaries probably holds four.

We show also how two tricks might have been saved by correct play of the weaker hands, Y and Z.

THE HANDS.

Spades—Kn. 10, 3.
Hearts—6, 2.
Clubs—Q. 7. 5, 2.
Diamonds—A. 8. 4. 3.

Spades—Q, 6, 5, 2.
Hearts—A. Kn, 8.
Clubs—10, 9, 8.
Diamonds—10, 6, 2.

B

Y — Z Dealer

Trump Card, Spades 4.

A

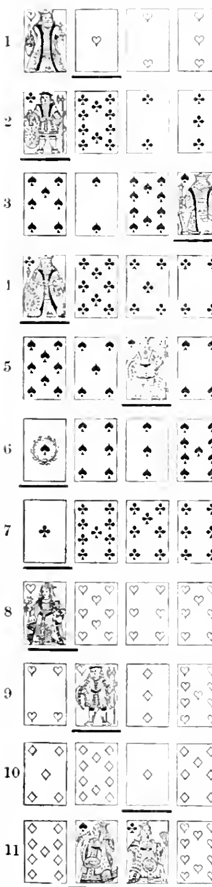
Spades—K, 9, 4.
Hearts—10, 9, 7, 3,
Clubs—6, 4, 3.
Diamonds—Q. Kn. 7.

Spades—A, 8, 7.
Hearts—K, Q, 5, 4.
Clubs—A, K, Kn.
Diamonds—K. 9, 5.

Score.—A B = 0; Y Z = 0.

NOTE.—The underlined card wins trick, and card below it leads next.

A *Y* *B* *Z*



B's INFERENCES.

1. *A* has Queen of Hearts and probably two other Hearts; neither *A* nor *Z* (who has not signalled) is very strong in trumps.

2. *Y* has led from 10 C and two small ones, probably from 10, 9, 8; *Y's* hand must be very weak, but in all probability he has four trumps. *A* has Ace of Clubs and King of Clubs.

NOTE TO TRICK 2.—With such a hand *Y* should have led a trump.

3. *A* has strength enough in trumps, with command in other suits, to justify a trump lead. (This is *B's* inference, and also our comment.)

4. *Z*, if he has played rightly, has no more Clubs. 4 C was the lowest Club in hand, and *Z* cannot have two more, for *B* can place four of the remaining five, viz., 9 C with *Y*, Ace with *A*, and two in his own hand. But *B* knows that 6 C cannot be with *Y*, and if with *A*, then *A* would have originally led Clubs unless holding at least three Hearts besides Ace and King. *B* is for the moment liable to be misled by *Z's* false card.

NOTE TO TRICK 4.—*Z* plays badly in returning his partner's lead when Honours in the suit are declared against them. If leading Clubs at all he should have led 6 C, if only for the sake of uniformity.

5. The Ace of trumps lies with *A*, the Queen with *Y*.

NOTE TO TRICK 5.—*Y* might well have played his Queen of Spades. The Ace certainly does not lie with *B*, and is far more probably with *A* than with *Z*. Even if with *Z* there is the chance of *Z* having also a small trump.

6. The long trump is with *Y*.

7. *Z* played a false card, or at least incorrectly, in trick 4.

8. *A* holds 4 and 5 of Hearts, see Trick 1.

9. 10 H is probably with *Z*.

10. *B* knows that *Y* has led from a weak suit, not from King, Knave, 10, for otherwise *Y* would not originally have led a Club. He knows also that *Z* cannot be strong in Diamonds, or he would not have returned Clubs in which his partner showed weakness. The King of Diamonds probably lies with *A*, as *A* led trumps from so weak a suit as Ace to three. *B* therefore boldly plays his Ace of Diamonds, and with the long Club forces out the long trump.

11. It matters not how *Z* discards. If he discards a Diamond *A* discards 5 H, and wins the remaining tricks, as in the actual game.

A B make three by cards. Had *Y* led trumps at trick 2, or played his Queen of Spades at trick 5, *A B* could have made but two tricks. But the game might have gone better yet for *Y* and *Z* if, *Y* having led as he actually did, *Z* had led 7 H (bad as returning an opponent's lead usually is) at trick 4. This would have fallen to *A's* Queen; and *Y*, knowing *Z* with two more (for he never would have returned opponent's lead unless with four, at least, of the suit originally), would place 9 H and 10 H at once in *Z's* hand (having the 8 himself). *Y* would therefore play away his Knave of Hearts. *A* would have followed with a trump lead as in the actual game, *Y* taking the trick with his Queen. *Y* would not then lead 8 H, because, though his partner could win with 9 H, and then lead the winning Heart, *B* would ruff, lead a Club for his partner to take, who would then lead trumps, &c. But, leading a Club, *Y* would throw the lead again into *A's* hand, who would then have taken out another round of trumps, and playing then Ace of Clubs, as in trick 7 of actual game. If next *A* played a small Heart, *Z* would take the trick with 9 H and another with 10 H. If *A* led a small Diamond, *B*, winning with the Ace, would force the long trump with Queen of Clubs, and on *Y* leading 8 H, *Z* would make both his hearts as before. Or if *B* returned a Diamond instead of leading his Queen of Clubs, then, whether *A* led a Diamond or a Heart after taking the trick with his King, the remaining tricks would be with *Y Z*. The last five tricks would run in one or other of the following ways, *A* leading in trick 9:—

	A	*Y*	*B*	*Z*	*A*	*Y*	*B*	*Z*	*A*	*Y*	*B*	*Z*
9.	4 H	8 H	3 D	9 H	5 D	10 D	A D	7 D	5 D	10 D	A D	7 D
10.	5 H	2 D	4 D	10 H	4 H	Q S	Q C	Kn D	K D	2 D	3 D	Kn D
11.	5 D	10 D	A D	7 D	5 H	8 H	3 D	9 H	9 D	6 D	4 D	Q D
12.	9 D	Q S	Q C	Kn D	9 D	2 D	4 D	10 H	4 H	8 H	8 D	10 H
13.	K D	6 D	8 D	Q D	K D	6 D	8 D	Q D	5 H	Q S	Q C	9 H

The other eight tricks would be the same respectively as 1, 2, 3, 8, 5, 4, 6, and 7 of the annual game, except that at trick 4, corresponding to trick 8 of the actual game, *Y* would play Knave of Hearts instead of 8 H, while at trick 5 (of both games) he would play Queen of Spades, and *B* 3 S, the 6 S and Knave of Spades falling at trick 7 (corresponding to 6 of actual game). *Y* and *Z* would have lost only the odd trick, which, with such wretched hands, would have been getting off easily.

THE TELEPHONE AS AFFECTED BY SOME METEOROLOGICAL PHENOMENA.—Several Continental observers appear to have been lately studying the sounds which may often be heard in a telephone that is connected with a wire stretched, say, between the roofs of two houses, and connected with the water or gas pipes. On the occurrence of lightning, more especially, sounds are heard, and at the same instant (according to M. René Thury, of Geneva) as the flash is seen, whatever the distance of the latter. Even when no thunder was heard, and the discharge must have been at least 35 kilomètres off, M. Thury observed these induction effects. M. Lalagade, who has experimented similarly for some time past, thought to amplify the sounds, and did so by placing two microphones on the plate of the receiving telephone. The arrangement is set up in a quiet room, where all foreign vibrations are guarded against, and the author is able to hear the least sound at a distance of one mètre or more from the second telephone. Again, M. Landerer, at Tortosa, finds currents produced in his telephone-circuit by atmospheric electricity in three different ways. First, the condensation of aqueous vapour results in a sound recalling the cry of tin. A sensitive galvanometer in the circuit is not, or hardly, affected. These sounds are strongest at night. Next, there are the sounds which occur during lightning (and the currents producing which affect a galvanometer considerably). Thirdly, the wind generates currents which do not act on the telephone, but act on the galvanometer strongly. At Tortosa the very dry west winds produce the greatest oscillations. Telluric or earth currents set both on the galvanometer and on the telephone; they are distinguished from atmospheric currents by the regularity and continuity of their action during pretty long intervals.—*The Times.*

Answers to Correspondents.

*** *Communications for the Editor requiring early attention should be addressed to the Editor of* Knowledge, *74 and 75, Great Queen-street, London, W.C.*

Hints to Correspondents. *1. No questions asking for scientific information can be answered through the post. 2. Letters sent to the Editor for correspondents cannot be forwarded; nor can the names or addresses of correspondents be given in answer to private inquiries. 3. No queries or replies savouring of the nature of advertisements can be inserted. 4. Letters, queries, and replies are inserted, unless contrary to Rule 3, free of charge. 5. Correspondents should write on one side only of the paper, and put drawings on a separate leaf. 6. Each letter, query, or reply should have a title, and in replying to letters or queries, reference should be made to the number of letter or query, the page on which it appears, and its title.*

John Hampden. Let us take it for granted that I am the "coward and sneak" you consider me; "my friend" the eminent naturalist you name (whom I have never met in my life), "a cheat, a swindler, and a shuffling cur," and our learned societies "selfish, dishonourable, and mischievous conspirators." Oblige, also, by noting outside your letters an address to which they can be returned.—Editor of British Chess Magazine. Many thanks.—J. Baxendell. In saying that the coincidences appear to me to disprove instead of proving your position, I refer to the circumstance that several of them are obviously mere coincidences, existing as such apart from the pyramid or its measures. They show that coincidences of the most remarkable kind can be found in this case, and presumably, therefore, in other cases, by those who look for them, and yet be, to all intents and purposes, meaningless, because accidental. Coincidences, therefore, do not supply that convincing evidence which the believers in what may be called the Great Pyramid religion finds in them.—W. Probyn Nevins. You are right; the comet is that of 1680. The comet of 1861, referred to at p. 28, was not seen by so many as the comet of 1858, being conspicuous only for a few days, and in the early morning, whereas the comet of 1858 was for months a brilliant object in the skies. But the comet of 1861 was in some respects even more remarkable.—A Fellow of the Royal Astronomical Society. You might regard me as "the kettle objecting to the pot its sordid superficies," for, like you, ... but my zeal is tempered now by the consideration that the world is censorious, and easily led to misinterpret warmth as necessarily implying personal feeling. Have we not both found it so? Moreover, most people are more easily pained than I should have thought possible when I first took part in controversy.—S. Biddle. It would save me a good deal of trouble to exclude the fifth magnitude stars; but many who use the maps want to have these stars in. It is very easy for those who want them excluded to take pen and ink and put them out.—J. I. Leighton. Occasional papers on quantitative chemical analysis might be admissible, if popular in tone; but at present we certainly have not room for a series of papers on the subject.—J. Norman Lockyer. We regret that you cannot at present spare time. Prof. Young has promised to write on the subject, and your treatment of it from another point of view would doubtless have been interesting to our readers. Perhaps at some future time you can oblige us.—W. Simons. The quotation was sent in a letter from an eminent physician. Did not notice the inaccuracy. The lines run, as you say—

Ignorance is the curse of God,
Knowledge the wing wherewith we fly to Heaven.

Thanks for encouraging words in reference to Knowledge.—J. W. C., Averillo. Thanks; corrected in Number 4. We began with the idea of a fortnightly map of the whole heavens, which would have made the pictures too small in scale. In changing our plan the mistake you notice arose, the first, third, fifth, &c., maps being taken with dates and hours, for the second, fourth, and sixth, instead of the true dates and hours.—Mar. S. Ridley. Book received, and forwarded to an enthusiast on the subject, for notice.—G. E. Bonney. Many thanks. The bookseller who thought the public likely to be interested in Knowledge would be less than we expected has certainly not thus far been confirmed by the event. The sale we hoped to attain in perhaps half-a-year or a year has already been exceeded twofold. Booksellers have a theory that nine-tenths of the journals started from time to time perish at the critical sixth number; possibly when we have reached the seventh your bookseller may have faith in our existence thereafter. We shall see.—J. B., *Norwich Advertiser.* Thanks.—F. C. Armstrong. Your query is not suited to our columns, though legal matters may be regarded as included in knowledge taken in its wider aspect.—P. A. Fothergill, F.R.A.S. Thanks.—Euclid. We have inserted your query; but though you may be readily shown how to trisect an angle by aid of a hyperbola, or the cycloidal curve called the *trisectrix*, you may depend no one will ever show you how to trisect an angle by use only of the straight line and circle, as in Euclid.—A Student. We are not free to comment on instruments advertised in our columns, but in this case the names of the advertisers stand very high indeed. We feel satisfied that you could rely entirely on any statement their house might make as to the qualities of their instruments.—J. H. Hodd. Many thanks. Mr. Foster has some kindred illusions already in hand, and finds the explanation in a peculiarity of Fig. 4 in No. 1. Your illusory picture shall be added to his collection, drawn as you suggest.—H. Griffith. Thanks for encouraging letter.—Henry Norton. You are severe, so far as we can make out your letter. Possibly you consider that readable writing and ordinary spelling would have been thrown away on persons so "binevolint" as to cater for "circle squairers, petty schoolmasters, and enquiring schoolboys." Your letter is absolutely alone thus far, and alone among so many, that we venture to conclude you are as exceptional in your views about Knowledge as in your system of orthography.—Jas. O. Jackson. On consideration, does it not appear to you that your request is a little unfair? To oblige many readers, we printed the map on a separate leaf. It is thus better printed, our number is enlarged by a full page of matter, and the map is made more convenient for study. As a reward for thus considering our readers' wants, you ask us to go to the expense of having each map separately pasted in. Apart from the consideration that most of our readers prefer to have the map loose, do you not think that your Society could pin or paste the map in? We printed 20,000 copies of each of the last two numbers, and these 20,000 extra leaves added considerably to the cost of those numbers; to have had them pasted in or an extra fold made for each would have involved additional expense, and have been unsatisfactory to many of our readers.—G. W. Niven. Your long article on "Clairvoyance" (seventeen very large and closely-written pages of M.S.) would occupy much more space than we can afford even for objects otherwise perfectly suitable for our pages; your subject can scarcely be so described.
F. C. We shall be delighted to receive your paper on "The Place of Dreams in the Growth of Primitive Beliefs."—Andrew Aitken. Thanks for the constructions of parabola, which shall appear if we can make space. First and fourth are scarcely suitable for "Geometricus," being so obviously deducible, one from the definition of a parabola, the other from the fundamental relation between the ordinate and the abscissa, that he could not have failed to recognise them. He wants an easier construction. The two others are in reality one, depending on the proposition that a series of equidistant concentric circles cut a series of equidistant parallels in a series of parabolas, having the centre of the circles as focus, and a line through that point perpendicular to the parallels as axis. There is a similar property for the ellipse, and the hyperbola, equidistant concentric circles replacing the parallels. Diagrams illustrating these relations have already been drawn for Knowledge, in accordance with promise made at foot of "Geometricus's" letter. There are simpler and better methods than these, however.—M. E. Pendred. Your first letter does not appeared, because unsuitable. If you are so fearful of science you had better leave it alone. We cannot strengthen your faith; we can only wonder why you are troubled.—John Steele. Many thanks for proffered M.S. (15 pp. closely written foolscap) in favour of the sun's being rigidly cold, and showing that we are chiefly indebted to water for both light and heat. But please don't.—Henry Grimshaw. Your illusions next week, if possible. We know nothing ourselves about the Extract referred to; the advertising and editing are distinct departments.—A Lover of Knowledge. Declining and inclining are different matters. The lines of the address, which are really horizontal, appear to decline—that is to be lower down—on the left. As to the other point, we labour under great difficulties with regard to correspondence. We should require twice the space we have, to find room for all. We will do our best to make room early for all that needs early publication, and in the long run for all that is worth preserving.—Ignoramus. You misinterpret my remark in "Half-hours with the Telescope." I do not say Mars shows a larger disc or more striking features, but the reverse. What I say is, that we see him on a larger scale; as, of course we do, seeing that he is much nearer the earth. No doubt you were looking at Mars. He is a disappointing object in the telescope.—B. H. Thwaite. Your letter is too long for what, after all, is an exposition of a very unsound doctrine. Take such a sentence as this:—"May not the rings of the planet Saturn be produced owing to that planet having two atmospheres capable of absorbing light from the solar rays?" Without wishing to discourage your efforts at original theorising, or to quote authorities against you, we must yet point out that a study of the facts known about Saturn's rings will enable you at once to decide that your explanation "may *not*" be accepted. Again we find a great pressure on our correspondence columns. Our own letter, promised in Number 4, was the first sacrificed.

Synchronizing Electric Clocks.—At the first ordinary meeting of the Society of Telegraph Engineers and of Electricians for the autumnal session, Professor G. C. Foster, president, in the chair, a paper was read by Mr. John A. Lund, on "The System of Synchronizing Clocks adopted in London and elsewhere." Mr. Lund said his paper was an attempt to demonstrate the results to be obtained by even a limited acquaintance with the science of electricity when patiently and perseveringly brought to bear upon some of the practical demands of daily life. The attention of electricians in the early days of their science was, he stated, as much devoted to synchronizing clocks as to message telegraphy, but the repeated failures in the former field, as contrasted with the triumphant successes of the latter, caused the application of electricity to telegraphy to become the favourite path of the electrician. Electric clocks were capable of being divided into six kinds. None of the old forms of electric clocks obtained a general public acceptance, the most successful, the Wheatstone, having only worked some five or six clocks from one motor. Their failure was due to the desire to make the clock subservient to the system, instead of *vice versa*. The criteria of a successful system of synchronizing, to obtain the approbation of the public while receiving the sanction of science, were that each clock should have a vitality distinctly independent of the time signal, and not stop through a failure of the regulating electric current to reach it; universal application to all kinds of existing clocks; lastly, due provision for the correction of errors. The system exhibited satisfied these conditions. Mr. Lund proceeded to say that, however perfect the mode of synchronising might be, it could not be successful without a complete "system." The system consisted of a correct standard clock, which, assisted by a well-erected and well-maintained system of telegraph wires, should send out the needed signals to the synchronisers. Two "standards" were used (one to fall in automatically should the other fail) for transmitting the currents of electricity, and those standards received their motion from a standard clock. The applicability of time current wires for telephone purposes was instanced by telephones being placed at each end of a circuit between the lecture-room (Institution of Civil Engineers) and Messrs. Barraud & Lund's establishment, Pall Mall, upon which wire there were also twelve electric clock synchronisers, through which conversation was carried on.

Contents of Knowledge *No.* 4.

KNOWLEDGE
AN ILLUSTRATED
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PLAINLY WORDED - EXACTLY DESCRIBED

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CONTENTS.

DREAMS.

By Edward Clodd.

The remarks which follow some questions concerning the attitude of science towards dreams, asked in Knowledge of Nov. 25,* indicate how belief in their quasi-supernatural character lurks in the minds of intelligent persons who would resent being called superstitious.

Certainly, the antiquity and persistence of that belief are small matter of wonder when we reflect that the phenomena of dreaming are precisely of a character to sustain that feeling of mystery which man's surroundings awaken within him; but an inquiry into its origin and growth may best dispel it, while such an inquiry will add its witness to that of the "great cloud of witnesses" concerning the survival, often in least suspected form, of rude primitive philosophies among the elaborated beliefs of civilised races.

The youngest and most vigorous of the sciences, Anthropology, has already made us familiar with the nature of a vast body of evidence, uniform in character, unearthed from old river-valleys, caverns, mounds, and tombs, witnessing to the primitive savagery of man and his slow uprising therefrom; but such evidence touches us only on the intellectual side. Even should desired skeletons of veritable men of miocene times—still better, of the "missing" *homo simius*—turn up, we should yet be within the limits of palæontology and zoology. Such relics of our remote ancestry would remain specimens only—"a little less than kin." It is not until the evidence from the Drift and from surface remains (about which Knowledge may hereafter tell its readers more in detail) gives place to that supplied by immaterial relics—articulate speech, myths which were for the time real, and sufficing explanations to him—that man touches us as *fellow*-man, as *thinker*,† striving to read "the riddle of the painful earth," and to peer into the mysteries of being.

* "Query 21," p. 80.

† "Man, a derivative root, means to think. From this we have the Sanskrit manu, originally thinker, then man."—Max Müller's *Lect. Lang.* I., 437.

Now, for the purpose of this inquiry, it is needful to have understanding of the mental condition of races in low stages of culture, and, generally, it may be said that the modern savage is, as the primitive savage was, in a state of "fog" concerning the nature and relation of what is in the mind to what is outside it. In this he may perchance command the sympathy of the modern philosopher, there being this important difference between the two, that while the philosopher speculates upon the nature of the connection between his mind and the external world, and confesses that "his knowledge of matter is restricted to those feelings of which he assumes it to be the cause," the savage has no capacity for such thought at all. He has nothing in his slender stock of words corresponding to the terms "objective" and "subjective;" that stock has no substantive verb "to be"—as, indeed, few of the languages of the world have ever had. He cannot distinguish between an idea and an object, an illusion and a reality, a substance and its image or shadow; and under bodily ailment, indigestion born of gorging, or delirium caused by starving, gives shape and substance, a "local habitation and a name," to "airy nothings," spectres of diseased or morbid imagination. Misled by superficial resemblances, he jumps at the most absurd conclusions; ignorant of the necessary relation between cause and effect, he is "carried about with every wind of" fancy; nor has he the capacity, which is the measure of intellectual growth, to strip the special of its accidents, and sink it in the general.

For example, he gives a different name to the tails of various animals, but has no name for "tail" in general; he can speak of sunshine, candle, fire-flame, etc., but "light" is an abstract term which he is unable to grasp. Such is his confusion between a thing and its symbol, that the name of a man is held to be an integral part of himself; he shrinks from revealing his own, lest the man to whom he imparts it injures him through it; still more does he recoil from naming the dead, or powers credited with baleful influence. He dreads having his portrait taken, feeling that some part of himself has gone in the process; the better the likeness, the more has "virtue gone out of him." Catlin relates that he caused great commotion among the Sioux by drawing one of their chiefs in profile. "Why was half his face left out?" they asked; "Mahtoochega was never ashamed to look a white man in the face." The chief himself did not take offence, but Shonka, the Dog, taunted him, saying, "The Englishman knows that you are but half a man; he has painted but one-half of your face, and knows that the rest is good for nothing." Which led to a quarrel, and in the end Mahtoochega was shot, the bullet tearing away just that part of the face which Catlin had not drawn.

We may now more clearly understand how the savage will interpret phenomena of a more complex order, and why he can interpret these only in one way. The phantasies which have flitted across the brain in coherent order or unrelated succession when complete sleep was lacking, leave the traces of their passage on the memory, and they are strong of head and heart, "true pepties who have no system," as Carlyle says, whose awakened consciousness is not affected by the harmonious or discordant, the painful or pleasant, illusions which have composed their dreams. But while for us they fill an empty moment in the telling, albeit now and again causing "eerie" feelings, and quickening such remains of superstition as slumber in the majority of us, they are to the untrained intelligence of the savage as solid as the experiences of his waking moments, true not only "while they last," but for ever afterwards. And the limits of his language only deepen the confusion within him when he tells what he has seen, and heard, and felt, and

whither he has been. For the speech cannot transcend the thought, and, therefore, can represent neither to himself nor to his hearers the difference between the illusions of the night and the realities of the day. The dead relations and friends who appear in dreams and live their old life; with whom he joins in the battle or the chase; with whom, the toils over, he sits down to feast, not, like the Psalmist, in the presence of his enemies, but upon succulent slices of the enemies themselves; the foes with whom he struggles, the wild beasts from which he flees, or in whose grip he feels himself, and, shrieking, awakens his squaw; the long distances he travels to dreamlands beyond and above—are all real, and no "baseless fabric of a vision." The belief is strengthened by that intensified form of dreaming called "nightmare,"* when gaping, grinning, spectre-monsters sit upon the breast, stopping breath and paralysing motion, which has helped to create the vast army of nocturnal demons that fill the folk-lore of the world, and that under infinite variety of hideousness have had lodgment for centuries in the beliefs of higher races.

What Schoolcraft says of the Indian mind, that "a dream or a fact is alike patent to it," applies throughout the whole range of the lower culture, a marked and widespread form of the confusion being in the belief that the soul leaves the body during sleep. Among the Zulus, when dead relatives appear to a man in his sleep, he concludes that their spirits still live, and the savage notion, that a sleeper should not be wakened, because of the possible absence of his soul, finds some continuity in the belief of mediaeval times, that trance and catalepsy were proofs of the temporary departure of the soul from the body. Hence, as Mr. Fiske has remarked, "it was no easy matter for a person accused of witchcraft to prove an *alibi*: for to any amount of evidence showing that the body was innocently reposing at home and in bed, the answer was obvious, that the soul may, nevertheless, have been in attendance at the witches' Sabbath, or busied in maiming a neighbour's cattle!"

(To be continued.)

INTELLIGENCE IN ANIMALS.

WIGAN, in his "Duality of the Mind," says that he once offered an apple to an elephant, letting the apple drop at the moment the elephant was about to seize it, so that it rolled out of its reach. The elephant waited a moment to see if Wigan would pick it up, and being disappointed in this expectation, set himself to blow violently against the opposite wall, and the recoil forced the apple to his feet. This may be regarded as a case of practical, rather than of abstract reasoning. Yet, as Wigan remarks, it was a trick which no one could have taught the animal, and "it must have arisen from a process of reflection perfectly similar to that which takes place in the human mind" under similar conditions. We have, indeed, he justly remarks, "examples of human minds not even capable of the degree of thought possessed in this instance by the elephant, yet performing, by a sort of automacy (*sic*) all the ordinary functions necessary to their occupation. In some of the mechanical processes in our great manufactories, where the minute subdivision of labour reduces the art of each individual almost to the very ultimate elements of muscular motion, I think that I have seen individuals incapable of a similar process."

In the following instance, from the same work, we have ingenuity combined with, and suggested by, indignation: "A large grey spider established himself in a recess formed by a shed and a projection of the house, and taking his long line diagonally from the corner of the house to the eaves of a small building which was at the bottom of the recess, he then filled up the triangular space with a well-defined circular web. I had noticed with admiration during the day his wonderful skill, the accuracy of his lines, and the equality of the spaces, and observed how carefully he pushed down his line, and fastened it securely with his two hind feet to each radius in succession. When he had finished about two-thirds of his concentric circles, or rather of his helix, he went to the centre and swallowed a quantity of white tenacious mucus, which he had deposited there at the commencement, having apparently spun himself out; he then proceeded to complete his work, which having accomplished, and thus reduced himself to very small dimensions, he hung himself up by the hind legs, and I presume went to sleep. The slightest touch of a fly was, however, sufficient to make him start out, and having wrapped up a few of them in his toils and well stocked his larder, he again betook himself to repose. In the meantime, one of the smaller spiders, considering that the diagonal line of his neighbour was strong enough to bear two webs, began to attach his lines to it, and having so done in four or five places, proceeded to spin his own web. My older friend tolerated the intrusion very patiently, and acquiesced in the use his neighbour was making of the "party wall," though against *spider law*. By-and-by the newcomer, having partly fitted up his own trap, and finding that no flies came into it, observing, I presume, the ample supply of food in his neighbour's premises, advanced along one of his own lines, seemingly for the purpose of open burglary. My old friend had tolerated much, but this was a degree of impudence for which he was not prepared, and which he determined to punish forthwith. He proceeded to the centre of his web, and giving the whole framework a violent shake, hoped to shake the intruder down upon the ground. He did no more, however, than turn him round on the line, where he hung very patiently till the shaking ceased, and then resumed his march towards his neighbour's territory. Again and again, and with increasing violence, did the large spider shake his web—it was all in vain; there was the enemy advancing, and though so small as to be easily overpowered, should he reach the mainland, the insult of the attempt was intolerable. On looking round, my elder friend saw that, during the violent shakes, he had broken two or three of his own short lines, and he left his opponent and set himself to work to mend them. Having completed the task to his perfect satisfaction, he returned to the burglar. The latter, when he came near, saw at once that he had been rash in provoking such an enemy, and hurried back to his own web. When his opponent saw him on his thin line in his retreat, he again set himself to his shaking fit, and made the most violent efforts to throw him down; it was all in vain, however, and he got safe home. After a moment's consideration, the other seemed to think that so audacious an attempt ought to be condignly punished, and he determined to retort the invasion. The thin lines of his diminutive antagonist, however, did not afford a sufficient support for his heavy bulk, and as he advanced, he carefully spun a strengthener upon the other's tenuous cord. It was now the little one's turn to shake off the intruder, and twice did he break the thin part of the line, and leave his enemy dangling. At last, the latter gave up the attempt, and went back to the centre of his own web, *after carefully detaching every one of the lines* which his neighbour had had the impudence to fasten to his long diagonal."

* ... Night-spirit. A.S. mare, nymph.

In this case we seem to recognise on both sides reasoning which approaches at times the abstract. In the calculation of means to an end, and change of plan in consequence of unexpected obstacles, there is practical reasoning. As Wigan well says, "Had the human race spun webs, and dared one another to single combat, they could not well have shown more judgment and skill in the attack and defence. The strengthening of the lines to bear the shaking, and doubling the smaller spider's lines while using them as lines of advance, belonged also to the order of practical reasoning, though of a rather advanced kind. But there was abstract reasoning, it seems to us, or a near approach to it, in the conduct of the smaller spider, first of all, in considering, as it were, how far he might trespass on the patience of an enemy whom he recognised as his superior, and again in the conduct of the larger in deciding when the time had come to give his small enemy a lesson, and in retreating finally without persisting, as if reflecting that his purpose was as well achieved as though he had actually driven the smaller spider from his web. His removing the lines which had supported the smaller web, though he had previously allowed them to remain, looks very much like a result of abstract reasoning.

We find illustrated by such instances the remark of Dr. Prichard, that among insects, if we take the different tribes collectively, manifestations of all the psychical qualities which we observe in mammifers and birds (regarding as a whole the properties divided among different departments), may be recognised in the most strict analogy. Attention, memory, the faculty of combining means to attain ends, cunning, the desire of revenge, care of offspring, and all the other psychical qualities which have been traced in the former class of animals (mammifers) are likewise to be observed in the latter as typical or characteristic phenomena—sometimes in one combination, sometimes in another; or, in different groups, sometimes strongly, sometimes more feebly expressed.

SOLIDS, LIQUIDS, AND GASES.

By W. Mattieu Williams.

PART IV.

As already explained, all gases are now proved to be analogous to steam; they are matter expanded and rendered self-repulsive by heat. All *elementary* matter may exist in either the three forms—solid, liquid, or gas, according to the amount of heat and pressure to which it is subjected. I limit this wide generalisation to *elementary* substances for the following reasons.

Many compounds are made up of elements so feebly held together that they become "dissociated" when heated to a temperature below their boiling-point. Or their condition may be otherwise defined by stating that the bonds of chemical energy, which hold their elements together, are weaker than the cohesion which binds and holds them in the condition of solid or liquid, and are more easily broken by the expansive energy of heat. To illustrate this, let us take two common and well-known oils, olive oil and turpentine. The first belongs to the class of "fixed oils," the second to the "volatile oils." If we apply heat to liquid turpentine, it boils, passes into the state of gaseous turpentine, which is easily condensible by cooling it. If the liquid result of this condensation is examined, we find it to be turpentine as before. Not so with the olive oil. Just as this reaches its boiling point, the heat, which would otherwise convert it into olive-oil vapour, begins to dissociate its constituents, and if the temperature be raised a little higher, we obtain some gases, but these are the products of decomposition, not gaseous olive oil. This is called "destructive" distillation.

In olive oil, the boiling-point and dissociation point are near to each other. In the case of glycerine, these points so nearly approximate that, although we cannot distil it unbroken under ordinary atmospheric pressure, we may do so if some of this pressure is removed. Under such diminished pressure, the boiling-point is brought down below the dissociation point, and condensible glycerine gas comes over without decomposition.

Sugar affords a very interesting example of dissociation, commencing far below the boiling point, and going on gradually and visibly, with increasing rapidity as the temperature is raised. Put some white sugar into a spoon, and heat the spoon gradually over a smokeless gas-flame or spirit-lamp. At first the sugar melts, then becomes yellow (barley sugar); this colour deepens to orange, then red, then chestnut-brown, then dark brown, then nearly black (caramel), then quite black, and finally it becomes a mere cinder. Sugar is composed of carbon and water; the heat dissociates this compound, separates the water, which passes off as vapour, and leaves the carbon behind. The gradual deepening of the colour indicates the gradual carbonisation, which is completed when only the dry insoluble cinder remains. An appearance of boiling is seen, but this is the boiling of the dissociated water, not of the sugar.

The dissociation temperature of water is far above its boiling-point. It is 5,072° Fah., under conditions corresponding to those which make its boiling-point 212°. If we examine the variations of the boiling-point of water, as the atmospheric pressure on its surface varies, some curious results follow. To do this the reader must endure some figures. They are extremely simple, and perfectly intelligible, but demand just a little attention. Below are three columns of figures. The first represents atmospheres of pressure—*i.e.*, taking our atmospheric pressure when it supports 30 inches of mercury in the barometer tube as a unit, that pressure is doubled, trebled, &c., up to twenty times in the first column. The second column states the temperature at which water boils when under the different pressures thus indicated. The third column, which is the subject for special study just now, shows how much we must raise the temperature of the water in order to make it boil as we go on adding atmospheres of pressure, or the increase of temperature due to each increase of one atmosphere of pressure. The figures are founded on the experiments of Regnault.

Pressure in Atmosphere.	Temperature F. °	Rise of Temperature for each additional Atmosphere.
1	212	37·5
2	249·5	23·8
3	273·3	17·9
4	291·2	14·8
5	306·0	12·2
6	318·2	11·4
7	329·6	9·9
8	339·5	8·9
9	348·4	8·2
10	356·6	7·6
11	364·2	6·9
12	371·1	6·7
13	377·8	6·2
14	384·0	6·0
15	390·0	5·4
16	395·4	5·4
17	400·8	5·1
18	405·9	4·9
19	410·8	4·6
20	415·4	—

It may be seen from the above that, with the exception of one irregularity, there is a continual diminution of the

additional temperature which is required to overcome an additional atmosphere of pressure, and if this goes on as the pressure and temperatures advance, we may ultimately reach a curious condition—a temperature at which additional pressure will demand no additional temperature to maintain the gaseous state; or, in other words, a temperature may be reached at which no amount of pressure can condense steam into water, or where the gaseous and liquid states merge or become indifferent.

But we must not push this mere numerical reasoning too far, seeing that it is quite possible to be continually approaching a given point, without ever reaching it, as when we go on continually halving the remaining distance. The figures in the above do not appear to follow according to such a law—nor, indeed, any other regularity. This probably arises from experimental error, as there are discrepancies in the results of different investigators. They all agree, however, in the broad fact of the gradation above stated. Dulong and Arago, who directed the experiments of the French Government Commission for investigating this subject, state the pressure at 20 atmospheres to be 418·4 at 21=422·9, at 23=427·3, at 23=431·4, and at 24 atmospheres, their highest *experimental*, limit 435·5, thus reducing the rise of temperature between the 23rd and 24th atmosphere to 4·1.

If we could go on heating water in a transparent vessel until this difference became a vanishing quantity, we should probably recognise a visible physical change coincident with this cessation of condensibility by pressure; but this is not possible, as glass would become red-hot and softened, and thus incapable of bearing the great pressure demanded. Besides this, glass is soluble in water at these high temperatures.

If, however, we can find some liquid with a lower boiling-point, we may go on piling atmosphere upon atmosphere of elastic expansive pressure, as the temperature is raised, without reaching an unmanageable degree of heat. Liquid carbonic acid, which, under a single atmosphere of pressure, boils at 112° below the zero of our thermometer, may thus be raised to a temperature having the same relation to its boiling-point that a red heat has to that of water, and may be still confined within a glass vessel, provided the walls of the vessel are sufficiently thick to bear the strain of the elastic outstriving pressure. In spite of its brittleness, glass is capable of bearing an enormous strain *steadily applied*, as may be proved by trying to break even a mere thread of glass by direct pull.

Dr. Andrews thus treated carbonic acid, and the experiment, as I have witnessed its repetition, is very curious. A liquid occupies the lower part of a very strong glass tube, which appears empty above. But this apparent void is occupied by invisible carbonic acid gas, evolved by the previous boiling of the liquid carbonic acid below. We start at a low temperature—say 40° Fahr. Then the temperature is raised; the liquid boils until it has given off sufficient gas or vapour to exert the full expansive pressure or tension due to that temperature. This pressure stops the boiling, and again the surface of the liquid is becalmed. This is continued until we approach nearly to 88° Fahr., when the surface of the liquid loses some of its sharp outline. Then 88° is reached, and the boundary between liquid and gas vanishes; liquid and gas have blended into one mysterious intermediate fluid; an indefinite fluctuating something is there filling the whole of the tube—an etherealised liquid or a visible gas. Hold a red-hot poker between your eye and the light; you will see an upflowing wavy movement of what appears like liquid air. The appearance of the hybrid fluid in the tube resembles this, but is sensibly denser, and evidently stands between the liquid and gaseous states of matter, as pitch or treacle stands between solid and liquid.

The temperature at which this occurs has been named by Dr. Andrews the "*critical temperature;*" here the gaseous and liquid states are "*continuous*," and it is probable that all other substances capable of existing in both states have their own particular critical temperatures.

Having thus stated the facts in popular outline, I shall conclude the subject in my next paper by indulging in some speculations of my own on the philosophy of these general facts or natural laws, and on some of their possible consequences.

PERSPECTIVE ILLUSIONS.

By H. J. Slack, F.G.S., &c.

Some persons have very little perception of perspective, and whole nations, as the Chinese, whether or not impressed by its effects, are not offended by drawings made in defiance of its rules. If experiments are made with English folk of different ages and degrees of education, a large proportion will be found obtuse in perceiving, and inaccurate in observing, the optical aspects of buildings, pieces of furniture, crockery, &c., as seen from different positions. On the other hand, the more artistically-cultivated or more naturally-endowed persons are extremely sensitive to all such effects; and, in some cases, most easily deceived. Etchings in simple lines, without shading, such as Flaxman's illustrations of Dante, show how easily the eye is led, by slight thickenings, curves, and angular approximations, to conceptions of distance or superposition, and it is probable that the illusions thus produced are strongest in the sharpest observers. Persons are sometimes met with who see nothing of the kind, and to whom all drawings look flat. Illusion figures are most deceptive when they give no hint of the real facts. Thus, your Fig. 8, p. 70, deceives an observer who finds no illusion in Fig. 9 on the opposite page. I see the illusions in both, but strongest in Fig. 8. Fig. 9 is more like a mosaic pavement, in regard to which the knowledge that the surface is flat makes it seem so, in spite of the lines. In looking at pictures and engravings, the mind willingly accepts their perspective indications. In Fig. 8 the illusion is produced by the series of diminishing arcs from the edge of the outer circle inwards. These correspond sufficiently with the representation in perspective of a series of curved objects of the same size, but looking smaller and smaller as the distance increases. This makes the centre of the line AB appear to bend inwards.

Compound vibration curves described in fine lines on paper are very deceptive. Mr. Washington Teasdale, who has great skill in these matters, has supplied me with many beautiful illustrations of this fact. He has also enabled me to make a variety of experiments with similar patterns, minutely ruled on glass as microscopic objects. The optical illusions are strongest when the lines correspond with those used in perspective drawing; and high magnification (say 1,000 ×) does not dissipate the impression, if enough of the pattern remains visible in the diminished field. When bands of lines of the same thickness cross each other, it is extremely difficult to illustrate them so as to make them look on one plane. Of two such series, either may be made to look uppermost. With large angle of operation and high powers, I believe it is quite impossible to obtain certain information of the structure of many objects, unless there are good reasons, independent of vision, for supposing one optical appearance more correspondent with fact than another.

Mr. Teasdale obliged me with a number of the compound vibration and curve slides, done in testing his apparatus, more or less imperfect. These are highly instructive. One especially, a spiral, with the initial part roughly scratched, and the subsequent parts wholly clear and fine, appears as a deep hollow under the microscope with minocular vision. A well known physiologist and microscopist, to whom it was shown, saw this perspective effect strongly when using one eye, but lost the illusion the moment both eyes were employed with the prism. This observer has both eyes better matched than usual, and trials with various persons show that the illusion is only partially dissipated when the binocular apparatus is used by persons whose eyes differ in focus, as is very common.

Reviews.

BRITISH FERNS.*

MANY find the study of ferns a difficult task, on account of the difficulty of recognising the distinguishing characteristics of each genus or class. The book before us is intended to remove this obstacle. At the beginning, a careful explanation is given of the general divisions of flowerless plants; of the different parts of ferns; of terms applicable to the shapes, etc., of their fronds; the grouping of ferns; and the genera of British ferns. Tables are given of each species separately, and according to the following plan, the technical terms used in which will be found fully explained in Miss Ridley's work.

1. First the scientific title is given, with the abridged or full name of the botanist who first gave this name, and after that the common name by which the fern is known.
2. In each instance the special points of the genus are indicated under the heading of generic characters.
3. Under distinctive specific characters are noted the special peculiarities and the points of difference by which the fern is known from all others of the genus to which it belongs.
4. Whether the fern has a rhizome or caudex.
5. The characters of the stipes and rachis.
6. The scales of the stipes.
7. The shape of the frond; its average size, the length and width being ascertained.
8. The texture of the frond, some ferns being hard and leathery, others very soft, thin, and transparent.
9. Whether deciduous (that is, shedding its fronds in autumn) or not.
10. The arrangement of the veins in the fronds.
11. Position of the receptacle.
12. Sori, shape of, and usual quantity.
13. Whether the sori are dorsal or marginal.
14. If there is an indusium or involucre, or neither; when present, shape and character of such.
15. Locality, or where found.
16. Lastly come the general remarks to each species.

At the end of the book is an index containing a list of technical terms used in the book, by which easy reference can be made to the page on which the meaning has been given. With such a help as this little pocket guide, many difficulties will be removed, and the study of ferns will be made both easy and interesting.

* "A Pocket Guide to British Ferns," by Marian S. Ridley. (London: D. Bogue.)

HIEROGLYPHIC INSCRIPTIONS OF THE SAKARA PYRAMIDS.

By a Member of the Society of Biblical Archæology.

THE recent magnificent discovery of royal mummies, papyri, and numerous other relics at Egyptian Thebes has been so surprising, as to throw unduly into the shade the almost equally important results attending the opening of pyramids at Sakara earlier in the year; doubly interesting, because their chambers and passages, contrary to those of pyramids previously explored, were found, except where mutilated, to be covered with hieroglyphics. The literature thus so marvellously preserved was soon seen to be of a very similar character to the most archaic chapters of the "Ritual," or "Book of the Dead," and of great mythological and theological value. They are, however, owing to their extreme antiquity, notwithstanding the distinctness and delicacy of the carved and painted pictorial hieroglyphics (which are equal to that of almost any other epoch), very difficult to translate, because many signs and ideas are used, or expressed differently from those of more recent texts with which scholars have hitherto chiefly been acquainted. Some of the inscriptions have now been published by Lepsius, in his "Egyptian Review," and a tentative translation of a portion of them made by Brusegh Bey.

The most valuable fact brought to light by a careful study of this decipherment of the texts is the decisive manner in which it enforces an immediate reconsideration of what were previously considered as axioms in Egyptology. Most, if not all, the myths and legends supposed to have been engrafted into the Egyptian religion at long subsequent dates are here found to be fully rife; not so much because some either appear in part or even amplified in the texts themselves, especially the Myth of Nut, but on account of the numerous allusions to, and short quotations from, legends which, to the initiated, at once called up, by a long train of associations, the story thus frequently only indicated by a sort of *double entendre*.

The whole history of Osiris, with his consequent belief in one supreme ever-existent deity, and account of a future judgment, with subsequent states of reward or punishment, is in full sway. It is distinctly and emphatically asserted of one deceased King, Pepi, that he is not really dead or annihilated, but that he has "received his spirit," or become spiritualised in the abode of the blessed. The legend of the struggle and triumph of Horus over Set, or of good over evil, is seen fully developed, as are many others. Ra, Sele, Harmachis, Tum, Tefunt, Harpocrates, Buto—in fact, nearly all the pantheon, hitherto considered to be a gradual accumulation of centuries after by many authorities—here burst into view in full splendour, and especially even Amen has his place.

The dog-star Sirius is mentioned in respect to its chronological importance, proving a careful observance of the constellations for at least two periods of fourteen and a half centuries previous to the writing of the inscriptions, or perhaps still older copies from which they were taken, and the objects depicted in the hieroglyphs indicate fully as advanced a culture and high civilisation as that of any epoch prior to the shepherd kings. The Elysian fields of heaven, besides many other places and events described in the ritual of the dead, are prominent, showing that this Bible of Egypt had long before been compiled and reverenced.

From this short *résumé* it will be gathered that these preeminently ancient texts afford no ground for arguments based on a theory of gradual development of Egyptian religion, from gross fetichism or anthropomorphic ideas; on the contrary, the theology exemplified is more spiritual, and diverges less into pantheism and animal worship by far than that of subsequent times. What are now believed to be divine truths, whether evolved out of necessity from man's spiritual nature, or imparted by primæval revelation, are, in fact, less buried beneath a *débris* of idolatry and ignorant superstition, than in the era when Egyptian religion (because misunderstood) became a jest to the writers of Greece and Rome.

A fortunate result of these explorations is also to terminate for ever the apparently endless controversy as to the original purport of the pyramids. These newly-opened pyramids are certainly the mausoleums of the monarchs whose names, creeds, and coffins they contain, and there can be no doubt but that such was the Great Pyramid which formed the basis for the wonderful conjectures of Prof. Piazzi Smyth and his school.*

These discoveries are almost certainly but the commencement of others equally interesting, for there are in the Nile Valley quite a number of pyramids yet unopened, and it is M. Maspero's announced intention to inaugurate his accession to the post of Marriette Pasha by completely exploring every one that is known.

[* I do not accept the wonderful conjectures of the Astronomer-Royal for Scotland; but no astronomer can doubt the astronomical significance of the structural details of the Great Pyramid. Whether intended as a tomb or not, it was certainly erected by men exceedingly well skilled in astronomy and mathematics, and as certainly astronomical observations, of considerable difficulty and requiring great exactitude, were carried on during its erection. Whether the Great Pyramid was erected with so much precision in order that astronomical observations might be made from it with great exactitude, or whether astronomical observations were made from it with great exactitude in order that it might be erected with great precision, may be a moot point. I have scarcely any doubt myself that both purposes were in view, the former as the primary one. —Ed.]

Letters to the Editor.

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*** *All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.*

All Letters or Queries to the Editor which require attention in the current issue of Knowledge, *should reach the Publishing Office not later than the Saturday preceding the day of publication.*

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

Our Correspondence Columns.

TO OUR READERS.

82.—Last week we had to add two leaves, including three pages of matter, to what we had intended to be our full weekly supply for at least the first half-year, and, probably, for the first year of our issue, and yet no less than 14 pp. of matter remained over. Our readers will see, therefore, that we lie under a considerable difficulty. We wish to find room for the better part of all the correspondence which reaches us. But we cannot do so without condensing most of the letters; and even so, we run over the limits which prudence dictates to us as suitable for a journal at the low price we have adopted. On the other hand, we have much to encourage us, even in the very circumstance which occasions our difficulty. Before we reached our fifth number we were receiving more correspondence than we expected to receive after we had been a year before the public; and the amount of this correspondence really measures the kindness of the welcome extended to us. That the friends we have already made are finding for us a welcome elsewhere, we know by the rate at which our circulation grows. We have had to reprint both our first and second numbers (every line having to be recomposed); and the indications are, that large though the second edition of No. 1 was, a third will before long be required. All this is very gratifying, and it leads us to hope that, long before we had expected, we shall be able to enlarge our weekly number. It may, perhaps, be said, that there are journals larger than ours at the same price, and that therefore we might at once enlarge our issue. There may be such journals, but if there is one giving anything like the same amount of original matter on *scientific* subjects at the same price, we have yet to hear of it. With the size we already have, and the amount of original (and costly) matter we give our readers, a very large sale is required to enable us permanently to enlarge each number. We shall not wait till such a sale is attained, but shall increase our weekly number in anticipation—probably long before. But our readers will understand that their co-operation is still needed. In the first place, each reader who contributes to our correspondence columns is earnestly requested to limit his remarks to subjects suitable to Knowledge, and to condense them to the utmost of his power. Then, each who approves our purpose can greatly help by making us known to others, noting to them in turn that they also, if content with us, can help to extend our sphere. We may point to the last four weeks' issue in evidence that we are anxious to meet our readers' wants by enlarging our numbers—without, however, engaging that such enlargement shall be permanent yet awhile. We may, indeed, note this, that if we had so kept down our correspondence and original matter as not to exceed what was given in our first number, our present issue, though the growth of only five weeks, would be remunerative. We have preferred, however, to increase our number, even at the risk of loss, and we shall continue to follow that policy so long as we receive such encouragement as heretofore from our readers and correspondents.

The Editor and Proprietors of "Knowledge."

"J. P. S." suggests that we should do as other papers dealing with popular science have done, in having reports of the meetings of scientific societies. We would gladly, if we could afford the space to do it properly. But let our friendly adviser consider how the matter stands. We already find it difficult to find space for what is absolutely essential as regards the purpose set before us. Now, the reports of scientific societies in the *Athenæum* for Nov. 26, which devotes only a section to science, and therefore condenses such reports as much as possible, occupy a page and a third, which, in the larger type of our correspondence columns, would be about a page and three-quarters. Now, let us ask how the readers for whom we specially cater, would care for so much space being occupied with reports like those in the *Athenæum*, excellent as those reports are in their proper place there. For instance, under the head "Chemical Society," we find this. "The following papers were read:—'Aluminium Alcohols, Part II. Their Products of Decomposition by Heat,' by Messrs. J. H. Gladstone and A. Tribe,—'On the Chemical Action of Decomposing Vegetable Matter on the Rock-forming Sediment of the Carboniferous Period,' by Mr. E. Wethered,—'On a and β Amylam,' by Mr. C. O'Sullivan,—'On the Action of Oxides on Salts, Part IV. Potassic Chlorate and Ferric Oxide,' by Messrs. E. J. Mills and G. Donald,—and 'On the Steeping of Barley,' by Messrs. E. J. Mills and J. Pettigrew."

Or take the following report of the Royal Society (Mr. W. Spottiswoode, President, in the chair):—"The following papers were read: 'Preliminary Note on the Photographic Spectrum of Comet *b*, 1881,' by Mr. W. Huggins,—'Note on the Reversal of the Spectrum of Cyanogen,' by Profs. Liveing and Dewar,—'The Sum of the Series of the Reciprocals of the Prime Numbers and of their Powers,' by Mr. C. W. Merrifield,—'Further Note on the Minute Anatomy of the Thymus,' by Mr. H. Watney,—'On the Production of Transient Electric Currents in Iron and Steel Conductors,' by Prof. Ewing,—'Experimental Researches on the Propagation of Heat by Conduction in Bone, Brain-tissue, and Skin,' by Dr. Lombard,—'On Allotropic or Active Nitrogen, and on the Complete Synthesis of Ammonia,' by Mr. G. S. Johnson,—'On the Comparative Structure of the Brain in Rodents,' by Mr. W. B. Lewis,—'The Prehensores of Male Butterflies of the Genera Papilio and Ornithoptera,' by Mr. P. H. Gosse,—'On the Propagation of Inhibitory Excitations in the Medulla Oblongata,' by Dr. H. Kronecker and Mr. S. Meltzer,—'Researches on Chemical Equivalence,' Parts IV. and V., by Mr. E. J. Mills,—and 'On the Refraction of Plane Polarised Light at the Surface of a Uniaxal Crystal,' by Mr. R. T. Glazebrook."

Or let us turn to a paper more especially devoted to science—*Nature*. Here the reports, as might be expected, are fuller; but they would scarcely correspond with our special purposes. Here is the report of the Entomological Society:—

H. T. Stainton, F.R.S., President, in the chair.—Exhibitions: An aberration of *Urapteryx sambucaria*, L., Mr. C. O. Waterhouse.—A new species of *Antheræa* from the Gold Coast; and some microscopic preparations of the saws, &c., of various *Hymenoptera*, prepared by Mr. P. Cameron of Glasgow, Mr. W. F. Kirby.—Pieces of honeycomb constructed on a bare wall, without any protection; and specimens and figures of new varities of *Armadillium vulgare*, L., and *Porcellio scaber*, Latr. Rev. A. E. Eaton.—A specimen of *Lycæna icarus*, Rott., var. *Icarinus*, Scriba, Dr. H. G. Lang.—An undescribed species of *Cicadidæ* from Borneo, with unusually developed opercula, Mr. W. L. Distant.—A female specimen of *Dufourea Minuta*, Lep., Mr. T. R. Billaps.—A specimen of *Scleroderma domestica*, Westw.; the larva was found parasitic on that of a Longicorn beetle in a pine-tree at Lyons; and some *Diptera* which attack figs in Turkey and Egypt, Sir S. S. Saunders (this led to an interesting discussion on fig-parasites and caprification).—Some remarkable tubes formed by Lepidopterous larvæ at Aden; and a specimen of *Cerura vinula*, L., which it was thought at first might belong to *C. erminea*, Esp., the President.—Papers read: Descriptions of new genera and species of Heterocerous *Lepidoptera* from Japan (concluded), by Mr. A. G. Butler; and a memoir on the various Dipterous insects (*Muscidæ* and *Tipulidæ*) destructive to cereals in Britain, by Professor Westwood."

The report of the Geological Society runs thus:—"R. Etheridge, F.R.S., President, in the chair. The following communications were read:—'On the genus *Stoliczkaria*, Dunc., and its distinctness from *Parkeria*, Carp. and Brady,' by Prof. P. Martin Duncan, M.B. Lond., F.R.S., F.G.S., Pres. R.M.S. 'On the elasticity and strength-constants of Japanese rocks,' by Thomas Gray, B.Sc., F.R.S.E., and

John Milne, F.G.S. 'The glacial deposits of West Cumberland,' by J. D. Kendall, C.E., F.C.S.'"

It would be the easiest thing in the world to fill four or five pages weekly with such reports. Indeed, it is easier to use them than to decline them; but however excellent such things may be in their place, we must point out that they would not at all correspond with the promises held forth in our prospectus. When we increase our space, it will be to find room for matter more likely to be widely and generally interesting. Of course, the papers read before our learned societies are full of interest for experts in the respective branches of science to which they belong; and their subject matter may be made very interesting by suitable treatment; but their mere names, or abstracts of their contents, could have no interest for the great majority of our readers. Even our scientific readers would only be interested, each, perhaps, in one or two out of a dozen such titles or abstracts. Richard A. Proctor.

PROFESSOR CLERK MAXWELL AND THE REVERSIBILITY OF THE GRAMME MACHINE.

[83]—Listening to Professor Sylvanus Thompson's paper on "Storage of Electricity," at the Society of Arts, last Wednesday, I heard to my surprise the following story (as an introduction to his subject):—"Not many months before he was seized with the mortal illness which robbed us too soon of his rare and unique genius, Professor Clerk Maxwell was asked by a distinguished living man of science what was the greatest scientific discovery of the last twenty-five years. His reply was, 'That the Gramme machine is reversible.'"

Now I fully subscribe to Professor Thompson's statement about the loss we have sustained by Professor Maxwell's death, of his rare and unique genius; but the latter part of the above citation I listened to as to a story told of great men; one of those stories which often have their origin in insignificant incidents or expressions, and are used for the purpose of raising the subject of which the great man speaks in the estimation of the listeners, or are intended to give a certain halo to his fame, and show his abstracted, simple mind. A story of this latter category I heard told of Arago. To enable a favourite cat to enter his study, he had a hole cut in his door; and when this cat had a kitten, he had a small hole cut at the side of the big one, to give facility to the kitten also to enter. We may look upon such stories as the spice with which sometimes scientific subjects are dished up; and in that light I accepted the account of Professor Clerk Maxwell's opinion about the greatest discovery within the last twenty-five years.

But I was astonished to find this story printed in the paper as given in *extenso* in the *Society of Arts Journal*, and hence offered to the reader as an undeniable fact.

Professor Thompson's paper itself contains the reason, why one may doubt, that Professor Maxwell should have made such a reply seriously; and the reason is, that ever since Ritter built up his first secondary pile, or Jacoby his first electro-magnetic engine, or Pacinotti his electro-magnetic engine with the first ring armature—nay, since Newton's law, as given by Professor Thompson himself in his paper ("to every action there is an equal and contrary reaction"), was enunciated, there could be no doubt whatever that the Gramme machine was reversible. But, principally, it follows directly out of Lenz's law of magneto-electric induction, published in 1834, and could have been foretold from all practical experience made with the electro-magnetic power engine which was intended and expected, some thirty to forty years ago, to supplant the steam-engine.

We will set aside for a moment Newton's "immortal" law *per se*, and its application to the secondary battery; and take the phenomena of the electro-magnetic motor, and of magneto-electric induction alone, into consideration.

Clerk Maxwell, in his "Electricity and Magnetism," 2nd edition, section 530 (Vol. II., p. 167), gives the latter phenomenon in the following words (under the heading Magneto-Electric Induction):—

"In all cases the direction of the secondary current is such that the *mechanical action* between the two conductors is *opposite* to the *direction of motion*, being a repulsion when the wires are approaching, and an attraction when they are receding. This very important fact was established by Lenz." (Pogg. Ann XXI., 483.—1834.)

Surely no great logical powers are required to interpret this law, as, in fact, years ago it has been interpreted—viz., the same arrangement of conductors and magnets which, by motion in relation to each other will produce a current in the conductor, will produce motion when from some electric source a current is sent through the conductor.

This "reaction" was applied in some of the earlier forms of magneto-electric "induction machines," which were used for illustrating the application of electricity as a "moving power"; and Pacinotti, when describing his electric motor, in which the ring armature was for the first time applied (1860-61), stated already that, when rotating the armature, his machine could be used as a generator of electricity.

Lastly, Jacobi showed nearly fifty years ago (about 1835 or 1836) that the efficiency of electro-magnetic motors was seriously interfered with by the electric currents induced in the machine. Hence, ever since the production of electric currents by means of magneto-electric induction was understood, and since the application of electricity for the production of motion has been studied, there could not have been any doubt about the reversibility of a magneto-electric or dynamo-electric generator into an electro-magnetic motor. And this story of Professor Clerk Maxwell declaring this "discovery" as the greatest scientific discovery of the last twenty-five years loses, to say the least of it, its point.

Perhaps some of your readers can bring some further light to bear upon this question, and witnesses will be forthcoming to prove either *pro* or *con*.

I may incidentally remark here that Professor Thompson's paper was the most complete account which it is possible to give of the important question of the storage of electricity, as he has thoroughly exhausted the subject in its scientific and practical bearing, for which he deserves the thanks of every electrician and engineer interested in this latest phase of the development of the science of electricity.

Nov. 26, 1881. C. G. G

SUNDAY ART EXHIBITION.

[84]—The exhibition of works executed by students of the City School of Art, which was opened on two Sundays in December last, having proved very interesting to a large number of people at the East-end of London, we have great satisfaction in announcing that arrangements have been made for again opening the exhibition on Sundays.

The Exhibition, which is the twenty-fifth Sunday Art Exhibition opened under the auspices of the Sunday Society, will be held in the Skinner-street Hall, Bishopsgate, and will be open from three to six o'clock p.m. on Sundays, Dec. 4 and 11. Admission will be free (without ticket), and we are pleased to be able to state that, in addition to the work of the students, some valuable pictures from the collection at South Kensington Museum will be exhibited.

In order that the widest publicity may be secured for this effort to provide innocent recreation on the leisure day of the week, we ask you to insert this letter in your columns, seeing that the facilities in London for visiting collections of art are far too limited, and that the want of open museums and art galleries on Sundays is especially felt by the inhabitants of crowded districts at this season of the year, when our climate so often practically closes the parks and gardens to them.—We are, &c.,

William Rogers, M.A., *Chairman*. } City School
R. H. Hadden, B.A., *Hon. Sec.* } of Art.
Thomas Burt, M.P., *President*. } Sunday
Mark H. Judge, *Hon. Sec.* } Society.

7, Conduit-street, W., *Nov.* 30, 1881.

THE PYRAMID AND PARADOXERS.

[85]—If I were called upon to classify paradoxers, according to the good or evil effects they have had upon the community, I should give the Pyramid craze a very honourable place, for it has served to carry a knowledge of certain elementary facts connected with astronomy and geometry into dark regions where attention to such matters would probably never have been aroused by other means. There are thousands, possibly I should be nearer the truth if I said hundreds of thousands, who would never have known that the pole of the earth's axis is moving amongst the stars if it had not been for the Pyramid paradox, and the literature which has sprung up around it. There seems to be something connected with such speculations which has a fascination for a large class who would be wearied by a more cautious search after truth. We have only to notice how a statement, that three of the major planets will be in perihelion next year, and, consequently, something extraordinary may be expected to happen on the earth, goes the round of English and Colonial papers, to see that such speculations are fitted to do a sort of missionary work for science.

I wish to enquire what attitude those who would like to see the scientific spirit spread, should take with regard to such speculations. There are some who think that this tendency of human nature may be utilised to obtain money for science; and they have not thought it unworthy of them, as seekers after truth, to pose before the uninstructed, as weather prophets or alchemists. Give us money to study the sun, they say, and we will tell your fortune by sun-spots. We will show that the elements are not elementary, and, perhaps,

In this, we may then note them. I expect that there are a good many besides myself who feel that such a method of trading upon the paradoxical proclivities of our neighbours is not honest, and must not for a moment be indulged in. How, then, can we utilise the interest stirred up by such paradoxes to spread a love of science, and to bring to our neighbours all the benefits which follow from a patient search after truth?

It seems obviously unwise to ignore the paradoxers, or even unnecessarily to hurt their feelings and snub them, as some able men seem inclined to do. Paradoxing is an approach towards science, and, at all events, is better, and more worthy of being spoken of with respect, than purely selfish or unintellectual forms of enjoyment. Most of us are, no doubt, pretty frequently thrown amongst paradoxers, or with those whom we believe to be paradoxers. I would suggest that when we have made up our mind that our friend is a paradoxer, we should endeavour to put our objections to his paradox in the form of questions which raise difficulties that he has probably not yet considered. We may thus lead him on, and at the same time exercise ourselves in the art of seizing the difficulties of others.

Let us take an example. Mr. Baxendell, in your number for Nov. 18, says that he has been led to believe "that the data which formed the basis for the design of the great pyramid were the diameters and distances of the sun, earth, and moon, combined with the ratio π of the circumference of a circle to its diameter." He proceeds, let $s =$ the diameter of the sun, e = the diameter of the earth, m = the diameter of the moon. Then we have

(1). $\dfrac{se}{m} = 1{,}000{,}000\pi.$

(2). $\sqrt{s}\,\pi^{2}$ = length of one side of the base of the Pyramid.

And then follow thirty-one more such relations. I will not occupy space by repeating them. We might commence by asking whether the relationships discovered refer to the solar system at its present temperature, or to the temperature which its various parts had at the time when the Pyramid was designed. Let us suppose our imaginary pyramid paradoxer to answer that the relationships must be true for the temperatures and magnitudes of the designer's epoch. We might then proceed to inquire whether the probable errors in the determinations of the quantities made use of in the equations are sufficiently small to enable one to determine whether there has been any change of magnitude in the various parts of the solar system since the Pyramid epoch. The answer to this question will give an excellent opportunity for inquiring into the amount of the probable errors in determining the height of the Pyramid—the length of its base—and other pyramid magnitudes, compared with the probable errors in determining astronomical magnitudes. If we succeed in convincing him that the moon's distance can be determined with greater accuracy than the height of the great Pyramid, the rest is easy. It follows that there can be no advantage in attempting to determine astronomical quantities from pyramid measures.

But let us suppose that our companion is not sufficiently acquainted with astronomical methods to understand such reasoning. We can point out to him that the relationships he has found are not homogeneous. His first equation is equivalent to the statement that a length multiplied by a length is equal to a length multiplied by a number. Such a relation between quantities can only be true when a particular unit is used; and we can point out that all relationships in nature can be expressed so as to be true, whatever unit is used. We may then show him that his different equations involve different units; for example, his second equation involves the use of Pyramid miles on the one side, and Pyramid inches on the other.

In such a conversation, do not on any account refer to authority or endeavour to crush your opponent with facts that he cannot verify for himself, but patiently plod on, using your ingenuity to utilise the interest in the study of nature which has been begotten in his mind by the paradox, and, if possible, stir up his enthusiasm for the further study of such things, and endeavour to leave him with a sense of your perfect fairness and an appreciation of the scientific method which you strive to apply in satisfying yourself as to truth,

A. C. Ranyard.

Nov. 28, 1881.

PYRAMID MEASURES.

[86]—I do not profess to be master of the whole theory of probabilities, and it is perhaps owing to this that I have failed in my attempts to apply it in proving that the coincidences given in my paper are merely accidental. So far as I understand the theory, and have been able to apply it, the results indicate in a marked manner that the coincidences are *not* accidental, and, therefore, till the contrary can be shown, I think I am justified in my conclusion "that so far, at least, as the values of the distances and diameters of the sun, earth, and moon are concerned, modern science has made no real advance upon the science known to the builder of the Great Pyramid four thousand years ago."

The subject is one of considerable interest and importance, and, whether I am right or wrong in my conclusions, I shall be glad to see it fully and impartially discussed in Knowledge, and leave it to be decided from the results of such discussion whether it is desirable to enter into the questions of inspiration and prophecy. At present the general feeling appears to be decidedly against the coincidence theory, and the very few who object to the inspiration theory argue that in remote ages a high degree of civilisation existed, and that knowledge had been acquired which, when men afterwards relapsed into a state of superstition and barbarism was almost entirely lost. But on this theory the question again rises, In what way did men in those early times acquire this knowledge, and what evidence have we of its existence except that shown in the Great Pyramid?

I am, dear sir, yours faithfully,

Joseph Baxendell.

[87]—I venture to offer an explanation of the remarkable numerical relations between different parts of the Great Pyramid, pointed out by Mr. Baxendell in your number of Nov. 18, more probable, I think, than the supposition that its ancient builders had anticipated the discoveries of modern astronomy as to the distance of the sun and moon and the size and shape of the earth, but buried their marvellous attainments in a mass of stone-work, with no explanation, till, thousands of years afterwards, they were brought to light by the indefatigable zeal and scientific skill of Professor Piazzi Smyth.

The measures are, I believe, founded on astronomical observations, but observations of the most primitive character—the length of the year, the length of the month, the length of the week, and, what appears to have symbolised the course of time, the number 60, which meets us in the 360° of the zodiac, the 60 hours in India, and, I believe, also in Egypt, of the day, 60 minutes of the hour, 60 seconds of the minute, &c.

It seems to me impossible to reason with any assurance on this matter unless we knew more certainly than we do what their calculations were. We cannot ascertain this positively from the actual measurements, unless we assume that the old builders measured as accurately as Professor Piazzi Smyth, which is very improbable. It is quite possible that if we had the plans of the architect before us, we should find relations simpler than those stated above.

There is no necessity for attributing to the Egyptian builders any preternatural knowledge of astronomical facts in order to account for the peculiar relations subsisting between the measures of the Great Pyramid.—Yours, &c., G. Vansittart Neale.

[We have been obliged to omit the numerical relations indicated by Mr. Neale. They correspond with the measurements as well as others which have been devised. One could explain the proportions of the Pyramid in a dozen different ways.—Ed.]

POSSIBLE DAILY VARIATION OF PENDULUM.

[88]—There is a question that I have long thought might be worth trying respecting the earth's motion. In round numbers, say, the centre of the earth travels round the sun at the rate of 1,000,000 miles per day, or say 40,000 miles per hour; and the earth revolves at the rate of 1,000 miles per hour. One side of the earth will be going at the rate of 40,000 miles + 1,000; the other side 40,000 miles − 1,000; difference of velocity to be imparted in 12 hours, 2,000 miles per hour.

If a heavy and steady pendulum of great length were properly suspended and protected, some daily motion might be discovered, due to this varying velocity being communicated through the suspending-rod. The direction of revolution round the sun and the earth's revolution are both known. Perhaps some indication of some other motion might be found. A long pendulum would be essential. This, I apprehend, would not be so much affected by minute, rapid tremors, but would more clearly show the action of long duration. My own experience has been only with transits and other instruments in engineering work.

No. 1 of Knowledge has been sent to me to-day, as a sample, I presume. I shall take it. Receiving this has led me to trouble you with these remarks.—I remain, yours truly,

Henry Carr.

DARWIN'S THEORY OF EVOLUTION

(*Abstract, much condensed.*)

[89]—Sir John Lubbock, in his able and luminous address delivered before the British Association at York, told us that we ought to believe "that the horse and the ass, the sheep and the cow proceeded from common ancestors." I cannot find a title of

satisfactory evidence to prove the conclusion advocated by Sir John Lubbock. Let us take a single instance of Mr. Darwin's "method" of treating facts and inferences. He tells us that the rattle of the rattlesnake was probaby evolved by the desire of the creature "to frighten its enemies." [Darwin says nothing about the rattle-snake's *desire*.—ED.] Now the "enemies" of any animal seek it out in order to attack and destroy it; and, therefore, the rattle, so far from being a source of alarm, would act rather as an invitation to the snake's "enemies" to pursue and overcome it. [How if they dislike the noise?—ED.] Further, if the desire to frighten an enemy is a creative cause of such an organ as a rattle, why does not the same desire develop the same product in other creatures? Asimilar cause ought to produce a similar effect under similar conditions.

Sir John also alluded to those known transformations which occur in a short time, and which have been observed in many objects of creation; and he cited them as collateral testimony of those greater and more profound transmatations which have been supposed to be wrought in higher organisations during remote ages. I submit that this argument is altogether illusory. Those obvious minor transformations, to which Sir John referred, take a certain course—pursue a certain round—in obedience to regular processes and laws of being, and then terminate their career under known conditions.

The attention of the audience was also invited to the circumstance that the stripes of the tiger correspond with the long grass in which he makes his habitat; and that the spots of the leopard resemble the speckled appearance of the light falling through the leaves of trees. Now, if this eminent teacher really means that we should believe that in these instances the long grass and the specks of light are in the remotest degree the *causes* of the stripes of the tiger and the spots of the leopard, then I must say that the impression he wished to create is the most astounding and intolerable tax upon our credulity ever levied by the greatest scientific fanatic.

London, *Nov.* 19, 1881. NEWTON CROSLAND.

[Mr. Crosland entirely misapprehends the Darwinian theory and Sir J. Lubbock's remarks on it. We feel justified in excluding objections based on mere misinterpretation of the theory attacked; though inquiries suggested by such misinterpretations will always find a place here.—ED.]

PROBLEMS GEOMETRICALLY INSOLUBLE.

[90]—Would the geometrical solution of one or two problems, hitherto unsolved by geometry, be suitable for the pages of KNOWLEDGE? One is to determine the centre of gravity of a semicircle, and a computation of its distance from the centre of the circle by trigonometry. This, of course, is analogous to what is called squaring the circle.

Another is to determine the diameter of a sphere equal in volume to a given parallelopipedon. I should esteem it a favour if you would let me know what you think about them.—Yours, &c.,

J. G. MOORE.

[The trigonometrical computation of the centre of gravity of a circular arc is well-known. If our correspondent knows of any simpler form of it, we shall, of course, be glad to have his demonstration. If, as a preliminary to either of his problems, he proposes to "square the circle" geometrically, we shall be content to wait awhile. If he *has* squared the circle geometrically, we shall never succeed in showing "where the error comes in."—ED.]

A REMARKABLE RAINBOW.—LOGIC *versus* MATHEMATICS.

[91]—About three o'clock in the afternoon, one day last week, I observed a very bright rainbow, accompanied by its secondary external arc. To my great surprise, I noticed that the primary bow consisted of a triple series of colours, the red of the second band being in close contact with the violet of the first, and so with the third. The colours of the third band were very faint, and it was only distinctly visible for a short time at the summit of the arc; but the second band was visible over almost the whole length of the bow for three or four minutes. I should be glad to know whether this phenomenon has been noticed before, and how it may be explained.

I am rather sorry to see that, notwithstanding the warning of "F.R.A.S.," in No. 1, your admirable paper is being seized upon by the crotchet-mongers to air their remarkable notions, and I trust you will not think that I wish to dispute accepted and infallible laws of mathematics or of logic because I send you a paradox in which they appear to confute one another. My paradox is as follows:—

For every whole number there is a square, which is also a whole number. No two whole numbers have the same square.

Therefore there are as many whole numbers which are squares as there are whole numbers.

But there are many whole numbers which are not squares to other whole numbers.

It follows that there are whole numbers which are not whole numbers.

I prefer, however, to infer that the series of numbers being infinite, and the series of their squares being therefore also infinite, the latter infinity includes the former. I should like, however, to see a more satisfactory explanation. Faithfully yours, THETA.

[We do not see how there can be a more satisfactory explanation than the one "Theta" has himself supplied. If we consider any corresponding parts of the two series, 1, 2, 3, 4, &c., 1, 4, 9, 16, &c.; we see that, taking them together towards infinity, the latter will run on to a higher infinity, as it were, the highest number in the latter series being always the square of the highest number in the former. Or, algebraically:—

$$\frac{1^2+2^2+3^2+\ \ldots\ +n^2}{1+2+3+\ \ldots\ldots+n}=\frac{\frac{1}{6}n(n+1)(2n+1)}{\frac{1}{2}n(+1)}=\frac{2n+1}{3}$$

or is infinite when n is infinite.—ED.]

STONE ON WHEELS.

[92]—The results obtained by "Queensland" and his mathematical friend (Query 28, p. 80), with regard to the stone rolled on wheels, probably differ through their not understanding each other. If "Queensland" means the wheels to be supported by and revolve on fixed axles, then the stone will move, as he supposes, through a distance of 75 in. If, however, he intends the wheels to rest on the ground, they will themselves move along a distance of 75 in. for each revolution, carrying the stone with them, and at the same time they project the stone forward (with regard to their own position) an equal distance; it will, therefore, move a total distance of 75 in. + 75 in. = 150 in. G. M.

POLARITY *versus* GRAVITATION (*Abstract*).

[93]—I AM too old a stager to take umbrage at any usage, however rough, which I may experience in the arena of debate; when, therefore, you tell your readers that I am "a paradoxer," who hardly knows what he is about, I accept the imputation in the parliamentary and controversial sense in which it is meant. I presume it is your mode of saying that you differ from me in opinion.

My ideas must naturally suffer some loss of cogency by the necessity which exists of compressing their exposition within the space which you have kindly allotted to me.

Permit me to submit a few words of reply to your remarks on my letters. With regard to my objections to the Newtonian theory of the tides, as you merely content yourself with reiterating that theory, and asserting its correctness, I can, of course, say nothing more on this subject. Discussion becomes profitless when one disputant sets up what the other knocks down.

Touching my criticism on the centrifugal and centripetal forces as regulators of the motions of the universe, I beg leave to say that "the ill-informed writers" who used the term "centrifugal force" in the sense which I condemned are Joyce, in "Scientific Dialogues;" Milner, in "The Gallery of Nature;" Ferguson, in his "Lectures edited by Brewster;" Dr. Lardner, in his "Astronomy;" and "Keith on the Globes"—all well-known expounders of the Newtonian system—and Sir Isaac Newton himself. When you say that "centrifugal force is only another way of viewing the centripetal force," I know what you mean, but I fancy that the general reader will require further explanation. The revolution of a planet round the sun is supposed to be effected by the attraction of gravitation or centripetal force of the sun drawing the planet out of the straight line on which it was first impelled by its Maker.

In one part of its orbit the sun draws the planet nearer to itself, and thereby accelerates its speed. This increase of speed is supposed to generate "a centrifugal force" which has a repellent effect, and thus sends the planet off again on its proper course. Now, here comes in the ticklish part of this theory. When once the attraction of gravitation overcomes a rival force, nothing can stay its career of conquest, except the intervention of a third power of equal potency and independent jurisdiction. As the accelerated speed above-mentioned has no such independent origin, but proceeds directly from the centripetal force which draws the planet towards the sun, the planet cannot, by any struggle of a centrifugal force, escape from the catastrophe of ultimately being precipitated upon the face of its ruler, and there terminating its blundering career. Fortunately for us, the planet knows better, and obeys the law of polarity, not that of gravitation. I submit that my theory of polarity—attraction and repulsion—gets rid of the difficulty here so patent, and enables us to arrive at a sounder idea of the laws of revolution.

I now leave my ideas to their fate. If they are good for any-

thing, they will flourish; if they are worthless, they will die. I should consider them of not much value if I thought they would generally be accepted without a hard battle.

It may perhaps be as well to inform the general reader that my views do not in any way affect the working of those mathematical problems which are based upon the facts and phenomena of Nature.

Newton Crosland.

THE FAURE ACCUMULATOR.

[94]—As one who has read and recommended several friends to get Knowledge, might I ask a favour?

Much mention has lately been made of the "Faure Accumulator," and the benefits likely to accrue by this addition to electrical appliances, but I, like a good many more, have no very clear or precise idea as to what this invention is.

Would you therefore give in a future (and let me hope near) number of Knowledge, an account and description of it?

A Scientific Smatterer.

FAURE ACCUMULATOR. GRAVITY.

[95]—Would you kindly furnish me with a short account of the construction and mode of action of the Faure Accumulator? also, if not asking too much, the proof, or where it is to be found, of your statement concerning gravity, on page 59, No. 3, of Knowledge?

G. T.

PRACTICAL WORK WITH THE TELESCOPE.

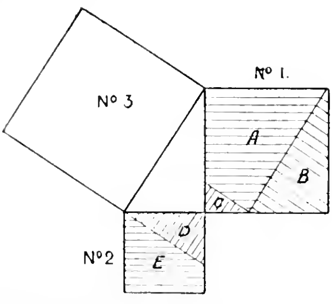
[96]—In reply to your correspondent "G. M.," letter 39, I would suggest for his use the Rev. T. W. Webb's "Celestial Objects for Common Telescopes," new edition, and R. A. Proctor's "Larger Star Atlas," 4th edition; and that he might do very useful work in carefully observing and recording the colours of stars, and the changes of brightness of some of the variables.

With best wishes for the success of Knowledge, I am, dear Sir, yours faithfully,

Joseph Baxendell.

THREE-SQUARE PUZZLE.

[97]—The enclosed has never been published in any book or periodical, but you are welcome to insert it in your paper, Knowledge, if you think well to do so. If a piece of cardboard is cut into the five pieces, A B C D and E, it is not easy to form the third large square with them. This makes a good puzzle for Christmas.



Arrange the five pieces A B C D E, which compose the two small squares Nos. 1 and 2 into a single square, No. 3. This, when done, is a mechanical proof of the above proposition.—Yours &c.,

Alf. A. Langley,
Engineer-in-Chief, Great Eastern Railway Co.

REPORTS OF SOCIETIES.

[98] Truly, as the "announcement" of Knowledge was originally gratifying to those who seek it, and equally as truly as the publication itself proves so, still, you will not wish the compliment of having got "one subscriber" to be often repeated to you. In my humble case I ordered three on "spec" of No. 1 before publication, and at once posted one off to the Australian up-country—the latitude somewhere where you did see what "Parallax" says not.

And after saying this, may I find a little fault? I will venture. It is in the omission of any chronicle of the proceedings of the learned societies, both home and foreign. For instance, the Meteorological met on the 16th, and discoursed on the big storm of Oct. 14. Now, sir, this feature is a great desideratum, and, therefore, let me urge it strongly; and also that the reports, however brief, be systematic and real. There surely would have been something most interesting to report concerning Professor Thompson on the "Storage of Electricity." And even the mental science societies should not be left out in the cold. The "Aristotelian," for instance, could tell the world briefly of its goings on in the "knowledge" of its mind-work. Yours, &c.,

J. F. S.

AQUEOUS VAPOUR.

[99]—In letter 20, on page 56 of your welcome paper, G. F. P. Dyer makes a statement about aqueous vapour, of which I venture to question the accuracy. He says "that aqueous vapour has the power to absorb rays of heat coming from the *earth*, but is incompetent to absorb rays from the *sun*." Now, in "Heat as a Mode of Motion," Professor Tyndall states (paragraph 684) that experiment proves that the aqueous vapour of the atmosphere absorbs about four-tenths of the heat radiated from the sun towards the earth. Has subsequent experiment disproved the Professor's conclusion, or has Mr. Dyer made a mistake? I think in this case Dr. Tyndall must be right.

H. P.

[H. P. will find, on more careful reading of paragraph 684, that Dr. Tyndall does not say exactly what H. P. has stated above. It is, however, true that aqueous vapour absorbs a portion of the sun's heat-rays, but in much smaller proportion than it absorbs the obscure heat-rays from the earth.—Ed.]

FLOWERS OF THE SKY.

[100]—May I take the liberty of calling your attention to certain passages in your work, "Flowers of the Sky?"

In page 2, the velocity of light, "at a rate exceeding more than ten million times the velocity of the swiftest express train."

In page 15, "that light carrying its message at a rate exceeding six thousand times the velocity of the swiftest express train, would be utterly unable to give a true account of the position and movements of the celestial bodies."

Again, in Figs. 17 and 18, the remarks that apply to 17 belong to 18, and *vice-versâ*.—Yours, &c.,

Wilhelmina K.

[I have not the book by me, but, unless I mistake the passage at page 15, it does not indicate what the velocity of light is, but a velocity which, though enormous, would utterly fail to do what light does. Figs. 17 and 18 got interchanged somehow after the book had been passed for press.—Ed.]

INTONATION IN SYNAGOGUES.

[101]—In answer to your correspondent "G. P." The accents in the Hebrew Bible supply the place of musical signs. Warschawski, in his "Progressive Hebrew Course and Music of the Bible," (London: Longmans, 1870), gives a series of equivalents for each of these accents in modern musical notation. For the accents themselves, see any large Hebrew Grammar. With regard to what may be called a cognate matter, the chanting of the Koran, Lane, in his "Modern Egyptians," gives the musical notes which would express the mode in which the first chapter is chanted. There are, of course, full treatises in Hebrew and Arabic on these subjects, from which deductions could be drawn: this, however, would require much patient research.

W. A. S.

A NEW COMPARISON OF POISONS.

[102]—In your last number (No. 4) you give an abstract from the *Times*, quoting the result of a number of experiments of the action of poisons in solution upon fish. Is there not some mistake in relation to the poisonous properties of lithium and barium. The paragraph runs thus:—Lithium (atomic weight 7, not 17), with an atomic weight only the twentieth of that of barium, is three times as poisonous. Now, it is generally known among chemists that barium (especially the chloride) is an exceedingly poisonous metal (or salt), whereas lithium, even in comparatively large quantities, is not poisonous, the citrate, for instance, being often prescribed for gout.

I think some mistake must have been made in copying the original, which, of course, is no omission on your part, it being merely an abstract. Wishing your valuable journal all success.—Yours, &c.,

Technical Chemist.

[The "copy" was the paragraph itself. Thanks for pointing out the inaccuracy. Newspaper science needs such correction, as we know from articles and paragraphs on subjects more especially our own than technical chemistry.—Ed.]

EVOLUTION—STAR-MAPS—BRITISH MUSEUM.

[103]—While defending, to the best of my ability, the theory of evolution, I have been asked the following question, which I have been unable to answer: "If man be an 'evolved' being, why has he not improved?"

I should like to be informed whether—(1) Since man has existed has the process of evolution effected any change in him? (2) If so, what change? (3) If not, why not?

With reference to your star-maps, you say in No. 4 of KNOWLEDGE that the times given in Nos. 1, 2, and 3 are one hour out, but that the error is corrected in No. 4. Yet taking the same dates, the times in No. 4 agree with the others. I would be glad if you would explain this.*

Could you, or any of your readers, inform me on what conditions and by what means admission can be gained to the British Museum library and reading-room?† G. M.

PROTECTING HOUSES FROM LIGHTNING.

[104]—I wish to protect a square house which has nine chimneys, all of equal height, from lightning, by means of two iron rods 3 ft. above two of the *centre* chimneys, connected with the iron water-pipe which supplies the cisterns from the main, by a galvanised iron wire conductor. Will you kindly tell me if this protection will be sufficient, and whether I have chosen the right chimneys. I prefer using the *centre* ones, as the conductors will be less conspicuous than if they were placed on chimneys *near the corners* of the house, the roof being nearly hidden from view by a parapet. I am delighted with KNOWLEDGE, but I wish you would print the names of the stars of the first and second magnitude on the map. B.

A QUESTION OF GRAMMAR.

[105]—Is "Nameless" (query 24) correct in writing "*would* the editor," &c.? Should not he have written "*will* the editor"? "Nameless's" mode of asking his question, I know, is frequently adopted, and my eye just now lights upon a letter in the *Guardian*, where the writer has "*might* I call your attention," &c. Would not *may* have been the proper word? Again, "I cannot but think" is an expression of every-day occurrence, and I presume right; but is "I can but think" wrong? "I can but think" seems only another way of saying "I can only think," which is also of common occurrence. If "I can but think" is not wrong, then we have both expressions, one with the negative, and the other without, meaning the same thing. We have the word "annul" and also "disannul," both meaning much the same thing, but "dis" is not used as negative, but as intensive. Would that be the case with the word "not," in the first instance? A. T. C.

THE MISSING LINK.

[106]—Mr. Wilson, in Letter 33, admits "where the links that connect man to his lower neighbours are to be found is a difficult question to answer," but until it is answered satisfactorily, I am afraid that common ignorance (?) will continue to ask the unfortunate question. One evolutionist to whom I put the question answered that he supposed the remains were all under the sea.

It seems to me that evolutionists would call every one that does not agree with their dogmatical statements an ignoramus.—I am, &c., ANOTHER IGNORAMUS.

THE MISSING LINK (*Abstract*).

[107]—I approach a criticism of Dr. Wilson's remarks (p. 74, No. 4) on the "Missing Link" with a nervousness that is only equalled by my desire to know and possess the truth respecting the circumstances surrounding the case.

Your correspondent, "Ignoramus," if not satisfied as to the foundation of evolution, has, I think, a right to ask for that which evolutionists declare exists, and is the only foundation upon which evolution is said to exist.

Professor Huxley ("Encyclopædia Britannica," 9th edition, vol. iii., p. 690) speaks thus: "The only perfectly safe foundation for the doctrine of evolution is in the historical, or rather archæological, evidence that particular organism, have arisen by the gradual modification of their predecessors (which is) furnished by fossil remains." Similarly Bastian, Darwin, Herbert Spencer, and others.

Now I ask for one species from the lowest to the highest strata in any of the geological periods that have been transformed into another?

Secondly, if the truth of evolution rests on fossil remains ("Missing Link"), where are those fossil remains? Where found? Who found by? Where are they now? Hæckel, Buchner, Darwin, &c., have failed to find or give any. Evolution's foundation does not rest on rudimentary organs, but on fossil remains.

I only plead for what evolutionists declare has led them to teach and hold the doctrine of evolution. To tell me that the fossil remains are gone down to the bottom of the sea—that where men first lived is all ocean—is to mock me in my earnest cry for more light.—Yours, &c., MITCHELL.

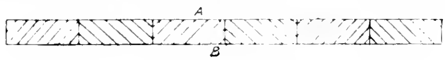
KNOWLEDGE FOR THE YOUNG.

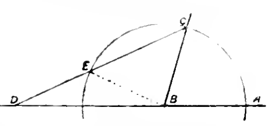
[108]—I am very pleased with KNOWLEDGE, and am endeavouring to make my children like it. Parents do not sufficiently estimate the importance of watching boys' literature. I have great difficulties before me, but if, when my boys are young men, these difficulties right themselves, I may be satisfied, but not before. The penny weekly journals started within the last few years by the Religious Tract Society and others, professed to supply an improved literature, but I fail to see it in such brutal tales of adventure as "From Powder-monkey to Admiral," &c. Boys eagerly read them, and the principles they learn are carried into practice in their every-day life. The few chapters similar to those in KNOWLEDGE are not read at all. I find it much easier to read "The Land of the Midnight Sun," with the map before me, to my boy aged nine, than to get his elder brother to look at it—he is too busy over some tale of the "to be continued in our next" class. I was really astonished to watch the interest the younger boy took in Darwin's book on worms, which I read to him, and he has since studied their habits to a surprising degree. How, then, will it be possible to rectify the mischief already done by the penny journals? Can you offer a column of inducements to boys for short essays on scientific or natural history subjects suited to certain ages (with permission to consult a parent or guardian in the composition)? It seems to me this would bridge over the difficulty I am now experiencing with the two elder of my eight children. Wishing your magazine much success. R. GILL.

[We fear it would not be possible yet to give a column to short essays by boys; but possibly a column or two especially for boys would be a useful addition.—ED.]

KNOWLEDGE—ILLUSION—TRISECTING AN ANGLE.

[109]—In addressing you a few words of congratulation on your success in producing KNOWLEDGE, I beg to suggest that there are a great many questions which may be elucidated in it (such as the method used by the American meteorologists in predicting storms, the present theory of the spectrum colours as differing from that taught in ordinary text books, papers on biology, &c.) which are *caviare* to the general public, and, therefore, exactly fitted for its pages, even at the risk of crowding out chess and whist, which are very fairly dealt with in the ordinary magazines, and hardly come within the special scope of KNOWLEDGE, as it seems to me. However, the many must be studied, of course. I personally should like to see it exclusively devoted to the dissemination of sound natural science, hence this hint. Your interesting notes on illusions have recalled to me the following, wherein the oblique lines enclosed within the parallel straight line A and B cause the latter





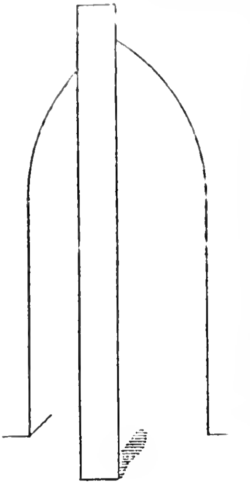
to appear zigzag instead of straight—which they really are. I send also a mechanical way of trisecting an angle, in reply to Query No. 48, thinking it cannot be done by pure geometry. It was got by me from "Wormell's Plane Geometry," and I find it quite correct for angles under 120°. ABC is the angle to be trisected. From B draw a semicircle, as in the figure. Then mark off the radius used for the semicircle on the straight edge of a piece of paper, which must then be placed so that the edge cuts through

* When we wrote as above, we supposed our correction was in time; but, as a matter of fact, the map, which, to ensure perfect blackness, was printed separately, had been for hours in the machine-room, and nearly half the impressions had been already struck off when the correction was made.

† By letter to chief Librarian, accompanied by a letter from a householder, certifying respectability, and stating objects of applicant: age must be over twenty-one.—ED.]

the point of intersection C, and so that one of the two marks on it lies in the semicircle, and the other in AB produced, as in E and D respectively. The point D thus obtained gives the angle CDA, which is the required third of ABC. The proof is interesting. For the exterior angle ABC is $= C + D$. But since BC is $=$ to BE and to ED, the angle BCE is $=$ to BEC, and this to $EBD + EDB$; that is, to twice the angle D. Therefore ABC is equal to 3 times D.—Yours, &c., James Warren.

SINGULAR ILLUSION.



[110]—A singular and practical illustration of the optical illusion, Fig. 2, of Mr. Foster's came under my notice a short time ago. The tower-arch of a church had been rebuilt, and during its construction a tall strut was set at an angle to the base, with its head fixed against the wall just above the apex of the arch. Whilst standing at the chancel steps, the arch presented a most singular appearance, and I called the contractor, who looked somewhat alarmed and astonished, and we both thought the arch must have dropped on one side. The accompanying drawing will explain what we saw. The pointed arch shows better than the semicircle the apparent illusion of the two lines belonging to different circles. F. Long.

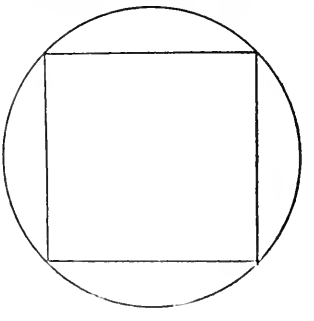
In order to make the illusion more plain, the strut is drawn out of proportion.

ILLUSIONS.

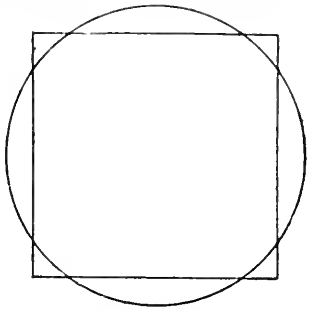
[111]—A few remarks upon the paper by Mr. Foster, on "Illusions," may be of some slight interest. The effects which angular, curved, and straight lines, in juxtaposition, have upon one another with respect to the eye are certainly very curious, and few eyes will fail to perceive the apparent distortions in the cases figured. I should like to remark, however, that in Figure 5, to my eyes, and to those of the one or two persons to whom I have presented the figure, the effect of the curves on the straight lines is just the reverse of that stated to be the case by Mr. Foster.

In Figure 6, to my eyes the straight lines AB and CD appear nearer in the middle, and EF and GH appear farther apart in the middle, thus agreeing with what is stated, and being the *reverse* of the two previous pairs of lines, which is what one would expect. I cannot by any method of gazing cause the straight lines in the last two pairs, Figure 7, to appear anything but parallel.

Below will be found a figure which shows the "flattened" appearance of a circle at the four corners of an inscribed square—a distortion of the same nature as that shown in Figure 3.



The figure beneath shows the same effect on the circle, and also gives the square the appearance of being drawn in at the middle of each side. Both these effects are shown much better



with larger figures. All these diagrams form pregnant comments on the oft-repeated adage: "Cannot I believe my own eyes?"—I am, yours, &c., Harry Grimshaw, F.C.S.

Clayton, Manchester, *Nov.* 21*st*, 1881.

CIRRUS CLOUDS.

[112]—In reply to "Anti-Guebre" (letter 2, p. 15), scientific men believe cirrus clouds to be composed of ice crystals because no other reasonable explanation is available. These clouds are found to produce certain optical effects upon light transmitted through them which can only be explained by the theory of their crystalline nature, ice being the only substance which will produce the observed phenomena under the conditions. This can be mathematically proved. In conclusion, I would ask your correspondent, Mr. G. F. P. Dyer (letter 20, p. 56), to refer to page 463-4 of Tyndall's "Heat," where it is stated that the aqueous vapour of our air does absorb a very considerable amount of direct solar heat, about four-tenths of the entire radiation; these being chiefly *dark* rays, which are the most effective as regards heating purposes.—I remain, sir, yours truly,

Birmingham. T. J. Hickin.

VIVISECTION. INJURIES TO THE BRAIN.

[113]—Most people think that of all cruel operations upon animals, that of cutting the brain is beyond all the most cruel. The first two extracts from recent numbers of the *Lancet* may modify their opinion. The third extract is from "Body and Mind," by Henry Maudsley, M.D., F.R.C.P., &c., &c.

G. A., aged 15 years, met with an accident by which one-half of the scalp was torn from the skull, which was itself fractured. From between the edges of the broken bone, brain substance was oozing, and this kept on for thirty-six hours. Recovery took place. The points to be noticed are:—

1. When the boy was found, he had already walked sixty yards without any assistance from the place in the coal mine where his head was crushed between a waggon and a rock, which forms the side of the "waggon way," and he afterwards walked home.

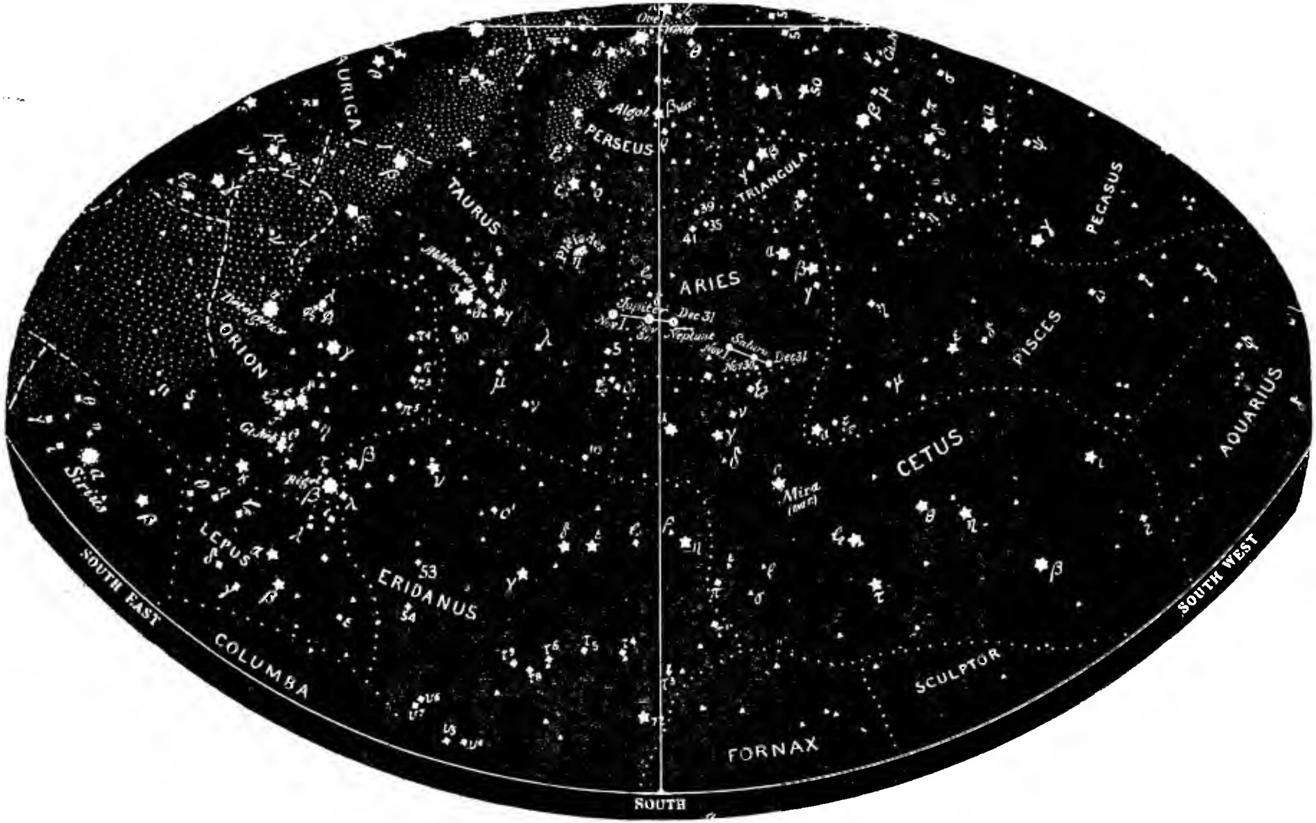
2. Although the injury was inflicted on March 6, 1880, the intellect is not impaired or mind affected in any way.

A man, aged 44, in an attempt at suicide, sent a small dagger through his skull into the brain. He had held the dagger in his left hand, and given it with the right several blows with a mallet, believing that he would fall dead at the first blow. To his profound surprise he felt no pain. He struck the dagger in all about a dozen times. When seen, about two hours after, the handle was projecting, 9 centimètres of the blade being sheathed in the head. For half-an-hour unsuccessful attempts were made to get the dagger out with a strong pair of pincers, the patient being held firm on the ground by two vigorous persons. The patient, who walked well and complained of no headache, was taken to a coppersmith's, and the dagger fixed to a chain passing over a cylinder turned by steam power. The man was fastened to rings in the ground. At the second turn of the cylinder the dagger came out; the patient, who had submitted to these manœuvres, suffered no pain or inconvenience. He walked to the hospital, remained there for ten days without fever or pain. He then returned to his work, and the wound healed.

R. E. Williams.

[*Continued on page* 121.

THE SOUTHERN SKIES IN DECEMBER.



This Map shows the position of the stars in the Southern Skies:—

On November 30, at 10½ o'clock. | On December 8, at 10 o'clock. | On December 16, at 9½ o'clock. | On December 23, at 9 o'clock. | On December 30, at 8½ o'clock.
On December 4, at 10¼ o'clock. | On December 12, at 9¾ o'clock. | On December 19, at 9¼ o'clock. | On December 27, at 8¾ o'clock. | On January 3, at 8¼ o'clock.

The movements of the planets Jupiter, Saturn, and Neptune, now travelling in this part of the stellar heavens, are indicated by the lines marked with these planets' names. All these planets are retrograding, but less swiftly than last month, when, as explained in No. 1, they passed their respective points of opposition. The stars in the southern skies move across ... to right.

... of the first magnitude are shown with eight points, those of the second with six, of the third with five, of the fourth with four, of the fifth with three

Continued from page 118.]

BRAIN AND BRAIN CASE.

[114]—In Letter 42, page 78, of Knowledge, Charles Hamilton asks three pertinent questions. Leaving you to answer the first two as you best can, allow me to make a few brief remarks on the last, viz., "Does the brain shape or mould the form of the cranium or skull?" Dr. Lawrence, late Professor of Anatomy and Surgery to the College of St. Bartholomew's Hospital, London, in his "Lectures on Man," says of the skull that "the general capacity and particular forms depend entirely on the size and partial development of the brain." While Dr. Mayo, late Professor of Anatomy and Surgery to Royal College of Surgeons, in his work on "Human Physiology," after considering the relation between mind and brain, says, "Then it is certain that the skull is formed after the brain, and moulded upon it; and that very moderate attention will enable an anatomist, for the most part, to distinguish those prominences which are caused by inequalities of bone from those which mark the proportions of brain." Dr. Turner, Professor of Anatomy in the University of Edinburgh, in a paper read before the Royal Society of that city, on Jan. 19, 1871, says, "The outer surface of the skull does not correspond in shape to the outside of the brain."

Now, sir, what is the meaning of all this? Out of mere curiosity I have consulted medical men with regard to the mutual relations of skull and brain, till I am tired, and, although they set out with the observation that the question is simply one of fact, no two of them give me the same answer. Indeed, they are completely at sixes and sevens. What I wish to point out as the moral of all this is, that for us there is nothing left but to fall back upon nature, which, as Dr. Gall said, is the only infallible testimony, and bring the inquiry to the test of observation and of fact, leaving medical men to dogmatise as they choose.

Glasgow, *Nov.* 25, 1881. A. B.

TECHNICAL TERMS—SCIENTIFIC NEWS—ILLUSION.

[115]—I take the liberty of sugggesting that you *request* corre spondents to place the technical terms they employ side by side with the popular modes of expression, for the reason given in letter 46, p. 78. In letter 46, p. 79, in the fourth line of the second paragraph, Mr. Allen, in alluding to the shape of the forehead, uses the term "orthomètopy," bracketing the meaning. So far so good, but only in the next line he makes use of the term "prognathism"; three lines further appears "orthognathism," and again five lines below the word "Sella." Writers must know that only specialists, or classical scholars, would understand the meaning of terms such as these. What, therefore, I take the liberty of suggesting would be your placing a standing notice to correspondents at the commencement of the correspondents' column. Also that, under the head of "Scientific News," you give weekly, or from time to time, the most recent inventions or discoveries in physical and mechanical science that would be of popular interest.

F. H. S.

In reference to the drawing I sent you last week illustrating an optical illusion (letter 65, p. 95), the effect is much more striking with the horizontal line uppermost.

[It seems to us more striking still when the angle is put uppermost or lowest.—Ed.]

THE DURATION OF A FLASH OF LIGHTNING.

[116]—Can you or any of your readers give me information on the following two points:—

1. Did Wheatstone or anyone else ever make an experiment on a flash of lightning to determine the time of its duration?

2. If so, what was the nature of the experiment, and where is it described?

I may state, to prevent misapprehension, that I am quite aware that Wheatstone made experiments to determine the duration of an electric spark produced under certain definite conditions, and that these experiments are fully described in the "Transactions of the Royal Society." But I find it explicitly stated by several writers of high authority—for example, Mascart,* Daguin,† Ganot,‡—that Wheatstone made experiments on the lightning flash itself. They say he employed a white disc with black rays, very close together, which he put in rapid rotation, and so placed that it was illuminated by each succeeding flash of lightning. The details of the experiment are fully given by Deschanel,§ who, however, does not ascribe it to Wheatstone. Now I have searched in vain for the authentic record of this experiment, and I should feel greatly obliged to anyone who can give me information about it in the pages of Knowledge.

I think you will agree with me that the subject is not without interest for your readers, when I mention the widely different statements that have been put before the public, quite recently, regarding the duration of a flash of lightning. According to Mascart,* it is less than $\frac{1}{1,000}$ of a second; according to Deschanel,† it is less than the $\frac{1}{10,000}$; and according to Tyndall,‡ less than $\frac{1}{100,000}$. "Wheatstone has shown that it certainly lasts less than a millionth part of a second."§ Of course, if this last statement be true all the others are, in a certain sense, true also; but they do not give us the whole truth.—Yours, &c. Gerald Molloy.

Catholic University of Ireland, 86, Stephen's-green, Dublin, *Dec.* 2, 1881.

*Electricité Statique, ii., 561. †Traité de Physique, iii., 213.

‡ "Treatise on Physics," translated by Atkinson. Sixth Edition, p. 828.

§"Natural Philosophy," translated by Everett. Sixth Edition. p. 641.

* *Electricité Statique*, ii., 561. † *Traité de Physique*, iii., 642.

‡ *Fragments of Science*, fifth edition, p. 311.

§ Lecture delivered in the City Hall, Glasgow, published in Nature, Vol. xxii., p. 341.

LUMINOUS PHENOMENON.

[117]—I find in the volume of the *Leisure Hour* for 1853, under the title of "Astral Wonders," a pamphlet on a lecture delivered by the Rev. J. Craig, M.A., a short time before.

I wish to call your attention to the following. He says:—"When I was a boy, from some cause or other I was put into a dark room and tried to escape out of it. I had a knife in my pocket, and I began to cut a hole in the door to try to get quietly away by coming at the latch. Still, something occurred which induced me to remain where I was. I heard a footstep, and I knew I was not doing quite right. When I pulled my hand back the sun happened to be shining very much and I saw little globules running off in all directions. I thought even then this was very remarkable. I again put out my hand, and on pulling it back I saw the little globules running off as before When I grew up to be a man I began to think of that childish thing. I felt certain then that light was a fluid and could run off our fingers like water; and if you feel any interest in light and will examine for yourselves you will see that light is truly a fluid; it has its waves, its currents, its ocean deeps, and our telescopes may yet tell us something of its tidal surroundings."

I shall be most happy to receive any information on the above subject, viz. (that of light being a fluid) which you or any of your correspondents may afford me.—Yours faithfully, W. M. M.

[Mr. Craig seems to have mistaken a physiological for a physical phenomenon. His experience showed, as many others do, that "seeing is not always believing." In a railway accident I have seen "astral wonders," as I have when I have been pitched on to my head by a galloping horse, but I do not infer that stars reside in my head.—Ed.]

A £5 TELESCOPE.

[118]—"Twenty" (letter 48, p. 79) cannot obtain an absolutely perfect instrument for £5, but for this sum he can obtain from numerous opticians a telescope which has all that is necessary for a beginner, and the other apparatus he can get from time to time (and at no considerable cost), as he becomes more experienced. The £5 telescopes advertised are refractors; that is, the usual sort with a lens (object glass, of 3 in. diameter) at the end of a tube. The other sort, termed reflectors, having a silvered mirror at the bottom of a tube reflecting the image to a focus at the top, are less costly (if a large instrument is wanted), but require very nice management, and should only be purchased after considerable experience is acquired. The £5 glasses are very good ones indeed, containing all the groundwork of a first-class instrument, and will be found all that "Twenty" wants to commence with. After a time, he will like a tall wooden tripod-stand (which can be made by a handy man), and will want to add a "finder;" that is, a miniature telescope fixed at the side, by looking through which a celestial object can be immediately brought into the field of view of the larger one. He will also want a couple of extra eyepieces of different powers to the one supplied with the instrument, as different objects require different powers to show them at their best. After this (if his taste for observation developes), he may like to replace the object glass with one by a celebrated maker. These things, however, only result from an increased passion for the study of the heavenly bodies. The instrument, as supplied for £5, contains all that is necessary for a beginner, and one who simply wishes to observe the stars for occasional pleasure will never want anything more. Such accessories

as the student wants can always be added, and the cheap instrument made as complete as the most expensive. It is a great mistake for a beginner to have too large and complex an instrument, and the difference in view through a very large glass compared with an ordinary one, such as is here referred to, is not so great as would be imagined. I may add, that the eye can be educated the same as the hand, and, after a time, "Twenty" will be able to see much more through his glass than when he started. A novice looking at Jupiter, for instance, will see but a small blank disc, but a practised observer, looking through the same telescope, will see a multitude of interesting details on the planet's surface quite invisible to the former. It is scarcely necessary to add to the foregoing that for £5 a very good second hand instrument can sometimes be got by advertising. There is no occasion, either, to reply to "Twenty's" other telescopic queries, as I see the Editor promises an article upon the subject. ALBERT P. HOLDEN.

107, Hoxton-street, N.

COMETS' TAILS.—RAINFALL AND FORESTS.—THE OLFACTORY TRACT.—INTELLIGENCE IN ANIMALS.

[119] 1. There is a question I should like to ask you with respect to comets' tails, viz.: Has the spectrum of the tail ever been obtained; if there has been one taken, was there any alteration or new band observed in the spectra? I look forward eagerly to the continuation of your articles on the subject, for, I am sorry to say, I am very ignorant on the subject.

2. With respect to your article on the Fiji Islands, I noticed the interesting circumstance of the rainfall diminishing simultaneously to the cutting down of timber. Was it not the same with the Island of Ascension, only in a still more remarkable degree; for had not the inhabitants to plant trees again, so that they might be again blessed with rain? I should be glad if you would inform me if I am correct in the above, for I do not know where to look to refresh my memory on the subject.

3. In reading "M. L.'s" article on "Vivisection," an idea occurred to me connected with the olfactory tract, on which I should like your opinion. It is this:—A good many years ago my cousin and self were working in the same "Lab.," and by accident he smashed a full Winchester of NH_4HO, with the natural result that he was half-suffocated; but the part I am curious about is, that from that day to this he has had neither taste nor smell. Do you think that the dose of NH_3 destroyed that part of the brain referred to by W. L.?

4. "Intelligence of Animals":—I am in a position to give rather a curious case of the intelligence of rats, proving—as does your article—the *practical*, but if we look at it in rather a favourable light, might prove even the abstract, which is wanting.

The rats in this case also "bored" through a lead pipe; so far there is no difference to the example in your paper, and you will think this is one of the "two and two equal four letters"; but this lead pipe, unfortunately for my father, was in the "hold" of one of his ships, and through their (the rats) craving for fresh water, and getting it, a great deal of damage was done; they actually did hit on the *fresh water* lead pipes in *preference* (may I say) to the other salt water ones (leading from the W.C.'s on deck, I mean, which, as you are aware, are cleaned by salt water.) This, I think, is a curious coincidence, for might it not be turned by scientists either way, either to accident or to sagacity on the part of the rats?—Yours, &c., F. C. S.

MEN'S HEADS.

[120]—I have read carefully the letters on the size of human heads. I am inclined to think that our heads, as a nation, are smaller. The size of the head corresponds to that of the body, so that large heads mean large bodies. This is seen amongst navvies, agricultural labourers, and Irishmen from country places. Now-a-days machines do a great deal of our mechanical work, and our muscles getting less exercise, are not so large. Again, living in towns, and having little bodily exercise or hard work, means small bones and smaller frames altogether. The size of the head increases with that of the body, so exercise ought to be part of the system at our public schools, and then we get a sound brain in a healthy body. T. R. ALLINSON, L.R.C.P., &c.

[*Owing to the extraordinary pressure of correspondence, our article on "Comets" (illustrated with views of the destroyed comet of Biela) is deferred till next week. We must earnestly entreat our correspondents to be concise, and only to write when they have something really interesting and new to communicate.*]

Queries.

[67]—RAINBOW.—I should very much like to know why it is that a rainbow is semicircular?—G. S. M.

[68]—SOLAR STORMS.—By what method is the velocity of wind, &c., in the sun ascertained?—G. S. M.

[69]—THE MOON'S INFLUENCE.—It has been stated by Dr. Ball that the result of the moon's action on the tides is to drive the moon further and further away, and that the consequence of this is that the day is getting longer and longer. He says that 50,000,000 years ago the moon must have been very close to the earth, at which time the day would be only three hours long. Will you kindly say if what Dr. Ball says is correct? and how the action of the tides drives the moon further and further away, and how the distance of the moon regulates the length of the day?—E. K. [Will shortly make the question the subject of an article. Dr. Ball's general conclusions are sound; the detailed results he would not, of course, regard as exact.—ED.]

[70]—SEISMOMETER.—A description of an approved form of seismometer would be much valued by M. A. F.

[71]—NAMES OF FLOWERS.—Can you let me know, by means of your valuable paper, of any botany which will give the Latin and English names to all flowers, &c., and thereby much oblige.—R. D.]

[72]—ANTIQUITY OF THE PYRAMIDS.—"Sir John Lubbock, speaking of the antiquity of man, in his address at the late meeting of the British Association, said that 'The researches in Egypt seem to have satisfactorily established the fact that the pyramids themselves are at least 6,000 years old,' and mentions Professor Rawlinson's researches in support of this." What is the evidence on which this alleged antiquity is based?—ACTINOLITE.

[73]—CLOUDS.—Is there any explanation of the peculiar forms of Cirri and other clouds, especially that known as a mackerel sky? If a large, flat vessel of water containing a little sediment is agitated slightly, the sediment is deposited on the bottom in forms resembling those of some clouds. Are the latter supposed to be formed in a similar way?—E. C. R.

[74]—EXPERIMENT ON SOLAR HEAT.—I have read somewhere that if one side be removed from a box (the interior of which is blackened), and in its place three panes of glass with spaces between them containing air are inserted, water placed in a vessel in it may be raised to boiling-point by the exposure of the box in its modified form to strong sunshine. How is it that the heat, when in company with the sun's light, can penetrate the successive layers of glass and air, yet when deprived of its luminous companion, is retained in the heat-trap?—E. C. R.

[75]—THE RADIOMETER.—Would it be asking too much of our Editor to request that a series of papers explaining and describing the little that is known as yet of the forces and phenomena connected with the high vacua and radiometers might be given before long in the very interesting pages of KNOWLEDGE?—E. C. R.

[76]—PLANETARY MOVEMENTS.—The earth moves round the sun because of the latter's attraction, *i.e.*, but for the sun's attraction, the earth would move forward in a straight line. The attractive force of the sun must, therefore, necessarily retard the forward motion of the earth. Is not this so? If yes, does the earth go more and more slowly round the sun, and will it not ultimately stop and be drawn into the sun, and when? If no, what is the force which causes the earth not to go more and more slowly?—E. F. B. HARSTON. [The answer to first question is "no"; and that, therefore, no force is required to prevent the earth from being retarded. Why should a force at right angles to her course retard the earth? Her path not being absolutely circular, the force of the sun sometimes slightly hastens and at others as slightly retards the earth; but in the whole year changes not her speed at all.—ED.]

[77]—TERRIBLE DREAMS.—What explanation can be given of the horrible dreams that sometimes occur to persons?—S. S. S. S.

[78]—ANTIPATHY AND SYMPATHY.—What is the explanation of the extraordinary antipathy felt by some persons against some tame animals and things; and the converse—extreme sympathy, almost amounting to infatuation, shown by other persons (innate, not acquired)?—S. S. S. S.

[79]—MENTAL PHYSIOLOGY.—Whose works are now the best authorities on the study of mental physiology?—S. S. S. S.

[80]—THE CHIN.—It has been said that man is the only animal having a chin. Long before recent attention was called to the deficiency of this feature in a certain homicidal criminal, observation had led me to note various cases of cruelty perpetrated by almost chinless people; once, indeed, by a very young creature of twelve years old, described truly, I think, as "a perfect brute." Where

cruelty has not been observed, gluttony has been the marked fail ng. I have seen a chinless man eat like a pig, and look exactly like a pig while eating. Does not the paucity of this characteristically-human feature point to the animal propensities being in excess of the moral, if not the intellectual, tendencies? The Papuans are said to have little chin, and to be very cruel. ETHNOLOGIST.

[81]—THE MOON'S ATMOSPHERE.—By whom and when was the discovery made that there is no atmosphere around the moon? Swedenborg claims the honour of having the fact communicated to him by angels, and that he was the first to know and publish it.

[82]—PROBABILITIES.—Has not the writer of the article on "Trusting to Luck" put "eight times running" and "nine times running" where he should have written "nine times running" and "ten times running" respectively, the chances being

$$\frac{1}{2^9} \text{ and } \frac{1}{2^{10}} \text{ equal to } \frac{1}{512} \text{ and } \frac{1}{1024}$$

respectively?—H. A. N.—[Yes.—ED.]

[83]—CHEMICAL QUERIES.—(*a*) Are there more than three allotropic modifications of sulphur? The two crystalline forms and the plastic modification I am acquainted with, but in Miller's "Inorganic Chemistry" ("Longman's Text-Books"), 1874, it is stated that "a fourth may be procured by placing in carbon disulphide the hard mass furnished by keeping the viscous sulphur till it becomes solid. The carbon disulphide discloses all that can be removed from the mass, and a *grey amorphous powder* is left" (p. 142). Roscoe, in his "Elementary Chemistry," simply says that "the tenacious" (*i.e.*, viscous) "form is insoluble in" carbon disulphide. Does the latter refer to the amorphous powder mentioned by Miller? (*b*) In Roscoe's "Elementary Chemistry," p. 62, edit. 1880, referring to nitrogen tetroxide, I find it stated that "this substance forms the greater part of the reddish brown fumes evolved when *nitrous* oxide gas escapes into the air." Should this not not be *nitric*? (I may say that at present I have not an opportunity of trying these for myself.)—THEION.

[84]—ANCIENT MAN.—In Darwin's "Origin of Species," I read, "Mr. Horner's researches have rendered it in some degree probable that man sufficiently civilised to have manufactured pottery existed in the valley of the Nile thirteen or fourteen thousand years ago." I should like some information about these researches, and their reliability.—CLIO.

[85]—A GRAVITY ILLUSION.—The Toricelli tube, or a long glass tube filled with mercury, and turned upside down, when full, into a cup of mercury again, is said by some acquaintance of mine to be unexplained, in so far that in theory, he says, the mercury glass-tube, when lifted up, should not weigh more than the weight of the glass, considering that the mercury is carried by the cup on the table in which it is. But, instead of this, it is found to weigh very much more. I was not many minutes in solving the mystery to myself, but do not yet know if it is really a mystery to professors of physics, as my acquaintance maintains. I can hardly think so, as I fancy to have read the law that gives the cause. However, I will give my version of it, after some one of your readers answers.—F. J. D. "SELBST." [There is no mystery in the matter, but the study of this question, as of others in our Query columns, may be a useful exercise to beginners in physics.—ED.]

[86]—THE COLOUR OF BIRDS' EGGS.—Are the different markings on birds' eggs considered to be merely accidental, or designed for special purposes?—ARACHNIDA.

[87]—TOADS.—What is the internal construction of the common toad, which permits of its existing for many years enclosed in blocks of solid matter?—ARACHNIDA.

[88]—BRAIN INJURIES.—How is it the brain can be cut and cauterised without exciting sensation? If true, this contradicts, apparently, the statement that the brain is the organ of feeling, made in W. L.'s letter (29, No. 3). Is electricity necessary to convey the least feeling or irritation to the brain?—BEE.

[89]—GELATINE PLATES.—I have just now a few gelatine plates, in which a red streak appears, either on the top or at the bottom, and seems to eat its way through the film, and so spoils them; also the silver from the paper coming on to the plate. A remedy for the above would oblige. Have any photographic readers of KNOWLEDGE tried to develope their gelatine plates by flowing over them a weak solution of silver, and developing afterwards the same as a wet plate? If so, kindly give me their experiences.—PERSEVERE.

[90]—THE MAGNETIC NEEDLE.—I should be glad if any reader can give me any information why the needle of the compass always points to the north, what is the attraction, &c.—W. H. PERTWEE.

[91]—MISSING LINK.—KNOWLEDGE for Nov. 25, p. 71, has an article, "The Missing Link," in which it refers to an article that has appeared on the same subject in a recent volume of the *Gentleman's Magazine*. Please say name of month in which this appeared.—TEASTLANT.

Replies to Queries.

[4]—THE EARTH'S INCLINATION. The motion of the earth is best shown by suspending a small globe by a piece of string, and carrying it round a candle (the candle being in the centre of its path). If the globe is carried on a level with the candle, it is seen that the poles will both just see the candle, *i.e.*, the poles and the whole earth will have perpetual spring. The true motion will be obtained by making the circular path *dip* half below and half above the level of the candle as it is carried round, so that at the lowest part the whole of the North Arctic zone may be in light, and at its highest point in shadow.—H. A. N.

[8]—VOLUME OF SPHERE.—Given that area of sphere = area of 4 great circles = $4\pi r^2$. Let the sphere be divided into pyramids, with the apex at the centre. If the number of pyramids is great, the base of each is nearly plane, and the pyramid

$$= \frac{1}{3} \times \text{height} \times \text{base}$$

$$= \frac{1}{3} \times r \times \text{base}.$$

The whole sphere is one of these pyramids multiplied by the number of times the area of the sphere contains the base, *i.e.*,

$$\frac{1}{3} r (\text{base}) \times \frac{4\pi r^2}{\text{base}} = \frac{4}{3} \pi r^3$$

$$= \frac{2}{3} \text{ of cylinder.}$$

This implies, however, the knowledge that the area = four great circles, which cannot be proved without the calculus.—H. A. N.

[10]—THE ZOETROPE.—The impression of any sight remains on the retina for the seventh part of a second; if a new impression is received before the first has faded, the two are seen simultaneously, and if the Zoetrope is turned too quickly, the images run into one another. If it is turned so that one-seventh of a second intervenes between two impressions, the motion will appear continuous; if it moves slower than this, it will seem jerky.—H. A. N.

[14]—VELOCITY OF SOUND.—"Sound's" difficulty arises, I think, from his not considering that the air is sensibly devoid of absorption and radiation, so that the heat generated in the condensation remains there to augment the velocity—or, rather, the elasticity upon which the velocity depends. In the rarefaction, the elasticity is lowered, both by the separation of the particles and the cold developed by such separation; and consequently "the cold developed augments the difference of elastic force upon which the propagation of the rarefaction depends." It is because the heat generated in the condensation augments the rapidity of the condensation, and the cold developed augments the rapidity of the rarefaction, that the heat and cold both help to augment the velocity of the sound wave. In gases possessing considerable absorptive and radiative power, "Sound's" objection would be perfectly valid.—T. J. H.

[16]—GERMAN AND ENGLISH.—In reply to "Eclecticus," Messrs. Macmillan & Co. publish a work which I think would suit him, entitled "A Comparative Grammar of the Teutonic Languages." By G. Helfenstein. Its price, I believe, is 18s.—W. G. ROLFE.

[18]—CHEMICAL TREATISES.—I have before me the ninth edition of "Fownes' Chemistry," on the old or equivalent notation, with an appendix epitome of the new system. Then I have Hoffmans' work on "Modern Chemistry;" also the little volume by Wurtz, "Chemical Philosophy according to Modern Theories," translated by Crookes; also "The New Chemistry," by Cooke (International Series); and, lastly, "Pilden's Chemical Philosophy." I would especially award the prize to the little work of Wurtz, which is exceedingly good and clever; but many years' study convinces me that "Modern Chemistry" is wholly based on very slender hypotheses, and that the consequent complexity and confusion must necessitate a change. An independent inquirer has little chance in England, but in France the Chemical Hierarchy is untainted with inconsiderate revolution; and M. Berthelot must be considered one of the greatest chemists of the age. If old or equivalent chemistry means *knowledge*, and new chemistry means hypothesis, it is especially *à propos* for this Paper to ventilate and make clear the difference.—ECLECTICUS.

[19]—COMPARATIVE ANATOMY OF BIRDS AND MAMMALS.—The furcula of birds are formed by the union of two bones called the clavicles. These are almost entirely absent in mammals. When present they are very rudimentary, and usually unossified; so that no trace of them is found in the dried skeleton. In man, the clavicles are more developed than in the other mammals, and constitute the so-called collar-bones. They are very rudimentary in dogs and rabbits. In the felidæ (lions, tigers, cats, &c.) they are more developed than in other carnivora, but have no particular

function. They are entirely absent in the ungulata. Clavicles are very characteristic of the lower vertebrata, being well developed in reptiles and fishes.—Geo.

[19]—Comparative Anatomy of Birds and Animals.—In answer to Query 19 p. 80, the furculum, or "merry-thought," of birds corresponds to the collar-bones, or clavicles, in man, which, instead of being separately joined to the sternum, or breast-bone, are joined together into a V-shaped arch of bone, the apex of which is commonly attached to the sternum by a ligament. The function of the furculum, according to Owen, is to oppose the force which tends to press the humeri or upper-wing bones towards the middle line during the downward stroke of the wing.—Medicus.

[23]—Faure's Accumulator. "Nameless" will find some information in regard to Faure's accumulator in *Nature*, vol. 21, p. 68, "Storing Electricity;" and p. 238 "Faure's Secondary Battery."—A. T. C.

[23]—Faure Accumulator.—A Faure accumulator may be made as follows: Take lead foil such as used for putting on damp walls; cut the plates, leaving a long tongue at one corner. I made the plates $1\frac{1}{4} \times 2\frac{1}{2}$ inches, with a tongue 2 inches long and about $\frac{3}{8}$ of an inch wide; next take some flannel—I used common, at 10d. a yard; cut it in strips one inch more in length than twice the length of the lead plates, and one inch wider than those plates; then take some blotting-paper and cut it into pieces, one inch each way larger than the lead plates; mix one pint of sulphuric acid, by measure, with ten pints of water, by measure, and with this and some red lead, make a paste; now, paint over one strip of flannel, on one side only, with the paste, leaving a half-inch margin of clean flannel all round, so that the painted part of the flannel will be exactly twice the size of the lead plate; paint over one of the lead plates on both sides, keeping the tongue clean; lay it on the painted flannel, and double the flannel over it; lay this covered plate down on any convenient board or piece of glass. On this covered plate lay one of the pieces of blotting-paper, or, if the blotting-paper is not thick and good, two pieces. Prepare another plate of lead with the flannel and red lead, as before; place this on the top of the blotting-paper, taking care that the tongue of this latter plate is on the opposite side to the tongue of the first plate. Lay another piece or pieces of blotting-paper, and so go on until you have a pile of as many plates as will go easily into the jar or cell you intend to use. See that the tongues of the alternate plates are on alternate sides of the pile of plates, then tie the pile round loosely in a couple of places with some paraffined twine; pinch the tongues on one side together, and those on the other side together, and attach terminal wires. Put the bundle of plates into the cell, fill up with the acid and water, and keep it full. I have tried this plan, and found it answer.—H. B. T. Strangeways.

[26]—Training.—There are some facts, if one could collect them, which seem to support "Oarsman's" question. Thus I know of two or three bicycle riders who ride a hundred miles a day, and yet who never taste animal food; and, again, a teacher of swimming who uses no animal food. The last few autumns a number of gentlemen have set out for walking tours, and they never use animal food, and yet get on very well indeed, saying they experience less thirst and less fatigue than if they eat meat, &c.—T. R. Allinson.

[27]—Marriage Death-rate.—With reference to "Benedict's" query respecting the marriage death-rate, I may say, for his information, that the result of an estimation made, showed that a number of married persons gave a mean death-rate of 66·76; and of unmarried a mean of 62·00.* I may also state that at the age of 15 to 20 the mortality is increased considerably, especially among women—the majority of deaths resulting from consumption and childbirth. A greater proportional number of deaths occur among those who marry at an early age.—Michael W. Reynolds.
[Rest of letter not strictly *ad rem.*—Ed.]

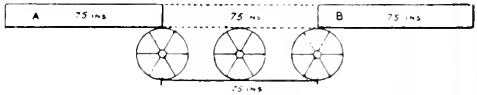
[27]—Marriage and the Death-rate.—I refer "Benedict" to the following extract from the *Times* (Weekly Edition), Febuary 14, 1879.

"It has been shown from statistics that in general, married people have a less mortality than the unmarried or widowed. Among facts indicating the relation between marriage and physical health, it has been proved by M. Janssens, of Brussels, that at all ages widowers are about twice as liable to phthisis as other men, but that married people are generally more liable to this disease than celibates. This law is constant for women; for men it holds good only before 25 years of age and after 45. Such facts and their meaning are discussed by M. Bertillon in a recent paper on the influence of the family state on morals. Not only do married people die less than others, but they show less tendency to suicide, to mental derangement, to assassination, to theft, and other like evils or crimes. . . ."

And the extract goes on to show that this is probably owing to the influence of children.—B. J.

[27]—Effects of Marriage on the Death-rate.—Dr. Bertillon, a French *savant*, some two or three years ago wrote an essay on "Nuptiality, or the Conjunctive Attraction of Human Couples." By comparing the mortality statistics of every country in Europe, he shows that, without exception, marriage is conducive to longevity. He arrives at the conclusion that a bachelor of 25 is not a better life than a married man of 45. French bills of mortality show that the annual death-rate among married men between 20 and 25 is rather under 10 per 1,000, and for bachelors of the same age, 16 per 1,000. For Paris itself the difference is still greater. Between 30 and 35 years of age the mortality is 69 per cent. greater among bachelors than among married men. Space will not permit further quotations from this interesting essay, but a more complete summary of Dr. Bertillon's conclusions "Benedict" may find in the *Lancet* for May 31, 1879.—Medicus.

[28]—Stone on Rolling Wheels.—If the stone was poised exactly in the *centre* of the wheels, it would then be carried forward 75 in. for each revolution, and the relative position of the stone and wheels is not altered. But, as the stone rests on the circumference of the wheels, it receives a motion from the revolution, and the part which rested on the wheels would be 75 in. beyond them for each revolution; and the forward motion of the wheel being 75 in., therefore the stone has advanced 150 in. It has travelled the same distance as a spot on the circumference of the wheels, which describes a cycloid for each revolution, and is equal in length to



double the circumference. I beg to inclose drawing, where A is commencement of a revolution, and B the end.—W. Stevens.

[28]—Stone on Rolling Wheels.—"Queensland's" "mathematical friend" is quite correct in saying the stone advances 150 in. for every 75 in. advance of the rollers. "Queensland," or any one else, may easily convince himself of the accuracy of the statement by a few minutes' experimenting with a two-foot-rule and an office ruler.—E. H. R.

[37]—"Vestiges of Creation."—I have seen it stated that the late Dr. David Page, the geologist, was the author of this book. It has also been attributed to Mr. Robert Chambers. Both may have been concerned in it, as Dr. Page was for a long time connected with *Chambers's Journal*. I think Lieut.-Col. W. A. Ross announced in the columns of the *English Mechanic*, some time ago, that he believed, and could prove, the author to be Sir Charles Lyell.—J. A. Westwood Oliver.

[37]—Vestiges of Creation.—This book was written by the late Dr. Robert Chambers, of Edinburgh. It foreshadowed, in some measure, the speculations of Darwin on the origin of new species through the modification of the old, as opposed to the doctrine of their "special creation." It advanced further than Lamarck; but Dr. Chambers' ideas were necessarily crude, for lack of the knowledge of later days, when compared with the certainties of modern evolution, which is founded on evidence derived from the development, structure, and distribution of animals and plants, as well as from their fossil history. "S. S. S. S." should read Darwin in preference to the "Vestiges," which possess, nowadays, more of a historical than a scientific interest.—Andrew Wilson.

[37]—Vestiges of Creation.—This work was published anonymously. Some—the majority—attribute it to Chambers, others to Sir C. Lyell. The subject was discussed recently in the columns of the *English Mechanic*. I should advise "S. S. S. S." to read it by all means, and mark, learn, and inwardly digest it.—W. G. Rolfe.

[38]—Neptune.—One satellite of Neptune has been discovered by Lassell, period 5 d. 21 h. 8 m., at a distance of 220,000 m. from the primary.—W. G. Rolfe.

[Let. 48]—Cheap Microscope and Telescope.—If your correspondent "Twenty" is not disposed to spend more than £5 on *each* of the above, he must restrict himself to a monocular microscope, and I should advise him to procure one of the so-called medical forms made by Swift, Beck, Baker, and several others. It is rather invidious making a selection from the several makers of low-priced microscopes, but I am bound to say that of the instruments which have come under my notice lately, Swift's are the best value for the money, taking into account the appearance and general qualities of

* There is some mistake about this statement.—Ed.

the stand and the quality of the lenses supplied. This is very much like an advertisement; but you, sir, may possibly know enough of me to tell you that I am not an advertising agent. Anyhow, whereever the instrument is purchased, let "Twenty" see to it that he is not misled by mere appearance. Get a solid stand—one that won't shake and twist about; and don't trouble about a lot of so-called accessory apparatus. You won't want it. Then see that you have a good inch and a good quarter-inch objective. You can do plenty of work with them: enough to make yourself a name in the world, and get plenty of recreation too. I am the possessor of a £5 telescope bought to let my bairns see the mountains in the moon, spots on the sun, and something of Jupiter, Saturn, &c.; but shall I say it, sir, and so shake your faith in my powers of replying to the query, that is all the "astronomy" I have done with it. What it does show it shows well, and enough for my purpose. That is all I can say.—H. P. H.

[48]—Trisection of Triangle.—There are three problems now given up as beyond the power of Geometry, namely, (i) to trisect any angle, (ii) to find a straight line equal to the circumference of a circle, and (iii) to find two mean proportionals between two given straight lines. Thus, "Euclid" is beyond the range of help. W. G. Rolfe.

[54]—Chemical Questions.—The explanation of (1) is that the iron of the ferrocyanide is not a base, but forms part of an acid, which, however, has never been isolated. Potassium ferricyanide acts in a similar way. A parallel case is chromate of potassium, or any metal which will not precipitate, as hydrate, unless reduced by zinc, or tin and acid. (2) To this part of "Castor and Pollux's" query, I cannot see an answer, unless the solution were alkaline; alkalies dissolving potassium tartrate.—C. T. B.

[54]—Chemical Questions.—In the first case given by "Castor and Pollux," the reason he has obtained no precipitate with ferrocyanide of potassium on adding the re-agents mentioned is this, that he has been inattentive, I might even say careless, enough to add hydrosulphuric acid instead of ammonium sulphide. He very correctly states that iron is one of the third group of metals, but the metals of that group are precipitated by Am Cl, Am HO and Am HS, and not by H_2S. And in the second case it is quite possible that the solution of tartaric acid was not sufficiently strong, although the salts under examination may have been so. This test is extremely delicate, and requires much more care than the questions of "Castor and Pollux" lead me to believe he has taken in his analysis.—Theophilus Pitt, A.K.C.

[55]—Greek Verbs.—Lengthening the vowel is the rule, deviation from it the exception. Those verbs which retain the short vowel take σ in pf. and ppf., med., or pass.; also in their verbal adjectives.—W. G. Rolfe.

[59]—Mercury's Revolution.—Undoubtedly 84 was a misprint; it should have been 88.—W. G. Rolfe.

[60]—Sound being the effect produced on the ear by any vibration, within limits of fastness and slowness, any substance that will vibrate can transmit it. Tyndall compares transmission of sound to a blow passed along a number of balls touching side by side; the last of the row only moves, so the last vibrated particle gives the sound, and can pass it on from one substance to another. The greater the elasticity—as steel, glass, ivory—and the lighter in weight, the better does a body transmit sound.—C. T. B.

[66]—Venus.—This planet is frequently visible in the day-time, and often casts a shadow at night.—W. G. Rolfe.

[66]—Venus by Daylight.—Taking the above query of F. H. S. to refer mainly to the planet's visibility *in England*, I may tell him that, owing to the unfavourable atmospheric condition that generally obtain here, she probably is not often visible during the daytime, although I saw her plainly several times this summer, on different days in (I expect) the month of July, when she had a considerable north declination, and was at her extreme westerly elongation from the sun (or thereabouts). I should mention that by visibility I mean visibility to the *naked eye*. If F. H. S. has sufficient knowledge of astronomy to understand the meaning of the terms "right ascension" and "declination," he will know how to find the position of Venus in the sky at any time by her bearing from the sun. Abroad, within and without the tropics, I have seen Venus day after day, for weeks at a stretch, and at sea have often determined the latitude daily for like periods, both in the mornings from about 9 to 10, and in the afternoons from 2 to 3, according as she was, respectively, to the west or to the east of the sun. In many of these cases, however, she would not be visible to the naked eye. If your correspondent has not that slight skill in astronomy with which I have credited him, I am afraid you would hardly allow me space to explain to him how and when to "spot" Venus.—Winter.

INFLUENCE OF MARRIAGE ON THE DEATH-RATE.

Note.—The enclosed is an answer to the query of "Benedict" upon the effects of marriage on the death-rate, if you should consider it worthy of insertion. W. H. Pertwee. *

For several years it has been noticed by statisticians that the death-rate of unmarried men is considerably higher than the death-rate of married men and widowers. I believe that Dr. Stark, Registrar-General for Scotland, was one of the first to call attention to this peculiarity, as evidenced by the results of two years' returns for Scotland.

But the law has since been confirmed by a far wider range of statistical inquiry. The relative proportion between the death-rates of the married and of the unmarried is not absolutely uniform in different countries, but it is fairly enough represented by the following table, which exhibits the mortality per thousand of married and unmarried men in Scotland:—

Ages.	Husbands and Widowers.	Unmarried.
20 to 25	6·26	12·31
25 „ 30	8·23	14·94
30 „ 35	8·65	15·94
35 „ 40	11·67	16·02
40 „ 45	14·07	18·35
45 „ 50	17·04	21·18
50 „ 55	19·51	26·34
55 „ 60	26·14	28·54
60 „ 65	35·63	44·54
65 „ 70	52·93	60·21
70 „ 75	81·56	102·71
75 „ 80	117·85	143·94
80 „ 85	173·88	195·40

From this table we are to understand that out of one hundred thousand married persons (including widowers), from 20 to 25 years old, 626 die in the course of each year; while, out of a similar number of unmarried persons between the same ages, no less than 1,231 die in each year. And in like manner all the other lines of the table are to be interpreted. Commenting on the evidence supplied by the above figures, Dr. Stark stated that "bachelorhood is more destructive to life than the most unwholesome trades, or than residence in an unwholesome house or district where there has never been the most distant attempt at sanitary improvement of any kind." And this view has been very generally accepted, not only by the public, but by professed statisticians; yet, as a matter of fact, I believe that no such inferences can legitimately be drawn from the above table. If death strikes down in five years only half as many of those who are unmarried between the age of 20 and 25 (as appears from the above table), and if the proportion of deaths between the two classes goes on continually diminishing in each successive lustre (as is also shown by the above table), it seems reasonable to infer that the death-rate would be even more strikingly disproportionate for persons between the ages of fifteen and twenty than for persons between the ages of twenty and twenty-five. I believe, indeed, that if Dr. Stark had extended his table to include the former ages, the result would have been such as I have indicated. Yet few will suppose that very youthful marriages can exercise so singularly beneficial an effect. It may appear at first sight that we are bound to accept the conclusion that matrimony is favourable to longevity. In the present instance we have simply to deal with the fact that the death-rate of unmarried men is higher than the death-rate of married men and widowers. All that we can can do is to show that one of three conclusions must be adopted. Either matrimony is favourable (directly or indirectly) to longevity, in a degree sufficient wholly to account for the observed peculiarity, on a principle of selection—the effect of which is such as, on the whole, to fill the ranks of married men from among the healthier and stronger portion of the community—operates in a sufficient degree to account wholly for the observed death-rates; or, lastly, the observed death-rates are due to the combination, in some unknown proportion, of the two causes just mentioned. No reasonable doubt can exist, as it seems to us, that the third is the true conclusion to be drawn from the evidence supplied by the mortality bills. Unfortunately, the conclusion thus deduced is almost valueless, because we are left wholly in doubt as to the proportion which subsists between the effects to be ascribed to the two causes thus shown to be in operation. It scarcely required the

* As I considered this article worth sending to the *Daily News*, I suppose I may accept Mr. Pertwee's suggestion. Passages from the article are quoted by Darwin, in his "Descent of Man," and his approval led me to include the article in my "Light Science for Leisure Hours." Mr. Portwee's quoting from the original suggests that the matter may be new to many of our readers.—R. A. Proctor.

evidence of statistics to prove that each cause must operate to some extent. It is perfectly obvious, on the one hand, that although hundreds of men who would be held by insurance companies to be "bad lives" may contract marriage, yet, on the whole, a principle of selection is in operation which must tend to bring the healthier portion of the male community into the ranks of the married, and to leave the unhealthier in the state of bachelorhood. A little consideration will show, also, that, on the whole, the members of the less healthy trades, very poor persons, habitual drunkards, and others whose prospects of long life are unfavourable, must (on the average of a large number) be more likely to remain unmarried than those more favourably situated. Improvident marriages are undoubtedly numerous, but prosperity and adversity have their influence, and that influence not unimportant, on the marriage returns. On the other hand, it is perfectly obvious that the life of a married man is likely to be more favourable to longevity than that of a bachelor. The mere fact that a man has a wife and family depending upon him will suffice to render him more careful of his health, less ready to undertake dangerous employments, and so on; and there are other reasons which will occur to everyone for considering the life of a married man better (in the sense of the insurance companies) than that of a bachelor. In fact, while we are compelled to reject Dr. Stark's statement, "bachelorhood is more destructive to life than the most unwholesome trades, or than residence in an unwholesome house or district, where there has never been the most distant attempt at sanitary improvement of any kind," we may safely accept his opinion that statistics "prove the truth of one of the first natural laws revealed to man, 'It is not good that man should live alone.'" Whether the law required any proof is a question into which we need not enter.

From the *Daily News*, *Oct.* 17, 1868. W. H. Pertwee.

Our Mathematical Column.

MATHEMATICAL QUERIES.

[2]—The Witch of Agnesi.—Will you kindly furnish a few particulars respecting the history, properties, and practical application (if any) of the above-named curve? If these are accompanied with a tracing of the curve all the better.—E. H. R.

[3] Apparent Paradox.—

$$\begin{aligned}
&\text{Let} & x &= y, & \\
&\text{then} & x^2 &= xy, & \\
&\therefore & x^2 - y^2 &= xy - y^2, & \\
&\therefore & (x + y)(x - y) &= y(x - y), & (A)\\
&\therefore & x + y &= y, & (B)\\
&\therefore & y + y &= y, \text{ since } x = y \text{ by Hyp.;} & \\
&\therefore & 2y &= y & \\
&\therefore & 2 &= 1. &
\end{aligned}$$

Anyone who kindly explains the fallacy in the reasoning which brings about this impossible result will much oblige—Puzzled.

[The fault lies in passing from A to B. Interpret them, and we see this at once. A really means that $(x + y)$ times nought is equal to y times nought, which is, of course, true; just as it is true that twenty times nothing is equal to ten times nothing. But we can no more infer that $x + y = y$ than that $20 = 10$. In fact, we cannot divide both sides of an equation by any common factor, unless we are sure that the factor is not equal to nought. In this case we know that it is.—Ed.]

[4]—I shall be very much obliged if you will kindly favour me with a solution of the following problem. Data. $R = 2$; $S = \sqrt{3}$; $r_b = 1$; r, r_a, r_b, r_c, being radii of inscribed and escribed circles: prove that $4 r r_a = a^2$, and $r_b = r_c =$ perp. from angle A on side BC of inscribed triangle.—Amicus.

Our Whist Column.

By "Five of Clubs."

A "YARBOROUGH" HAND AT WHIST.

Sir,—I was told the other day that a former Earl of Yarborough was always ready to wager £1,000 to £1 against the occurrence of a hand at Whist in which there should be no card better than a nine. Was the bet a fair one? Aletheus.

[The bet was decidedly unfair, and if made a great number of times must have resulted in large gains to the person who made it. It is easy to calculate the odds before the deal, (after the deal, or if the cards are cut and the lowest card is known, the odds are slightly altered). In each suit there are five cards, ace, king, queen, knave, ten, above a nine, or in the pack, 20 cards above a nine. From the remaining 32 cards a hand of 13 cards may be formed in

$$\frac{32 \cdot 31 \cdot 30 \cdot 29 \ldots 20}{1 \cdot 2 \cdot 3 \cdot 4 \ldots 13}$$

different ways. The whole pack, however, will form

$$\frac{52 \cdot 51 \cdot 50 \cdot 49 \ldots 40}{1 \cdot 2 \cdot 3 \cdot 4 \ldots 13}$$

different hands of thirteen cards. The chance, then, that any hand taken at random will have no card better than nine is represented by the ratio which the former of these amounts bears to the latter, or by the fraction

$$\frac{32 \cdot 31 \cdot 30 \cdot 29 \ldots 20}{52 \cdot 51 \cdot 50 \cdot 49 \ldots 40} = \frac{32 \cdot 31 \cdot 30 \cdot 29 \cdot 28 \cdot 27}{51 \cdot 49 \cdot 47 \cdot 45 \cdot 43 \cdot 41 \cdot 2^7}$$

$$= \frac{31 \cdot 15 \cdot 29 \cdot 14 \cdot 27}{51 \cdot 49 \cdot 47 \cdot 45 \cdot 43 \cdot 41} = \frac{31 \cdot 29 \cdot 2 \cdot 3}{17 \cdot 7 \cdot 47 \cdot 43 \cdot 41}$$

It will be found, on reducing, that this fraction is rather less than $\frac{1}{1828}$, so that Lord Yarborough, if he had been fair, (assuming always that he knew how to calculate probabilities) should have offered rather more than £1,828 to £1 against the occurrence of the hand in question. It must be understood, of course, that he wagered with one of the players against that player having a "Yarborough," not against the occurrence of a "Yarborough" among the four hands dealt. The chance of this latter event is, of course, considerably greater. It might seem at a first view that it was exactly four times as great, since there are four hands for each deal, but this is not the case, any more than the chance of the occurrence of a Yarborough in 1,828 hands amounts to 1,828 times $\frac{1}{1828}$, or to absolute certainty. The real chance that a Yarborough will not occur in four hands is thus obtained. The chance that a Yarborough will not occur in any given hand is $\frac{1827}{1828}$; that it will not occur in two hands is $\left(\frac{1827}{1828}\right)^2$ that it will not occur in three hands is $\left(\frac{1827}{1828}\right)^3$, and that it will not occur in four hands is $\left(\frac{1827}{1828}\right)^4$. This is very nearly, but not exactly, equal to $\frac{1828}{1832}$, or to $\frac{457}{458}$; so that the chance of a "Yarborough" occurring in any four hands, taken at random in different deals, is equal to about $\frac{1}{458}$; nor is the chance different when the four hands are in the same deal.

Supposing Lord Yarborough offered a wager of £1,000 to £1 to each member of a whist party, for ten deals, on each of 100 nights in each of ten years, he would have cleared about £18,000.]

"Mogul" writes to us that there are mistakes, some of them serious, in our Whist Column. It is very likely; but the principle on which this column, like the rest of Knowledge, is conducted, is that of free discussion, and the correction of errors as soon as detected and pointed out. "Mogul" only notes one, and there he misapprehends us entirely. He says we in effect say that the rules for leading are based on the principle of giving information to your partner. We have said nothing of the sort. We have said that the first great principle of the scientific game is to give your partner all the information in your power, consistently with the rules of the game. This is a very different thing. "Mogul" states rightly enough that the primary consideration in selecting what card to lead, especially what card to lead from any peculiar combination of cards in a suit, has been the best chance of trick-making. Of course, this is true; but, as an objection to our statement, "Mogul" might as reasonably have told us that the primary object in Whist was to make tricks. "Mogul" will find, as we proceed, that all questions of leading and play, second-hand or third-hand, are primarily weighed with reference to the chance of making tricks (which, by-the-way, has not yet been fully done, even the ablest Whist players being apt to shirk the mathematical problems involved). But that is not at all inconsistent with the statement respecting the distinction between scientific and unscientific Whist, or between what may be called the twenty-six card and the thirteen card games.

"Mogul" invites our attention to Hoyle and Cavendish. These, with Matthews (though Hoyle and Matthews are now a little out of date), Pole, Clay, Drayson, and others are our guides; to this degree, at any rate, that we should not depart from their teaching without assigning our reasons and speaking under correction from our readers. But vague corrections, like "Mogul's," are of little use to us.

Our Chess Column.

Great pressure of other matter compels us to limit our chess this week to a single game. It is, however, annotated fully, as promised in Number 1.

Game Played at Leamington Meeting, October, 1881.

Queen's Fianchetto.

WHITE. Mr. Walton.	BLACK. Mr. Bowley.
1. P. to Q.Kt.3.	P. to K.4. (a)
2. B. to Kt. 2.	Q.Kt. to B.3.
3. P. to K.3.	P. to Q.4.
4. B. to Kt.5.	B. to Q.3.
5. Q. to K.2. (b)	B to K.3
6. P. to K.B.4.	P. to K.B.3.
7. P. takes P.	P. takes P.
8. P to B.4. (c)	Q. to R.5.(ch.)
9. P. Kt.3.	Q. to K.5.
10. Q. to B.3. (d)	Q. to B.7.
11. B. to B.3. (e)	K. to K.2. (f)
12. Kt. to K.2.	R. to K.B.
13. Q. to Kt.2.	Kt. to B.3.
14. Castles.	P. to K.R.4.
15. P. to Q.4.	Kt. to K.Kt.5.
16. P. to B.5. (g)	R. takes R.ch. (h)
17. Q. takes R.	R. to K.B.
18. P. takes B.(ch.)	P. takes P.
19. Q. to K.sq.	Q to K.5.
20. Kt. to Q.2. (i)	Q. takes P.(ch.)
Resigns (j)	

NOTES BY MEPHISTO.

(Incorporating Mr. Bowley's comments.)

(a) In reply to 1. P. to Q.Kt.3., it is not advisable to advance P. to K.4. and Q.4. too hastily. P. to K.3. and Q.4. brings about a normal and safe development.

(b) Loss of time: White might have played P. to K.B.4. at once if that was his intention; but this attack was premature. P. to K.R.4. would have been better than Q. to K.2., to prevent the check with the Queen on R.5. Proper play was to develop the game by 4.P. to Q.B.4, Kt. to Q.B 3., &c., after the fashion of Queen's openings.

(c) Having missed the flood tide of the opening, fortune turns against White. P. to B.4. here is weak, as it opens up his game, of which Black at once takes advantage. In view of the good position of the Black Bishops, it would have been dangerous to castle on the King's side. 8. Kt. to Q.B.3. might have been played. Mr. Bowley points out that Black could not venture on

8. $\frac{\text{B. takes Kt.ch.}}{\text{P. takes B.}}$ followed by 9. B. takes P., because of

9. Q. to R.5. (ch.) 10. $\frac{\text{P. to K.Kt.3.}}{\text{Q. to K.5.}}$ winning.

(d) 10. Kt. to K.B.3. would have been better. If then Black played 10. Q. to B.7. 11. P. to Q.3. would have equalised the game; or, if B to Kt.5., in reply to 10. Kt. to K.B.3., then 11. Castles with a safer game than that obtained through the move in the text.

(e) Mr. Bowley thinks that 11. $\frac{\text{P. takes P.}}{\text{B. takes P.}}$ 12. $\frac{\text{Q. takes B.}}{\text{Kt. to K 2.}}$ 13. $\frac{\text{Q. to Q.3.}}{\text{Q. takes B.}}$ 14. Q. to B.3. would have been better. Should Black venture on 11. Q. takes B., White ought to win, as the following variation shows:— 11. $\frac{\text{P. takes P.}}{\text{Q. takes B.}}$ 12. $\frac{\text{P. takes B.}}{\text{Kt. to K.2.}}$ (to avoid the mate, and at the same time prevent B. takes Kt. (ch.), followed by Q. takes P. and Q. to Q.B.3.) 13. $\frac{\text{Q. to B.7. (ch.)}}{\text{K. to Q.sq.}}$ 14. $\frac{\text{Kt. to K.2}}{\text{Q. takes R.}}$ 15. $\frac{\text{Castles}}{\text{R. to K.sq.}}$; *best* to avoid the mate by Q. to Q.B.3.(ch.) 16. $\frac{\text{Q.Kt. to B.3.}}{\text{Q. to Kt.7.}}$ 17. Q. takes P. White now threatens to win by R. to B.8. If, to prevent this, Black plays 17. Q. to B.7., with the object of bringing her to K.Kt.3., then White plays 18. P. to K.4. first. The only move for Black would be 18. Kt. to Q.5.; then follows 19. $\frac{\text{R. to B.8.}}{\text{Kt.takes Kt.(ch.)}}$ 20. $\frac{\text{B. takes Kt.}}{\text{P. to B.3.}}$ to avoid the mate by R. takes R.(ch). 21. $\frac{\text{R. takes R (ch.)}}{\text{K. to B.2}}$ 22. $\frac{\text{R. takes Kt.(ch.)}}{\text{B takes R.}}$ 23. $\frac{\text{Q. takes B.(ch.)}}{\text{K. to Kt.3.}}$ 24. $\frac{\text{Kt. to R 4 (ch.)}}{\text{K. to R 4}}$ 25. Q. to B.5.(ch.) and wins. We give a diagram of the position

Position after Black's tenth move, Q. to B 7

Black.

WHITE.

(f) Played to make the Rook available for attack. White could not now play 12. P. takes P., as pointed out by Mr. Bowley, as Black would win by 12. $\frac{}{\text{R. to K.B.sq.}}$ 13. $\frac{\text{Q. to Kt.2.}}{\text{B. takes P.}}$ winning, as Queen cannot take the Bishop on pain of Mate in two by Q. to B.8.(ch.) and Q. to K.B.8 Mate.

(g) White's game is now seriously compromised. If he had tried to play 16. Kt. to B.4., instead of P. to B.5., then Mr. Bowley points out that he would have played the fine move of R. takes Kt., and after the subsequent exchanges Black would retain a slight superiority, *e.g.*, $\frac{\text{Kt. to B.4.}}{\text{R. takes Kt.}}$ $\frac{\text{K.P. takes R.}}{\text{Q. takes Q.}}$ $\frac{\text{K. takes Q.}}{\text{Kt. to K.6.(ch.)}}$ &c. Of course White could not play Q. takes Q. in reply to R. takes Kt., for then Black would win by R. takes R.(ch.) and Kt. to K.6.(ch.)

(h) If Black play Kt. takes K.P., White's reply would be 19. $\frac{\text{R. takes R.}}{\text{Kt. takes Q.}}$ 20. Kt. to R.3. forcing the Queen back, and retaining three pieces for the Queen.

(i) A slip, of course. Mr. Bowley tells us that Kt. to B.4., giving back the piece would have been White's best; but even then Black would retain a superiority.

(j) White must lose the Queen, or be Mated in very few moves.

In last week's Chess column position numbered 3 should have been 4. In the solution to Mr. Healey's Problem 2 it is unnecessary to consider the reply, 1. K. to Q.R.sq., for if Black so play White can win by 2. R. to Q.B.7.

In reply to numerous correspondents, we note that solutions of all problems we may give will appear a fortnight later. Names of any who, having correctly solved problems, care to forward their results will also appear.

While adhering to our plan of giving fully annotated games, and analysis of the openings, we shall be glad to publish problems and games, &c., of interest, which correspondents may send us.

Our friend "The Enemy" (need we say we refer to Mephisto) sends us a three-mover, recently published in the *Glasgow Herald*, by Mr. C. R. Baxter, Dundee, which we shall venture to publish next week. In this problem our two-mover of last week would seem to be embodied. Sometimes such coincidences are accidental (we may give a case in point presently). As our two-mover was published in the *Illustrated London News* more than twenty years ago (more exactly, on May 7, 1859), we presume Mr. Baxter will hardly claim priority.

Synchronised Electric Clocks.—It appears that an error has occurred in the *Times*, and most of the papers, including Knowledge, in noticing the lecture lately given by Mr. John Lund, on the above subject at the Society of Telegraph Engineers. In mentioning the practicability of utilising the telegraph wires for the double purpose of telephoning and synchronising, it was stated that communication was effected between the Lecture-room and Messrs. Barraud & Lund's premises in Pall Mall, whereas it should have read, between the Lecture-room and the premises of Messrs. Lund & Blockley, in Pall Mall, who are concessionaires for the West-end district. The two firms are, however, otherwise distinct.

Answers to Correspondents.

**** All communications for the Editor requiring early attention should reach the Office on or before the Saturday preceding the current issue of* KNOWLEDGE, *the increasing circulation of which compels us to go to press early in the week.*

HINTS TO CORRESPONDENTS.—1. *No questions asking for scientific information can be answered through the post. 2. Letters sent to the Editor for correspondents cannot be forwarded; nor can the names or addresses of correspondents be given in answer to private inquiries. 3. No queries or replies savouring of the nature of advertisements can be inserted. 4. Letters, queries, and replies are inserted, unless contrary to Rule 3, free of charge. 5. Correspondents should write on one side only of the paper, and put drawings on a separate leaf. 6. Each letter, query, or reply should have a title, and in replying to letters or queries, reference should be made to the number of letter or query, the page on which it appears, and its title.*

F. H. R. The Witch of Agnesi is one of those curves invented in former times for the solution of geometrical problems which cannot be solved by use of the circle and straight line only. We find little about it in our books. Its equation is given in Brande's "Dictionary of Art and Sciences." We must study it a little ourselves before answering your question; and, next week, hope to give a drawing of the curve and a brief account of it, geometrical, if possible.—PUZZLED. We do not wonder you are puzzled. The statement in the *Times* is out of date where it is correct, and the rest of it amusingly inexact. Newspaper science is apt to be "mixed" in this way. O. G. WAKEFIELD. Your form of solution was specially excluded when the prize was offered. We have, however, given your solution as a good illustration of the way of writing down to any required position, altering, however, your notation, which occupies too much space. W. W. WILLMOTT. Your query appears, and number containing it has been sent to author of the paragraph.—E. C. R. Thanks for interesting letter.—W. A. S. Very glad indeed to hear from you. How long it is since we met. Your reply is a specially welcome one, for I feared that particular query would remain unanswered. Sought everywhere for information, but knowing no Rabbis could get none that was trustworthy.—WALTER E. R. We hope to have many papers on Biological subjects ere long; but Science is infinite, and Knowledge is not.—JOHN BLAND. In your long letter you urge the familiar objections to the sun's being the source of heat, and advance a new theory of the tides. Of the former our readers have had a fair supply already, and the latter is quite inconsistent with known facts. You are also evidently unacquainted with the facts on which the accepted theories are based. For instance, you say the sun exercises greater attractive power than the moon, yet raises no double tide, the facts being (1) that the sun's tide-raising power is less than the moon's in the proportion of 2 to 5, yet (2) that he does raise a double tide of proportionate height. We wish you and others who propound paradoxes, to understand that your objections are in reality your difficulties. Those objections were difficulties to science students some two or three centuries ago. There is an unscientific objection to your communication on the ground of postage underpaid.—J. McGRIGOR ALLAN. We agree with much that you have written, but our pages are scarcely suited for the physiological study of the subject. We agreed to a concise presentation of your views, but supposed the column in Number 4 was what you meant; or, that if section 2 were treated at all, it would be very briefly. The other sections will be more suitable. Pray understand that we are satisfied no student of science could by any possibility object to your views or your way of putting them; but we write not for students of science only—*virginibus puerisque cantamus.*—BERTRAND E. DAWSON. There is no real difficulty. Binders have repeatedly to insert half-sheets. They will have to do so in the case of our star maps. There is much less risk of the single page separating from its fellows than of a full sheet coming out through imperfect sewing.—J. F. D. SELBST. It is impossible to insert your "intentionally ambiguous" remarks on the cause of gravity. Your replies to "Queries" 13 and 3 are unsatisfactory from a similar ambiguity. Your fears seem uncalled for. Publishing a new and sound theory on any subject whatever, by no means involves, as a necessary consequence, "starvation, despair, and early death." Wasting many years over a false theory, through unwise caution, may be more likely to lead to such undesirable results.—J. HINES. Your communication is too long for the subject, now nearly exhausted (though the sun's heat is not). The "prevailing hypothesis of the action of undulatory forces" is by no means inconsistent with the heat of the sun being "true heat in the naked form," that is (as we understand you) such heat as there is in a fire or furnace. Your theory is quite inconsistent with facts; and the growing pressure on our space, though it does not forbid our weighing objections and considering doubts, will not allow of our devoting columns to the careful enunciation of untenable hypotheses. No paradox-monger who can honestly say (and effectively show) that he has thoroughly mastered the accepted theories and the researches on which they rest, shall be refused ample space here. But paradoxes evidently based on misconceptions like those of Mr. Newton Crosland and others can no longer have the space which we earlier allowed them. They must make way for the sounder matter which reaches us.—H. M. HAASSEN. You can scarcely form an idea of the pressure on our space. Such a list as you suggest would be useful only to a small section of our readers. Almost every one who attends such meetings knows when they are to be held without our assistance.—JOHN SPILLER. Thanks for your letter, which shall appear, unabridged. It is very much to the purpose.—ACTINOLITE. Have inserted the gist of your query, but have omitted the closing words. What has KNOWLEDGE to do with Professor Rawlinson's belief, or want of belief, in Mosaic chronology? We know nothing about the matter, to begin with; and we do not want to know.—ALBERT E. HOLDEN. Thanks for your useful letter.—E. KAYCE. Thanks for your suggestions. We are proceeding tentatively at present, and, indeed, just now our unexpectedly rapid success involves certain practical difficulties which will presently disappear. The principles which will guide us in the choice of subjects will be Darwinian, to wit, natural selection and the survival of the fittest.—JOHN PHILLIPS. We consider art questions altogether suited to our columns; and shall be glad to admit correspondence on such subjects as you mention.—S. P. EASTICK. We have been promised a paper on Faure's Accumulator by a skilful electrician; but there has been great delay. If your suggested contribution were short and simple, we should be glad to use it. Spare not illustrations such as the subject may require.—DIXON KEMP. Many thanks. We should be glad to hear of the results of your experiments.—NEWTON CROSLAND. Can insert only portions of your very voluminous letters. You misunderstand our comments; but not more than you have misunderstood the teachings of science.—HENRY SIGGIN. We by no means intend to forget geology.—E. F. B. HARSTON. Thanks for kind wishes. Query appears.—A. FROOD. Many thanks for your offer, but original matter, correspondence, queries, &c., leave no room for reports of lectures, which are nearly always unsatisfactory.—T. J. H. Your remarks on "Anti-Guebre's" letter are sufficiently sound, but he has been already answered. Wonderful to relate, he sees and admits his error. We say wonderful, because when anyone has allowed misapprehension to blossom into false theories, his case is too often past cure. There are the makings of a man of science in "Anti-Guebre."—S. S. S. S. We think it better to let our readers join in answering questions even on those subjects which we have specially studied. Where no other answer comes we shall endeavour to supply the omission, and, sometimes, as you may have noticed, we take the initiative. But we wish this to be a magazine of general knowledge, not a mere organ for the expression of Mr. Richard A. Proctor's opinions on things in general. *We* have, indeed, a number of questions to ask, only hitherto we have not been able to find room. Your query about spiritualism is omitted. We could not find space for the evidence which would pour in from all sides, for and against, still less could we find time to sift it, and without sifting, it would be of little worth either way. Another question is omitted as not quite suited to our pages. We must not offend even the weakest brethren, still less the weaker sisters.—E. C. R. Your letter about parhelia in 1879, is very interesting, but crowded out by more recent matter.—R. E. WINTER. Note answer to Mr. McGrigor Allan.—B. M., F.R.A.S. Many thanks. Your interesting stories of intelligence in animals shall certainly appear.—J. PARKER FOWLER. In the great pressure of more strictly scientific matter, your hypothesis that ghosts and spectres, *et hoc genus omne*, may, perchance, be ultra violet or ultra red, and so only visible to certain eyes, cannot appear in our columns. We perceive that you do not regard it as "true, or even in any slight degree probable." We agree with you thoroughly, and also in considering that it is original. "If it has ever been started before," we, like yourself, "have never seen anything of it."—HARRIS RUSSELL. You will see that many stars are named in the maps for this month. We may shortly find space for the notes you suggest; but being astronomical ourselves, we do not wish to give our readers too much astronomy.—F. H. G. We hope to be able soon to give short biographies of eminent men of science. Space is our trouble just now. As we grow we shall do all we can.—F. COWLEY. Many thanks.—R. C. COWELL. No advertisements need be bound in; of course there will be a title-page.—P. M. TURNBULL. Compare the date of the passage you quote with that of Huxley's later utterances about automatism in animals.—ENQUIRER. Where do you get the "hydrogen of the air" in sufficient quantity to account for the heavy rainfall?—C. T. B. The question of the inferiority of woman has been now sufficiently discussed. Mathematical answer well known; chemical answer inserted.—R. J. BURNSIDE, W. MONT, and others. Thanks.—MARY B. ALDER. See reply above to C. T. B. Thanks.—J. D. HARDY. We regret that space cannot be found for your ingenious speculations about comets. Most probably the coma may consist of crystalline particles ("of the particular nature belonging to the gas" forming the comet as a whole); but the laws of optics will not permit us to regard the tail as produced in the way you suggest. Moreover, the motion of the particles under solar attraction would not be what you suppose. Each particle would travel on its proper ellipse as certainly as a planet would. We, unfortunately, *must* limit greatly the space we allow to the unsound, however ingenious, in order that we may keep room for scientific truths.—A. S. FARQUHARSON. We would rather not insert your query. Sleep preventatives are not rashly to be meddled with. Better yield to nature's hints that she needs her "sweet restorer."—AMICUS. Your mathematical problem belongs to an easy class; we have left it for our mathematical readers to deal with.—D. C. ANDERSON. Your kind suggestion noted.—W. H. PERTWEE. Have printed the passage you have kindly sent me; recognised an old friend in it, as you will see.—VEGA. Thanks; an obvious mistake.—E. M. KING. The article was not such easy reading as we could have wished; but the author is very eminent in that department.—J. A. L. R. Your reasoning, which is perfectly sound, has already been employed (p. 8).—WEATHER GUIDE. A correspondent suggests that a weather guide might be framed "without difficulty," in which the indications of the heavens, stars, moon, sun, clouds, movements and cries of animals and birds, the human body, plants, flowers, liquids, apparent nearness or distance of hills, &c., would help. We believe such a guide might be framed, but not without difficulty. "Exceptions" would, probably, occupy the larger part of the guide. But the matter is worth considering.

We are sorry to say that we continue to receive many letters calling attention to the difficulty of obtaining copies of KNOWLEDGE. We can only repeat that we have done all in our power to prevent this, and that our publishers will be obliged if subscribers will call their attention to the fact, should they experience any difficulty in obtaining KNOWLEDGE at any of the railway book-stalls, or from their local newsagents. To ensure a supply of the Back Numbers, which are rapidly becoming scarce, orders should be sent without delay.—ED.

KNOWLEDGE.—Although we offer our readers more in the way of original matter (apart from correspondence, which is not to be estimated by mere bulk) than any other journal of similar price and character, we wish to do better still. We hope so to extend the circulation of KNOWLEDGE that we may be justified in enlarging each number, in giving more illustrations, and in extending the number of our original contributors. *To attain this end we need the co-operation of our readers.* Those among them who approve our scope and plan can do more to improve KNOWLEDGE than either editor or publishers. If every reader were to obtain but one new subscriber, not only would our circulation be doubled, but our power to improve the matter placed before our readers would be increased in like proportion. If our readers will remember this, they will follow the best course for making KNOWLEDGE what we wish and hope that it may before long become.

Contents of KNOWLEDGE *No.* 5.

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CONTENTS.

RECENT STUDIES OF VOLCANIC ACTION.

By G. F. Rodwell.

Although, during the last few years, no great eruption of a European volcano, similar to that of Vesuvius in 1872, has occurred, and no great earthquake comparable with that which devastated Calabria in 1783, the records of minor volcanic disturbances have been unusually numerous. In 1878 Etna poured out a stream of lava which ran for six miles; a few months before, Hekla threw up a new monticule, from which a quantity of lava issued; Vesuvius more than once since 1872 has furnished enough lava to run into the Atrio del Cavallo: Santorin was active from 1866 to 1870; Stromboli is always more or less energetic; and Volcano has given evidence that its vitality is by no means dormant. Again, within the last year, three disastrous earthquakes have occurred in Europe—at Agram, in Ischia, and in Chios—and many minor shocks have been recorded. In fact, earthquakes are much more common than we generally realise. A year ago (Nov. 28, 1880) an earthquake was reported from no less than fifty different stations in Scotland; some days ago a severe shock was announced from Agram; the previous day from Switzerland. During a few months of 1879, earthquake shocks were felt in such widely-distant places as Florence, Aachen, North Wales, Carinthia, Agram, Athens, and Swizerland. In 1878 there were more than a hundred earthquakes and twelve volcanic eruptions.

For another reason it appears to be a not inappropriate time for reviewing recent European Vulcanology. The literature of the subject has lately received some important additions in the form of such books as Dr. Arnold von Lasaulx's "Der Ætna"; M. Fouqué's "Santorin et ses Éruptions"; the "Studien uber Erdbeben" of Dr. Julius Schmidt; the United States' "Report of the Geology of Utah," by Captain Dutton; the "Minéralogie Micrographique" of MM. Fouquè and Michel Lewy; and the treatise on "Volcanoes" of Professor J. W. Judd, which only within the last few months has issued from the ponderous and iron jaws of the press. The rapid growth of the literature of the subject may be fairly judged of from the fact that Von Lasaulx prints sixteen quarto pages of "Ætna-Literatur."

Vulcanology is a recent science. If any one man can originate a science, Spallanzani must be regarded as the father of vulcanology. There had been many observers before his time, for the great phenomena of nature receive the earnest attention even of unlettered men, but they had observed blindly, and without method. They recorded their observations, but they made no attempt to classify or correlate them, formed no hypothesis to embrace them, and did not follow up the particular line of thought suggested by one or other of them. The value of a good working hypothesis, even in the early stages of a science, cannot be overrated. Moreover, the earlier vulcanologists did not go to work in the right way; they either watched, necessarily at a distance, some grand paroxysmal outburst, or they visited the scene of its action when the main effects had died out. Spallanzani did otherwise. He chose as the source of his observations an ever-active volcano, which never approaches the paroxysmal violence of Etna, Hekla, and Vesuvius during their great eruptions, and never sinks to the quasi-dormant condition which a Volcano commonly presents. Stromboli has been active for more than 2,000 years, and so moderately active, that the scene of its operations can be closely approached, and from a projection which is situated a little above the crater, the observer may sit for hours when the steam and vapours are blown out to sea, and watch the phenomena which are taking place within the crater. Spallanzani did this; and for the first time realised and enunciated the important fact, that volcanic phenomena are mainly, if not entirely, due to the violent escape of steam and other gases at high pressure from molten matter.

In 1874, Professor J. W. Judd, already an accomplished disciple of Scrope, visited Stromboli, and minutely examined, from the vantage-ground erst occupied by Spallanzani, the operations taking place upon the floor of its crater. He divides them into three classes. From certain large apertures in the floor steam escaped in loud, irregular puffs; within, some lava could be seen, which at intervals rose and swelled out, at the same time emitting large volumes of steam; while within the depths of other openings, viscous molten matter was seen to be heaving up and down, and violently agitated, like boiling water. Ever and anon, as the agitation increased, a great bubble of lava would swell out, and suddenly burst, emitting steam at high pressure, the force of which hurled the red-hot scum high into the air. Thus, the three essential conditions for the production of volcanic phenomena appear to be: (1) apertures or fissures affording communication with the interior of the earth; (2) highly-heated matter beneath the surface; and (3) subterranean water which, in the form of high-pressure steam, is competent to produce all the crater operations. A mass of lava within a small active crater precisely resembles a boiling fluid. In one of the lateral cones of Vesuvius the writer once saw a veritable geyser of fire, imitating in all respects its great prototype at Haukadalr. The viscous seething mass of lava within the *bocca* contains water entangled in its mass, and when this rises to the upper part of the column of lava, it is relieved from pressure, and flashes explosively into steam. This restores equilibrium for awhile, during which more steam is being generated, and presently another outburst occurs. The pressure of the steam which thus accumulates may be judged of by the fact that, during the eruption of Vesuvius in 1872, masses of vapour (and, it is said, fragments of scoriæ), were projected to a height of nearly four miles. The friction of this steam against the

rock masses generates enormous quantities of electricity, which appears as flashes of lightning issuing from the column of steam above the crater. The generation of the steam within the molten mass, and its competence to produce all volcanic phenomena, are ably discussed by Prof. Judd from a thoroughly scientific standpoint, and the result of the discussion he gives us in the cardinal generalisation—the keynote of modern vulcanology:—"The varied appearances presented, alike in the grandest and feeblest outbursts, can all be referred to one simple cause, viz., the escape from the midst of molten materials of imprisoned steam or water-gas."

Now the presence of large quantities of water within the recesses of the earth is by no means difficult to account for; but the cause of the internal heat is a far more difficult and yet unsolved problem. Astronomy has taught us that the world is not, as was long believed, a liquid mass, surrounded by a thin solid shell. Sir Humphry Davy endeavoured to account for the heat of volcanoes on the supposition that large quantities of uncombined alkali metals exist in the earth, which, when water finds its way beneath the earth, violently decompose it, generating heat and evolving hydrogen. Mr. Mallet considers that the contraction of the earth's crust can develope enormous quantities of heat. It has been calculated that if a portion of the earth's crust, fifty miles in thickness, were to have its temperature raised 200° Fahr., the surface would be raised by expansion more than a thousand feet. Moreover, crumpled and distorted strata clearly prove that enormous pressures have been exerted by contracting rock-masses. Hopkins imagines that the earth has solidified both at the centre and circumference, while cavities of molten matter are distributed between them. Captain Dutton believes that lava is pressed to the surface by the weight of superincumbent rocks, and he admits that we cannot allow one general reservoir, because lavas of different composition come from the same crater at different periods, and a lower volcanic vent sometimes remains open, while the lava rises and flows from a higher one. Some have assumed that the interior of the earth, although intensely heated, remains solid on account of the enormous pressure, and that the effect of any local diminution of pressure is to lower the fusing point, and thus cause the solid to become liquid. Volcanic phenomena would thus be exhibited at the points of diminished pressure.

But, although we seem to be as far as ever from determining the ultimate cause of volcanic effects, the phenomena themselves, as presented to our observation, are far better understood than heretofore; and this is mainly due to two causes, viz., the establishment of seismological observatories, and the microscopic study of eruptive rocks. Of course, we must add to these the application of more exact and scientific methods, and the advances due to the perfection of such instruments as the spectroscope and recording seismometer. The Vesuvius observatory was the first to be established on anything like a scientific basis, and several valuable volumes of reports have been published by its director, Palmieri. For sixteen years, Tacchini, of Palermo, and Silvestri, of Catania, have urged the erection of an observatory near the summit of Etna, and, thanks to the liberality of the Italian Minister of Agriculture and Commerce, and of the Municipality of Catania, it is now an accomplished fact. In August, 1882, the observatory will be ready for use. It adjoins the site of the old Casa Inglese, nearly 10,000 feet above the sea, and it is in telegraphic communication with an observatory in Catania, 24 ft. above the sea level, and also with observatories in the seven principal towns upon the flanks of the mountain—Aci Reale, Randazzo, Paterno, Aderno, Bronte, Giarre, and Linguaglossa. It will be furnished with all the most approved meteorological and seismological instruments, with spectroscopes, and with a fine refractor by Merz, of Munich, the object glass of which, during the winter months, will be transported to the Catania observatory and mounted in a similar telescope. Owing to the persevering efforts of Prof. M. S. di Rossi, of Rome, who is the editor of the *Bulletino del Vulcanismo Italiano*, seismological observations are now made in more than fifty towns of Italy, and although we could wish that they were better organised and placed under the control of some central Government observatory in Rome, it cannot be denied that the *Bulletino* (now in its seventh year of publication) has placed on record a multitude of observations of high interest to vulcanologists. Observatories are now being instituted in very distant volcanic centres. In January last, Lord Granville forwarded to the Royal Society some valuable "notes on the earthquake of July, 1880, at Manila," by Commander W. B. Pauli, which record the first scientific account of an earthquake in the Philippine Islands. It occurred along the line of the Taal volcanoes, and affected an area 220 miles by 75. The most severe shock lasted for 70 seconds, and combined oscillatory, trepidatory, and rotatory movement. Some very interesting engravings of pendulum curves accompany the paper, which appears in the *Proceedings of the Royal Society* for February, 1881. The curves are of great complexity, and show both the direction and relative intensity of the earthquake shocks, which were sometimes so violent that the pendulum was jerked upwards from the paper, and thus broke the continuity of the curve. Seismological instruments, although far from perfect, have been much improved since the time when the patient student of vulcanology contented himself with watching a bowl of treacle. The electrical seismograph records the direction of the shock, its intensity, and the moment of its occurrence and its cessation. Professor M. S. di Rossi has ingeniously applied the microphone to the detection of the slight subterranean noises which may be heard at any hour of the day or night in some districts. He affirms that both in his observatory in Rome, near the Ara Cœli, and at Rocca di Papa, he has often, while watching the point of a seismic pendulum in a microscope and simultaneously applying the telephone to his ear, heard harsh sounds in the latter at the instant when the pendulum has been seen to quiver.

DREAMS.

PART II.

By Edward Clodd.

KEEPING in mind what has been said about savage mental philosophy, it is not surprising that the inference drawn from the phenomena of dreams is belief in a double existence. Besides that waking self of which the savage is hazily conscious, there must be *another self*, which, roaming the world while the body is at rest, sees and does the things dreamed. Waking, the savage knows, or will be told, that whatever his dreams reveal to the contrary, he has not moved from the place where he lay down; therefore it is that ghost-soul—that *other* self—which has been away on the strange or familiar errand. And such belief in another self—*in* the body, yet at times not *of* it—is confirmed by daily experience. There are the suspensions of consciousness witnessed in swoon, apoplexy, catalepsy, and other forms of insensibility. Then there are the phenomena of shadows and reflection, actual existences to the savage, mocking doubles of himself. The shadow

accompanies, goes before, or follows him by sunlight and by moonshine, disappearing mysteriously only when these are withdrawn or intercepted. Still more complete in its mimicry is the reflection of himself—the image repeating every gesture, while perchance, as he stands shouting by the stream, the echo of his voice is thrown back from the hill-side, and adds confirmation to his notion of duality. How else can man at low stages of thinking, ignorant of the laws that govern the reflection of both sound and light, interpret the shadow and the echo? Hence it is that we find the word for "shadow" chosen to express this other-self in both barbaric and civilised speech, from the dialects of North and South American and African tribes, to the classic and modern languages, as witness the *skia* of the Greeks, the *manes* or *umbra* of the Romans, and the *shade* of our own tongue. Did the limits of a brief paper allow, it would be easy to show, from the evidence of language, how man explained to himself the mode in which this other self makes the passage from the body to the external world, and wherein lay the difference between the sleeping and waking, the living and lifeless body. It must suffice to say that throughout the entire savage and civilized world, the life, the spirit, the soul of man has been identified with *breath*. Not with that alone; but with the blood, the heart, &c., although chiefly and universally with the act of breathing, "so characteristic of the higher animals during life, and coinciding so closely with life in its departure."

It is interesting to watch the primitive nebulous theories of another self, a vaporous, ethereal, or otherwise unsubstantial, impalpable thing, condensing into theories of semi-substantiality, or of rude or refined resemblance to the body, theories which become indispensable to account for the appearance of both the living and the dead in dreams, when their persons were clasped, their forms and faces seen, their voices heard.

Such theories differ not in kind, but only in degree of refinement, and unite, as Dr. Tylor remarks, "in an unbroken line of mental connection," the savage fetish worshipper and the civilised psychologist adding their welcome witness to the similar working of untrained intelligence in different ages among different races on corresponding levels of culture, and therefore, to the underlying unity of our race. This we shall realise only as we realise that the laws of mind, like those of matter, are uniform, and approximately calculable in their operation; the phenomena of one interrelated and interdependent as are the phenomena of the other, and equally the subjects of observation and comparison, if not by identical methods, yet on like principles.

It would be an interesting and informing chapter in the history of the illusions through which man has made continuous, and as yet unaccomplished, passage to the truth, to show how belief in indwelling spirits, of fitful habit and varying form, was enlarged to belief in souls in the lower animals, in plants, and in lifeless things, from stars to stones; how the phantasms of the brain have filled earth, sea, and sky with spirits innumerable, from white-winged celestials to the degraded ghosts of haunted houses. But this would be an undue extension of the subject, for the completeness of which some reference must be made to the part played by dreams as supposed media of communication between gods and men, and as monitions of coming events.

The awe and wonder excited in the savage mind by waving trees and swirling waters, by drifting cloud, whistling wind, and stately march of sun and moon—all invested by him with personal life and will—were immensely quickened by his dreams. In their unrelated and bewildering incidents, the powers indwelling in all things around him seemed to come nearer than in the more monotonous events of the day, uttering their warnings and conveying their messages. There needed but slender data to reach conclusions. Let the death of a friend be dreamt of, and the event follow; or a hunting-feast fill the half torpid fancy, and a day's privation give the lie to the dream; the arbitrary relation is made. Lord Bacon says:—"Men mark the hits, but not the misses," and a thousand dreams unfulfilled count as nothing against one dream fulfilled. Out of that a canon of interpretation is framed by which whole races of men will explain their dreams, never staying to wonder that the correspondences are not more frequent and minute than they really are.

"To this delusion," says Cornelius Agrippa, an ancient rationalist, "not a few great philosophers have given a little credit . . . so far building upon examples of dreams, which some accident hath made to be true, that thence they endeavour to persuade men that there are no dreams but what are real." When Homer says that "dreams, too, from Jove proceed,"* painting the vividness and agonising incompleteness of those passing visions; when Tertullian says that "we receive dreams from God, there being no man so foolish as never to have known any dreams come true," both classic and patristic opinion are clearly survivals from the lower culture, its lineal and thinly-disguised descendants. For the savage, the bard, and the theologian lived in days when the conception of orderly sequence was unthinkable to them; where the arbitrary act was wrought, the isolated or the conflicting influence manifest, there the deity or the devil was present; while for us, could we discover where law is not, thence God would seem to have withdrawn.

The passage from the crude interpretation of his dreams by the savage to the formal elaboration of the dream-oracle is obvious, the more so as this latter was only one of many modes by which it was sought to divine the will of heaven, and read that "book of fate" hidden from men. This dream-lore, as ancient records far back to Accadian times show, not only called into existence a class of men whose position as interpreters of royal and other dreams ensured them commanding place, but gave rise to a mass of literature most prolific in classic times. It maintained an almost canonical supremacy down to the Middle Ages, finding its befitting level in our day in the "Libri dei Sogni" which the Italian lottery-gambler consults, and in the "Imperial Dream-Book" by which the English domestic forecasts whether King Cophetua or Police-sergeant X 32 is to be her fate!

At this nether depth, Science, content with having shown the persistence of primitive modes of thinking in all subsequent interpretation of his own nature by man; finding its evidence and the warrant of its conclusions in that human experience which the sources of our knowledge cannot transcend; may well let the matter rest. It need not concern itself with denials that dreams have been sent as warnings from Heaven to man; this were as foolish as to take pains to disprove the existence of ghosts, or to seriously challenge the predictions in Zadkiel's *Vox Stellarum*. Science need not argue; it explains; and to such matters explanation is death. For the changes which revelation of the order of nature and the establishment of that doctrine of continuity, which has no "favoured-nation" clause for man, involve, will bring about, in quiet and unmourned, the departure of belief in dreams as omens or warnings, just as they have brought about the decay of belief in witchcraft and astrology.

* "Iliad," Book I., 77.

BETTING AND MATHEMATICS.

By the Editor.

When I was travelling in Australasia, I saw a good deal of a class of men with which, in this country, only betting men are likely to come much in contact—bookmakers, or men who make a profession of betting. What struck me most, perhaps, at first was, that they regarded their business as a distinct profession. Just as a man would say in England, I am a lawyer or a doctor, so these men would say that they were bookmakers. Yet, on consideration, I saw that there was nothing altogether novel in this. Others, whose business really is to gain money by making use of the weaknesses of their fellow-men, have not scrupled to call their employment a trade or a profession. Madame Rachel might have even raised her special occupation to the dignity of "a mystery" on Shakespearean grounds ("Painting, sir, I have heard say is a mystery, and members of my occupation using painting, do prove my occupation a mystery"); and if aught of wrong in his employment could be made out to the satisfaction of a bookmaker, his answer might be Shakespearean also, "Other sorts offend as well as we—ay, and bettor, too."

My own views about betting and bookmaking are regarded by many as unduly harsh, though I have admitted that the immorality which I find in betting has no existence with those who have not weighed the considerations on which a just opinion is based. I regard betting as essentially immoral so soon as its true nature is recognised. When a wager is made, and when after it has been lost and won its conditions are fulfilled, money has passed from one person to another without any "work done" by which society is benefited. The feeling underlying the transaction has been greed of gain, however disguised as merely strong advocacy of some opinion—an opinion, perhaps, as to whether some horse will run a certain distance faster than another, whether certain dice will show a greater or less number of points, or the like. If here and there some few are to be found so strangely constituted mentally as really to take interest in having correct opinions on such matters, they are so few that they do not affect the general conclusion. They may bet to show they really think in such and such a way, and not to win money; but the great majority of betting men, professional (save the mark) or otherwise, want to win money (which is right enough), and to win money without working or doing some good for it, which is essentially immoral. That in a very large proportion of cases this negative immorality assumes a positive form—men trying to make unfair wagers (by betting with unfair knowledge of the real chances)—no one acquainted with the betting world, no one who reads a sporting paper, no one even who reads the sporting columns of the daily papers, can fail to see. Why, if half the assurances of the various sporting prophets were trustworthy, betting, assisted by their instructions, would be as dishonourable as gambling with marked cards, as dishonest as picking pockets. Here is my "Vaticinator,"* the betting man might say, who says that Roguery is almost sure to win the "Beggar my Neighbour" stakes, but if he does not, that speedy mare, Rascality, will unquestionably win. Here are the bookmakers, who seem all quite as ready to lay the odds against Roguery and Rascality as against any of the other horses, to say nothing of my friends, Verdant and Flathead, who will freely back any of these latter. Now, if I back Roguery and Rascality with the bookmakers, and lay odds against the certain losers in the race, I shall certainly win all round. Of course, "Vaticinator" is not the prophet he claims to be, but the betting-man of our soliloquy supposes that he is, and so far as the morality of the course the latter follows is concerned, the case is the same as though "Vaticinator's" prophecies were gospel. There is not a particle of real distinction between what the bettor wants to do, and what a gambler, with cogged dice or marked cards, actually does. The more knowing a betting man claims to be, the easier it is to see that he wants and expects to take unfair advantage of other men. Either he knows more than those he bets with about the real conditions of the race or contest on which they wager, or he does not. If he does, he wagers with them unfairly, and might as well pick their pockets. If he does not, but fancies he does, he is as dishonest in intention as he is in the former case in reality. If he does not, and knows he does not, he simply lies in claiming to know more than he does. In claiming to be knowing, he really claims to be dishonest and (which is not quite the same thing) dishonourable; and probably his claim is just. Of course, this is only a comparatively mild case. Men have been known to take the odds against a horse after they knew certainly that the horse would not run. Others, a shade more advanced, have been known to bribe a jockey to "hold," or "rope" a horse, or a stableman to poison or stupefy him. Owners—aye, even "noble" owners—have been known to work the market in ways fully as flagitious. Every one agrees about these. But the majority are disposed to stare, and perhaps to sneer, when Herbert Spencer describes ordinary, and what is commonly called fair gambling, as immoral; and the calmness with which a betting man claims to be knowing, shows that he, at any rate, does not think wagering with unfair knowledge (with any knowledge, I suppose, short of absolute certainty) dishonourable. He argues, and many who do not bet argue for him, that he takes his chance with others; as if it might not quite as justly be argued that the pickpocket takes his chance between a successful transaction and the prison cell.

As one of our "Five of Clubs," I gave last week a case in which a certain man of title used to offer freely—and possibly with a sense that he was acting quite fairly—a most unfair wager, though it seemed a very generous one. Odds of a thousand pounds to one are very tempting to the inexperienced. "I risk my pound," such a one will say, "but no more, and I may win a thousand." That is the *chance*; and what is the *certainty?* The certainty is that in the long run such bets will involve a loss of £1,828 for each thousand pounds gained, or a net loss of £828. As certain to all intents as that two and two make four, a large number of wagers made on this plan would mean for the clever layer of the odds a very large gain. Yet Lord Yarborough would probably have been indignant to a degree if he had been told that in taking £1 for each hand on which he wagered which did not prove to be a "Yarborough," he was in truth defrauding the holder of the hand of 9s. 0¾d.—notwithstanding the preliminary agreement, simply because the preliminary agreement was an unfair one. As to his being told that even if he had wagered £1,828 against £1 the transaction would have been intrinsically immoral, doubtless he and his opponent would equally have scouted the idea.

A curious instance of the loss of all sense of honour, or even honesty, which betting begets, occurred to me when I was in New Zealand. A bookmaker ("by profession," as

* I hope there is no turf prophet with this *nom-de-plume*. I know of none, or I would not use the name; but it may have been hit upon by some sporting man with a taste for polysyllables.

he said), as genial and good-natured a man as one would care to meet, and with a strong sense of right and justice outside betting, had learned somehow that ten horses can come in (apart from dead heats) in 3,628,800 different ways. This curious piece of information seemed to him an admirable way of gaining money from the inexperienced. So he began to wager about it, endeavouring—though, as will be seen, he failed—to win money by wagering on a certainty. Unfortunately, he came early across a man as cute as himself, and a shade cuter (*à brigand brigand et demi*), who worded the question on which the wager turns, thus: "In how many ways can ten horses be placed?" Of course, this is a very different thing. Only the first three horses can be placed, and the sets of three which can be made out of ten horses number only 10 times 9 times 8, or 720 (there are only 120 actual sets of three, but each set can be placed in six different ways). My genial, but (whatever he thought himself) not quite honest friend, submitted the matter to me. Not noticing, at first, the technical use of the word "placed," I told him there were 3,628,800 different arrangements, he rejoiced as though the money wagered were already in his pocket. When this was corrected, and I told him his opponent had certainly won, as the question would be understood by betting men, he was at first depressed; but presently recovering, he said, "Ah, well; I shall win more out of this little trick, now I see through it, than I lose this time."

I shall hereafter give some illustrations of the true principles on which all chance questions should be determined. There is no hope that men generally will give up gambling, but it is, at any rate, desirable that when they gamble, the chances should be as equal as they can be made; that, in fact, they should not play (as the opponents of Lord Yarborough and my New Zealand friend were certainly playing) against cogged dice or marked cards. The matter is one strictly appertaining to the subjects with which Knowledge claims to deal. There is science in chance, certainty in probabilities. What is thus scientific and certain is what we propose to bring before our readers.

OUR UNBIDDEN GUESTS.

By Dr. Andrew Wilson, F.R.S.E.

The fact that in most animals there may reside, as "guests," within unconscious or unwilling "hosts," certain other animal forms, is, of course, widely known. These animal "guests" form the "parasites" of the natural historian. But, although the fact of their existence is known, the general history of even the commonest parasites is a matter concerning which the general public are, as a rule, lamentably ignorant. I say "lamentably," and I mean what I say. A vast amount of disease, and that of a preventible nature, is caused by the carelessness of man in the preparation of his food. This carelessness is in its turn founded upon gross ignorance, for there are not a few persons who believe that parasites come, like Dogberry's reading and writing, by nature, and that they are part and parcel of an animal's constitution. That this opinion is very far removed from the true state of matters can easily be shown. It is perfectly provable that animals were not created with the parasites infesting them as we find them to-day. Common-sense forbids such a supposition, and the organised common-sense we call "science" shows us that the reverse is the case. All parasites are acquired, and not original "guests." This alone is provable by the facts of parasite-development. There is a bag-like parasite called *Sacculina*, for instance, which attaches itself to the bodies of hermit crabs. Now, sac-like though this parasite is, and destitute as it is of all the ordinary belongings of animal life, it yet begins its existence as a little free-swimming animal, exactly resembling a water-flea. The first stages in a sacculina's development are, in short, like the beginnings of the development of some shrimps, of barnacles, of water-fleas, and of crabs themselves, though in a less marked degree. Only after becoming degraded in structure does the sacculina become the "guest" of the crab. The mere facts that sacculina is at first as free-living as a fish, and that it afterwards settles down on the crab, testify, if we read nature's story aright, that "once upon a time" the sacculina race was not a parasitic one. Whether or not the sacculina-stage itself was the beginning of the attached existence, we do not know. It is most probable that the bag-like body we term a "sacculina" was the result of the adoption of the lower and rooted way of life. But, apart from all other considerations, the main facts that a young sacculina is always free, and that it begins life under a similar guise even to some of the shrimp race, shows that its parasitic life has been acquired, and is by no means an original condition.

Now the same rule holds good of all "parasites." The development of most of them shows us the lingering remnants of a once-free life. But there are other proofs at hand of this assertion. There are degrees and stages in the perfection of the parasitic state. There exist animals which are mere "lodgers," so to speak—who "dine out," but who repose within the anatomical establishment of a "host." This is the case with certain little fishes, which choose the very "jaws of the lion" as a dwelling-place, since they appear to live in the interior of certain big, tropical sea-anemones. These fishes may be seen to swim in and out of the anemone's mouth, and they may be enclosed within the anemone's body when that animal contracts itself, and yet swim free and unharmed out of the mouth when these flower-like animals once more resume their normal and expanded state. Here, then, there is mere "association," but it is in some such association that the beginnings of pure parasitism have orginated. Suppose the case of an animal which, at first merely "lodger," took to feeding upon the tit-bits secured by its host for home-consumption. The "lodger," in such a case, would practically become a "boarder" as well. But nature has a law as fixed as the edicts of the Medes and Persians, called the "law of disuse." This law enacts that whatever structures or organs of living beings are not normally used, will waste and tend to disappear. It is the operation of this law which has caused the two outer toes of our horse to grow "small by degrees and beautifully less," until they now appear as the "splint bones" on each side of the single toe upon which the horse walks. And applying this law to the case of the animal lodger, we see how an animal which does not require to move about when resident within another animal will lose its organs of motion. If it obtains fluid food, all ready digested, the probabilities are its digestive system will become rudimentary. Not requiring eyes or other sense-organs, these will disappear; and thus we see represented a kind of zoological backsliding, which reduces the parasite to the elementary and degraded condition we, as a rule, discover in the races of animal "guests."

The histories of some of the most common parasites are fraught with instruction, not to speak of the curiosity that invests them. Take, for instance, the history of the fluke (*Fasciola hepatica*), found in the bile-ducts of the liver of the sheep and ox. It is the presence of this parasite that makes

sheep-rot, a disease known to veterinarians as the "rot." A fluke is a little, flattened, oval body, about 1 in. or $\frac{3}{4}$ in. in length and about $\frac{1}{2}$ in. in breadth. It possesses a nervous system, a set of water vessels, two suckers, a branched digestive system, and an egg-producing apparatus. It has no organs of motion, but it is by no means a very degraded being after all. Its development is very curious. The eggs, liberated from the animal "host," get scattered abroad. Many—as in the case of all parasites—must perish, but a proportion finding their way into water, enter the body of the water-snail, where they develop into curious little tailed beings called *Cercariæ*; and there are sundry other forms assumed by the fluke in the days of its youth, but which need not be mentioned here. Sooner or later, however, these *Cercariæ* escape into the water or into the meadows; and it is believed that from the damp meadows, or from the water itself, the sheep obtains these little beings. Once within the sheep's stomach, each *Cercaria* seems to waken up to its ultimate destiny. It drops its tail, and bores its way through the tissues of the sheep towards the liver, where it soon appears as the young fluke, which will develope eggs that will repeat its own curious history. The most notable fact, however, of this development is that if a sheep swallowed the *egg* of the fluke, no development would ensue. The egg requires to pass through its water-snail stage, ere the sheep can obtain the new fluke.

STATISTICS OF SUICIDE.*

In these days, when the question is asked, as a not unreasonable comment on the phenomena of social life, "Is life worth living," the statistics of self-slaughter have an exceptional value. Suicide could not be regarded as a subject of scientific investigation at all, until after statistical researches had been made. As Professor Morselli says, the character which classical paganism attributed to suicide was simply individual. The famous phrase of the Stoics, "Mori licet cui vivere non placet" (he whom life pleases not has a free right to die), was the product of ancient philosophical individualism. Law and religion alike declare suicide criminal. But as yet the crime has not been considered as a tendency—hurtful, unquestionably, to society, but connected with society's natural development. It is this aspect of suicide which Professor Morselli discusses in the book before us. "The old philosophy of individualism," he remarks, "had given to suicide the character of liberty and spontaneity, but now it has become necessary to study it no longer as the expression of individual and independent faculties, but as a social phenomenon allied with all other racial forces."

At the outset, we may notice that if there is one thing which would render the statistics of suicide pre-eminently valuable, and if there is one thing which Dr. Morselli seems to regard as little worthy of discussion, it is the recognition of the motives which lead to suicide (and rather in their psychical than their social aspect). Can we, or can we not, from the statistics of suicide, determine the motives most potent to drive men to self-slaughter? By studying the statistics of times and seasons we may determine the physical condition which best favours the influence of such and such motives. When we find that the hot, bright months of summer are those in which the suicidal tendency prevails most, we seem to recognise physical, rather than psychical influences; or, to speak plainly, we seem to see that the state of the body, rather than that of the mind, is important, so far as this special peculiarity is concerned. When we learn that suicide is more common in the daytime than at night, and that there are three hours of maximum suicidal tendency—viz., about 8 a.m., about noon, and about 3 p.m.—we recognise the influence of social relations; as we do again when we notice the greater number of suicides on Monday, Tuesday, Wednesday, and Thursday, as compared with those on Friday, Saturday, and Sunday (except among women, with whom Sunday is the favourite day for suicide). But in these statistics the influence of motive is not readily to be recognised.

The case is somewhat different when we consider the influence of marriage on suicide. Here it comes out very clearly, as we might naturally expect, that responsibility has its influence even on those so weak that, but for some such restraining influence, they would "shuffle off this mortal coil." When in one and the same nation we find that the number of suicides among married men is far less than among single men, we may be in doubt how far the difference is due to difference of motive, or to the different stuff of which (taking the average) the ranks of the married and of the unmarried are made. It might be that the men who are either less likely to be selected as desirable mates, or who are averse to marriage, are more likely antecedently to be life-weary. But we can have no such doubts when we see that widowers without children are nearly twice as likely to commit suicide as widowers with children. It seems almost certain here that the restraining influence is the sense of responsibility. As Douglas Jerrold, when the doctors pronounced his death-warrant, looked round at wife and children, and said, "I will *not* die"; so we may well believe that even the weakest among men, who, on his own account, sees nothing that makes "life worth living," will yet feel that he cannot die and leave his little ones without a protector, or, it may be, in want. He feels that though his life may be worthless to himself, it is worth something still for them, and haply he may find later that in its worth to them it has had a worth to himself also.

We take it, indeed, that ninety-nine self-slayers out of a hundred show by their act that they feel their life to be worthless not only to themselves but to everyone. Suicide is, to all intents and purposes, an admission of utter worthlessness. Even in cases where a sort of halo of romance or heroism has surrounded the act, this has been so (we except, of course, all cases in which suicide has meant self-sacrifice—that is, no more self-murder than homicide in self-defence or defence of others is murder or even manslaughter). The lover who kills himself or herself because rejected or slighted, admits inferiority, if not worthlessness, as certainly as the man who kills himself because he has failed in the struggle of life. The Cato who kills himself rather than yield to his country's enemies, admits as certainly that his best has proved a failure, as the man who takes away his own life because he fears poverty or misery. Thus viewing suicide, one might, at a venture, predict that, other things being equal, suicide would be most frequent among that set of men who most feared contempt. Other things are not equal; but it does so happen that the proportion of suicides is greater in Paris (and the *Ile de France* generally) than anywhere else in Europe, four times greater than in London and the south-eastern parts of England, and nearly ten times greater than among the self-contented people of Southern Holland.

When we take employments, we find a similar lesson, though we cannot be quite so sure of our interpretation.

* "Suicide: An Essay on Comparative Moral Statistics." By Henry Morselli, M.D., Professor of Psychological Medicine, &c. (London: C. Kegan Paul & Co., 1881.) Price, 5s.

Men of art, science, and literature, with their keen susceptibilities, and their tendency to watch with anxiety, if not with envy, the success of their fellow-workers, stand at the head of the professions and trades in suicidal tendencies, despite the enormous increase to the value of life which the study of art, science, and literature brings with it. Military men come next, in Italy at least (with 404 to the million of their class, as compared with 618 to the million among literary men). But this may be due to other causes, as Dr. Morselli notes. "Military life" (in Italy), he says, "has the misfortune of increasing the loss of active and vigorous elements by means of unhappy sacrifice to suicide; whether that is owing to distance from home and disgust for military life, or to the severity of discipline, this is not the place to discuss."

Dr. Morselli's work is full of curious statistics, singularly ill-arranged, and at times rendered almost unmeaning for want of fuller information, or by the badness of the translation, yet well worth careful study. The conclusion to which he comes is melancholy. The cure of the suicidal tendency is indicated, he says, in one precept: "*To develope in man the power of well-ordering sentiments and ideas by which to reach a certain aim in life; in short, to give force and energy to the moral character.*" This amounts, in fact, to saying that, since the weak and idle are more apt to commit suicide than the strong and active, it is necessary to become strong and energetic in order to avoid the suicidal tendency. *But how?* "Intemperance and dissoluteness are powerful causes of weakness, and consequently of suicide." Therefore we must avoid intemperance and dissoluteness. But may it not with as much truth be said that weakness is a cause of intemperance and dissoluteness? We are no nearer the *causa causarum*—no nearer than was Hamlet when he reasoned how

> Oft it chances in particular men,
> That for some vicious mole of Nature in them,
> As in their birth (wherein they are not guilty,
> Since nature cannot choose his origin)
> By their [query "the"] off-growth of some complexion,
> Oft breaking down the pales and forts of reason
> Their virtues else . . . . .
> Shall in the general censure take corruption
> From that particular fault.

The true cure, it seems to us, is from without, not from within. Show a man that his life need not be a useless one; give him some worthy end to achieve, and his mind will be turned from the idle question: "Is life worth living?" Life is always worth living when any good work remains to be done.

THE DESTROYED COMET.

By the Editor.

BEFORE considering the theory of repulsion as applied to interpret the phenomena of comets' tails, it may be well to consider a case in which some active force (other than gravity), exerted by the sun, seems to have wrought the destruction of a comet, or, at least, to have broken up the comet into unrecognisable fragments.

No comet ever observed has exhibited phenomena more remarkable than those displayed by the comet known as Biela's (more properly called Gambart's). We wish we could agree with a modern astronomer, who has said that no comet has thrown more light on the nature of these bodies; but, in point of fact, it is only promise of light, not light itself, that we have obtained.

Discovered in 1826, Biela's comet was presently found to be identical with one seen in 1772 by Montaigne, and again by Pons in 1805. A careful study of the observations showed that the comet travels round the sun in a period of about $6\frac{2}{3}$ years, or, roughly, thrice in twenty years. Its path was found to approach very near to the path of our earth. The comet returned in 1832, when the ignorant were scared much as they have been recently by the threatened influence of the larger planets in perihelion. The comet crossed the earth's track several weeks before she herself came to the place where the two orbits approach nearest, and it is hardly necessary to say that the comet's passage did not injure the earth's roadway in any appreciable degree.

Fig. 1.—Biela's Comet in 1846, before its division into two.

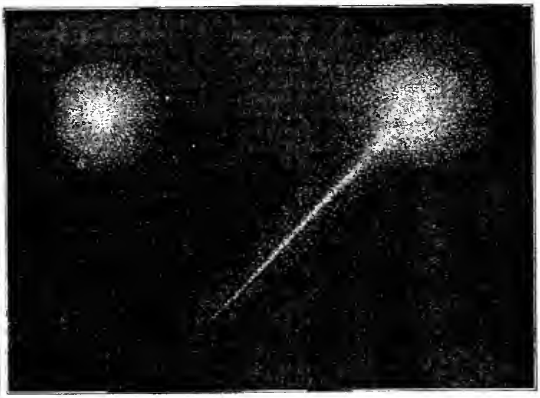


Fig. 2.—Biela's Comet on January 15, after its division into two.

In 1839 the comet returned, but was not seen, travelling across a part of the heavens only above the horizon in the day-time, so that the comet's light was hidden by the sun's.

It was at the next return in 1845-46 that the comet first attracted special attention. On that occasion, instead of behaving as comets usually do, Biela's, which in the first days of 1846 had presented the appearance shown in Fig. 1, was found to have divided into two. There is some little doubt as to the time when the comet underwent division. Lieut. Maury reported on January 15 that he had seen the

comet double on January 13; but Wichmann observed it as a single comet on the 16th. But Professor Challis, in his account of his own observations on the comets, states that even on January 15 the second comet might easily have been overlooked. M. Valz saw nothing unusual on the 18th and 20th; but on the 27th: "I was struck with amazement," he says, "to find two nebulosities, separated by an interval of two minutes of arc, instead of one nebulosity alone. . . . Each head was followed by a short tail, whose direction was perpendicular to the line joining the two nebulosities." Earlier, only the larger comet had had a tail, the appearance presented by the double comet being that shown in Fig. 2.

The two comets travelled along, side by side, until at last both passed out of view, at which time the distance between them amounted to about 157,000 miles.

In 1852 both comets returned. Sir John Herschel says, in his "Familiar Lectures on Scientific Subjects," that when they returned, the distance between them was unchanged. This, however, was a mistake. The distance now amounted to about $1\frac{1}{4}$ millions of miles. Again they passed before the interested gaze of astronomers, travelling side by side, though rather far apart, until finally they disappeared from view—we say finally, for neither has ever been seen again.

Whether the two comets returned in 1859 is doubtful. It is certain that if they did, they would have been invisible, for the same reason that the comet was invisible when it returned in 1839.

But in 1866 the double comet should have been well seen. It should be remembered that each return of a comet of short period (like that which our correspondent Mr. F. Denning, of Bristol, discovered this year) gives the astronomer more perfect mastery of the comet's motions. The return could be predicted with sufficient accuracy in 1832 to cause the comet to be easily redetected. The next visible return might have involved a difficulty, because the comet had in the interval made two circuits. But that return was successfully predicted. The return in 1845-46 was still more accurately calculated. Nor did the breaking up of the comet into two on that occasion interfere with the successful calculation of the return in 1852. The case may be compared to the rating of a clock, which is more satisfactorily effected in a week than in a day, for the simple reason that any error of observation is spread in one case over seven times as long a period as in the other, and therefore affects the estimate of any given circuit of the hands by an error only one-seventh as large. Just so, whatever error an astronomer might make in observing Biela's comet in, say, 1843, was distributed over all the revolutions of the comet which had taken place since 1826 (one might almost say since 1772), and in a correspondingly small degree affected the astronomer's estimates of the comet's motion during any single revolution. This being so, astronomers had good reason for believing that in 1866 Biela's comet would return. When the time came that it should have been visible, telescopes were turned towards the spot where it should have been seen. Night after night from that time its calculated track was swept with the finest telescopes in Europe and America. But no trace of the comet could be seen. "It is now," wrote Sir John Herschel, in February, 1866, "overdue. Its orbit has been recomputed, and an ephemeris calculated. Astronomers have been eagerly looking out for its reappearance for the last two months, when, according to all former experience, it ought to have been conspicuously visible, but without success! giving rise to the strangest surmises. At all events, it seems to have fairly disappeared, and that without any such excuse as in the case of Lexell's—the preponderant attraction of some great planet. Can it have come into contact, or exceedingly close approach to some asteroid as yet undiscovered; or, peradventure, plunged into and got bewildered among the ring of meteorolites, which astronomers more than suspect?"

Be the cause what it might, the comet was not seen in 1866. In 1872 it was looked for even more carefully. Every possible contingency depending on planetary perturbations was considered: and the telescopes of astronomers swept, not only the calculated path, but to a considerable distance on either side of it. No trace of the comet was seen, however, in 1872 any more than in 1866. So far as telescopic observation is concerned, Biela's comet seems to have come to the end of its career as a comet.

Yet the observations of 1852 were not the last which were made on this interesting object. It has been seen again, though not as a comet. Nay, the occasion on which it was seen in the way referred to was predicted, and the prediction fulfilled, even in details. We shall return to the consideration of this remarkable apparition of the comet in changed form—a form which but a quarter of a century ago no one would have thought of associating in any way with the long-tailed star whose approach had been regarded as heralding some great change in the fortunes of men and nations.

TOAD IN A HOLE.

By Dr. A. Wilson, F.R.S.E.

In letter 87, "Arachnida" asks, "What is the construction of the common toad that enables it to be enclosed for many years in blocks of solid matter?" "Arachnida" should first of all have asked, is it true that toads are ever found so enclosed? The usual story is that of some quarrymen, who, blasting stones, see a live frog or toad hopping about, after the blast, among the *débris*. Because the toad is found thus, it is *assumed* that it came from the interior of the rock. Not a particle of evidence exists to show in such a case that the animal had anything whatever to do with the rock. If "Arachnida" will read in the "English Cyclopædia" the account of Dean Buckland's experiments, he will find that the Dean enclosed healthy frogs and toads in holes cut in limestone and sandstone blocks. He buried the blocks in his garden three feet deep. At the end of the first year most had died, and the living ones, re-buried, all died long before the end of the second year. Common sense, apart from exact knowledge, would tell us that animal life of higher kind, with all its demands in the way of food, &c., could not exist under the circumstances of the popular tales and superstitions "Arachnida" inquires about. The oldest fossil toads and frogs occur in *Tertiary* rocks. If, therefore, a live toad hopped, as has been alleged, out of a *Cretaceous* or *Devonian* rock, such a fact would amount to the declaration that the live toad could be ages and ages older than its fossil relatives, which declaration is, of course, the height of absurdity. There is no doubt that a frog or toad has an elastic constitution. It is cold-blooded: it can live under water for months; it can live for months after excision of its lungs, because the skin takes on the functions of lungs in such a case: and these animals (as in Dean Buckland's experiments) can live without food for a year or two, but, like all other animals (and plants), die *starved and meagre*, sooner or later. If "Arachnida" will only take the trouble to inquire into the *evidence* on which such stories as those he mentions are founded, he will find not one single *proved* or *provable* fact which will warrant any belief in the utterly impossible existence of toads or frogs in rocks. I may refer him for a fuller account of such cases to the essay on "Some Facts and Fictions of Zoology" in my "Leisure Time Studies" (Chatto & Windus). As a naturalist, I stake my reputation on the correctness of the views stated above, and also repeated in my book.

EASY LESSONS IN BLOWPIPE CHEMISTRY.

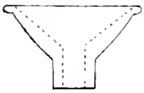
By Lieutenant-Colonel W. A. Ross, late R.A.

Introduction.

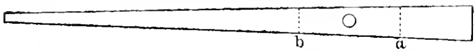
I FEEL some difficulty in expressing the satisfaction experienced by myself, in common, no doubt, with thousands of others, at seeing what has been for so long the great moral and intellectual *desideratum* of London—a *cheap* and simply-written, but at the same time thoroughly scientific magazine—so well supplied as it promises to be by Knowledge; and I beg, therefore, to offer you my humble efforts towards the accomplishment of this praiseworthy object. First, if I have your permission, in the simple exposition of a scientific subject which has attracted my most ardent attention, and extracted every leisure moment for its study, during the last twenty-two years of my life; secondly, by getting all the working people I can in my neighbourhood to spend their spare twopences weekly in the acquirement of Knowledge in the way of literature, instead of (as most do at present) in the purchase of trashy penny and halfpenny novels, boiled-down Scott, police *réchauffées*, &c., &c.

Lesson I.—How to Make a Blowpipe and Use It (Chemically).

Many English working young men already know how to use a common mouth-blowpipe in soldering and goldsmithing, &c., so that I shall have less difficulty in explaining how very little more trouble or pains are required in using it chemically. And first, as to making a blowpipe. We all know that a common conical, or "Black's blowpipe" (invented by the celebrated Dr. Black, of Edinburgh), made of tinned iron, with its brass nozzle, can be purchased for 9d.; but I would strongly advise every investor in such nine pennyworth, to add to his blowpipe a *trumpet-mouthpiece* (invented by the German chemist, Plattner), which any wood-turner will make of some hard-seasoned wood for 2d. more. If, however, my young friend is determined to make a blowpipe for himself, I will now show him how to make a far more powerful instrument than the above, and one, also, which does not require *any* art in blowing. A child can use it without being taught; indeed, my little boy (age 7 years) has already done so.



First, he must procure about a foot, in 2 in. pieces, of brass tubing, half of which should be 7-10 in. in diameter, and the other half *a little* smaller, so as to draw in and out of the former like a telescope, and yet fit air-tight. He must now solder a round piece of brass into one end of the wider tube, so as to close that for ever. These tubes are called the "Body-tubes" of the blowpipe. He can then make *the jet* of a small brass tube, tapering to a point, this form *and size*. Two holes are now to be correctly bored through



the sides of the wider body-tube above mentioned, about ½ in. from its closed end, so that this jet shall pass into them, closing air-tightly the larger hole at *a* and the smaller at *b*, which places are exactly 7-10 in. apart in the figure. A smaller hole like that in the figure is to be bored through one side of the jet, between *a* and *b*. A brass *nozzle*, this shape and size, having a bore the width of a good stout sewing-needle, is to be fitted (but on no account screwed) on the tapering end of the jet. A child's indiarubber air-balloon, of the smallest size procurable, is tied on the thick end of the jet, with waxed silk-thread, and this part of my young friend's blowpipe is complete. To complete the whole, all he has now to do is to procure a wooden trumpet-mouthpiece to fit air-tightly into one end of the smaller of the body-tubes above described; but before fitting it, he must fasten over the inner end of its bore a strip of oiled-silk cloth, rather loosely, so that air blown upon the silk from the outside shall pass into the tube, but cannot pass *back again* into the mouth. This silk, in fact, forms a simple *valve*.

The young "pyrologist," or blowpipe-chemist, now possesses a blowpipe which, with proper "re-agents,"—cheap chemical substances with which to heat and treat his minerals, &c.—will enable him, after he has been through a course of these lessons, to "analyse"—that is, take to (chemical) pieces, so as to show what they are composed of—the "rocks" of the geologist or the "stones" of the mineralogist, belonging to a whole continent, if necessary (and if a decent time is allowed him for the purpose).

The Editor will explain far better than I can, that everyone's eyes see objects (small print, for instance) best at a certain distance. This point is called their "focus," and this distance their "focal distance." Most people's eyes have not only a different focal distance, but the focal distance of the same pair of eyes is apt (unhappily!) to alter through age; also in consequence of sickness or accident. The telescopic form of this blowpipe (which was invented by me about a year ago) enables each operator to draw it out or shut it up to the length suitable to the focus of his eyes. In packing, he draws out the jet, and, folding its air-bag as closely as possible, slips the whole into the smaller body-tube; that, with its contents, is passed into the wider body-tube, and the whole shut up, telescope fashion, forming a single short tube which can be carried in the waistcoat pocket.

In using the ordinary blowpipe, a little art is at first necessary, which generally requires from one to three days' practice, according to the capability of the learner. The best way to acquire the requisite proficiency I believe to be for the student to sit down with a lighted candle, having the wick bent to one side, on a table before him; to take his blowpipe in the right hand, not gingerly between three fingers, as some writers recommend, but firmly grasped in the fist; to apply the mouth-piece to his mouth with his cheeks puffed out, and elbow on table; to thus try to produce from the candle-flame, a "pyrocone," which is simply a cone of *blue* flame, breathing through his nose all the time. He will find this quite easy, but after a time, the air, or rather breath, in his cheeks will begin to fail, when all he has to do is to pronounce the word *cow* without opening his mouth. The effort to do this will replenish his cheeks with air.

HINTS TO LOCAL METEOROLOGICAL OBSERVERS.

(Reprinted, with the Author's permission, from *Science Gossip*.)

I. Hailstorms.

AS hailstorms are essentially local phenomena, it is to local observers that we must look for any addition to our knowledge of them.

A reference to any Encyclopaedia article will show the sadly confused state of our information on the subject. Flat contradictions will be found on every page. It will be seen that while some authorities assert that hailstorms occur most frequently in summer, others maintain that winter is pre-eminently the hail season. There must be an error somewhere. As a matter of fact, false statements have been so persistently reiterated, that by mere repetition they have come to be regarded as truths.

Let me allude to one fertile source of error. I have referred to it before.* It is the confusion of hail proper, or French *grêle*, and winter hail, or *grésil*—two entirely different phenomena. *Grésil* is the small round powdery snow which often falls towards the end of a snowstorm and in the early part of a very frosty night. I suspect that Dalton and other observers did not distinguish between the two kinds of hail, and spoiled their results in consequence.

The following points are of primary importance, and should not be overlooked in taking down an account of a hailstorm:—

1. *Period.*—The date and hour at which a hailstorm occurs, though apparently small matters, are of the utmost importance. I am inclined to think that while each country has distinct maximum and minimum periods of hail-fall, the distribution of the quantity over the year varies materially. For example, in this country, the maximum occurs in summer, and is very strongly marked; while in Germany, the maximum is in spring, and is not so decided. If the periods of maxima and minima all over the world could be determined, a comparison of them would doubtless throw much light upon the nature and cause of hailstones.

2. *Area.*—When a hailstorm is not purely local, it usually assumes the form of the tornado, and sweeps over the country in one, or sometimes two, narrow bands. In the former case, it is sufficient to ascertain the area covered by the storm, but in the latter, it is necessary to determine (*a*) the length, (*b*) breadth, (*c*) direction of motion, and (*d*) rate of progression of the storm-band. Such particulars can only be obtained by the co-operation of numerous local observers. Our organised observatories are quite incompetent for such work, being too sparsely distributed over the country.

3. *Physical features of the locality.*—The proximity of mountains seem to induce the fall of hail, while that of forests has the opposite effect. Progressive storms often diverge from their course on encountering a river or valley, and follow that of the depression. It is therefore useful to note if any of those physical features be in the neighbourhood of the storm, and if so, their effect upon it. It is also of importance, especially in the tropics, to determine the elevation of the country above the sea level. It is frequently asserted, on what authority I know not, that hail never falls in the

* *Nature*, vol. xxiv. pp. 187-90.

tropical regions at a less elevation than 2,000 feet. It is worth while testing the statement.

4. *Temperature.* The fluctuations of temperature during a hailstorm are often very remarkable, and should be carefully observed. A reading of the thermometer may be taken shortly before the storm begins and another directly on its cessation.

5. *Barometrical readings* should be taken, if possible.

6. *Wind.* Its (*a*) direction near the earth's surface, (*b*) direction in the higher regions as indicated by the cloud motion, and (*c*) force, are important points. Some observers have noticed that the clouds move in various directions while a hailstorm is in progress. Kamtz actually went the length of attributing the formation of hail to the conflict of opposing winds; and Beccaria says, "While clouds are agitated with the most rapid motion, rain generally falls in the greatest plenty; and if the agitation be very great, it generally hails." Howard, in 1809, noticed the wind change from E. to S., then to W., back again to E., and finally to W., during a hailstorm.

7. *Rain.*—Rain sometimes falls before hail, sometimes after it. The area of a hailstorm is generally fringed with rain, and in the case of a moving storm, rain falls along both edges of the track. Rain before hail is somewhat rare, and its occurrence should be carefully noted.

8. *Clouds.*—Hail clouds are invariably cumulus. Volta and other theorists have assumed that there are always two strata of clouds at different elevations. Arago pointed out that they are generally of an ashen hue. Their aspect, apparent thickness, and height above the earth may be noted.

9. *Electrical phenomena.*—It is frequently stated that thunder and lightning always accompany the fall of hail, but such is by no means the case. When there is lightning, it is important to observe the relation between the discharges and the fall of the hail—whether the lightning precedes the hail, or *vice versâ.* If possible, the electricity of the air before and after the storm should be ascertained by means of an electrometer.

10. *Duration.*—The duration of the storm at one spot may be noted.

11. *Preliminary sound.*—Kahn, Tassier, Peltier, and others affirm that they have heard a peculiar rumbling or pattering sound in the air immediately before the descent of hail. This cannot be a common phenomenon, or it would have been more generally remarked.

12. *Structure and size of the hailstones.*—Observations of the structure of hailstones are seldom of any use, as the necessary precautions are generally neglected in conducting the examination. The ice of which they are composed undergoes a rapid change when exposed to a high temperature, so they ought to be collected immediately on descent. Further, as collision with the ground is liable to cause alteration of shape, if not entire fracture, it is well to catch the hailstones destined for examination upon a piece of flannel, which not only preserves the stones in their entirety, but, being a bad conductor of heat, keeps them from dissolving rapidly. The scrutiny may then be conducted in a cool room. Size should be determined by accurate measurements. Such vague terms as the "size of peas," or the "size of eggs," or "like large nuts," are useless for scientific purposes.

The above points may be supplemented by any others that the experience of observers may suggest or that peculiarities in individual storms may require.

J. A. Westwood Oliver.

Athenæum, Glasgow.

ANECDOTES OF DOGS.

I SEND you some anecdotes of dogs, which, I think, show most strongly that they not only have powers of reason of the same nature as ourselves, but that they share with us distinctly some of our virtues. To me, dogs have always appeared to be by far the nearest animals to man in their intelligence, and in their evident sympathy with their masters, and they show this latter by a power of expression in their faces which no other animals have a trace of. Nothing can be more distinct than the smile of pleasure which lights up a dog's face at the approach of a kind master, and that such an animal should ever become the victim of the tortures of the physiologist is to me inexpressibly horrible.

The following facts, though not actually occurring under my own eyes, were related to me by friends who witnessed them, and I am as certain of the truth of the stories as if I had been myself present:—

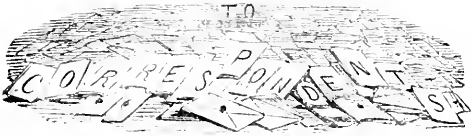
A lady residing in a house not a hundred yards from mine has a pug dog, also a cat of which the dog was always very jealous, chasing it about whenever it saw it. Not many weeks since the pug astonished its mistress by coming up to her, sitting up and begging, then barking and running a little distance from her, till it appeared evident that it wished to persuade her to come with it. It continued to beg and to run on in the same manner till it led her out into the garden, to the foot of an apple-tree, against which the dog raised itself on its hind legs and barked vehemently. On looking up the lady saw the cat with a trap on its foot, evidently in great pain. She got it down and relieved it of the trap, the dog showing the greatest joy, and on the cat being placed on the ground, the dog, who before had never done anything but hunt and worry it, licked it all over and over till it was quite wet, and ever since they have been the best of friends.

A dog had a kennel in the yard of a house which was overlooked from one of the windows. A lady, my informant, saw this dog hiding some of its dinner in a corner behind the kennel, and this performance, she noticed, was repeated for a day or two. On the third day the dog was missing some little time from the yard, but before long it was seen to return, followed by a small, half-starved canine friend, which it took up to the store of hidden food, and stood by, wagging its tail with evident pleasure, while the strange dog consumed it. Now, if these two stories do not show the virtue of charity in a dog, I don't know what can be required to prove it.

The following is of a different nature. In Ceylon the large Lambur deer is hunted by dogs, the huntsmen going on foot. The deer generally comes to bay in a stream among the hills, and the huntsmen, guided by the sound of the dogs, make their way to the spot as quickly as they can with knife or spear to end the combat. Sometimes, however, as the distance, or the form of the hills, prevent the sound of the dogs at bay from being heard, the huntsmen do not arrive, and the deer, if strong, may escape, or is, perhaps, killed by the dogs. On one of these occasions, the owner of a pack of hounds, who related this to me, lost all sound of the hounds, and came back home to breakfast. After this he sallied forth again, thinking he would go to a distant part of the jungle, where he imagined the pack might have taken the deer. Before he had gone a mile or two he met two of his pack by themselves, coming straight for home. They no sooner saw their master than they expressed the greatest delight, and at once turned round, went before him, and led him straight through several miles of jungle, to where he found all the rest of the pack, with a large buck Lambur at bay. They were quietly waiting round it, preventing its escape, and, on seeing their master, the jungle at once resounded with their voices, as they went in with renewed energy at their quarry, till the knife of their master put an end to the battle. Now, it was perfectly clear to my friend that these dogs had agreed among themselves that two of their number should go home and fetch their master, while the rest kept the deer at bay.—B. M., F.R.C.S.

Knowledge.—Although we offer our readers more in the way of original matter (apart from correspondence, which is not to be estimated by mere bulk) than any other journal of similar price and character, we wish to do better still. We hope so to extend the circulation of Knowledge that we may be justified in enlarging each number, in giving more illustrations, and in extending the number of our original contributors. *To attain this end we need the co-operation of our readers.* Those among them who approve our scope and plan can do more to improve Knowledge than either editor or publishers. If every reader were to obtain but one new subscriber, not only would our circulation be doubled, but our power to improve the matter placed before our readers would be increased in like proportion. If our readers will remember this, they will follow the best course for making Knowledge what we wish and hope that it may before long become.

Wordy Science.—It has always been characteristic of a science, real or so-called, which for any reason was not advancing, that its professors have endeavoured to give it an unreal importance by heaping over its facts a mass of incomprehensible verbiage; and the traditions of such a time have survived, in many instances, long after the branch of knowledge concerned has begun to share, or has shared even in the most marked manner, in the general advance of the human mind. Botany, chemistry, the smaller ramifications of natural history, are all still concealed beneath technicalities which are neither English, nor Greek, nor Latin, nor hybrid of any declared description, which convey no meaning, are not generally intelligible to the learned, and are not intelligible, indeed, to any, save those who have wasted precious time in learning them by heart from text books. Such technicalities are unmixed evils, because they do nothing in reality to facilitate the acquirement of knowledge, and they hinder many from even attempting to acquire it.—*Times.*



Letters to the Editor.

[The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with their writers. All communications should be as short as possible, consistently with full and clear statements of the writer's meaning.]

All Editorial communications should be addressed to the Editor of KNOWLEDGE; *all Business communications to the Publishers, at the Office, 74, Great Queen-street, W.C.*

All Remittances, Cheques, and Post-Office Orders should be made payable to Messrs. Wyman & Sons.

⁂ All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.

All Letters or Queries to the Editor which require attention in the current issue of KNOWLEDGE, *should reach the Publishing Office not later than the Saturday preceding the day of publication.*

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

Our Correspondence Columns.

DEMANDS ON OUR SPACE.—CURIOUS PUZZLES.—GRAMMAR.—CENTRIFUGAL FORCE, &c.—A PARABLE FOR PARADOXERS.—THE EARTH'S PROCESSIONAL REELING.—ILLUSIONS.—BRAIN AND SKULL.—MARRIAGE AND THE DEATH-RATE.

[121]—A correspondent remarks, and with some justice, on the space awarded to correspondence, queries, and replies to queries in No. 6. It is natural that in the first number of a magazine admitting correspondence at all, the space devoted to such matter should be much less than in later numbers. As correspondence (including notes and queries) was mentioned in the prospectus, I cannot consider that the increase of this section in Nos. 2, 3, &c., was inconsistent with our promises. But, while noting this, and that, indeed, number 6 was enlarged to oblige correspondents, I must admit that it would not do to allow correspondence often to occupy so much space. As letters come in more and more freely each week, and as we have not room for more than a third of them, it is clear that two-thirds must be omitted. Those that are most original and interesting will naturally have the best chance of being retained, and among these the concise will be preferred.

Some letters will be given in abstract, with such reply as may seem necessary, in a weekly communication from "the Editor."

"H. A. S." notes that there is an exhaustive solution of the problem of the Fifteen School-girls in the "Ladies' and Gentlemen's Diary" for 1862. Magic squares are treated fully in Hutton's "Mathematical Recreations." Nevertheless, I think the three letters on these two subjects published in this or following numbers will interest many who would not care to look up the above-named writer. They may suffice, however, for matters which are rather curious than scientific.

"L. F." remarks (*see* "A. T. C.'s" letter, 105, p. 117) that "would the editor" is more deferential than "will the editor," "might I" less manly than "may I," "I cannot but think," "I can but think," and "I can only think" equivalent expressions. He notes justly that such a sentence as "the editor requests that letters may be addressed," is incorrect. "May" should be omitted. The same with "he requests that all communications (should) be addressed," &c., "should" should be left out; or, better, "he requests that" should be left out. We have under our correspondence heading "he requests that all communications should be as short as possible." Elsewhere in our remarks, be it noticed, we adopt the correct form. Perhaps we meant to say editorially, "we request that the principle be adopted 'all communications should be as short as possible.'" We take "L. F.'s" hint, however, and put this principle down definitely as one which we are obliged to insist upon.

Mr. Newton Crosland is, I fear, offended because I have said that he misapprehends the principles on which the accepted theories which he opposes are based. I am very sorry. I have no wish to offend him. I have simply stated the facts as I see them. He describes the centrifugal force in terms entirely inconsistent with the real nature of this so-called force. (He is quite right in saying that Newton spoke of the centrifugal force; so also Newton spoke of the force of inertia. Newton, however, carefully distinguished these from "impressed forces." Modern science, to avoid precisely such misconceptions as Mr. Crosland's, ceases to use the term force except in the case of what Newton called an impressed force. Joyce, Milman, and Ferguson are not authorities in such a matter, not one of them having had any mastery of the higher mathematics.) Again: Mr. Crosland, in dealing with the Darwinian theory, uses words only applicable to the Lamarckian hypothesis. A student of science knows the Newtonian theory and the Darwinian theory, attacked by him, to be unlike what he supposes; and, therefore, is justified in saying to him that very likely these theories, so misunderstood, appear egregiously absurd. And there the matter should end. I am not interested, and I am sure readers of KNOWLEDGE would not be, in defending the straw-giants which Mr. Crosland attacks after having constructed them. But there is nothing which need offend him in the remark that he misunderstands the teachings of Newton or of Darwin. Many misunderstand both. Of the former, at any rate, which belongs to my own special department of study, I may say that few understand them. Many who understand the Darwinian theory, reject it, or regard it as so far not perfectly proven. But no one who understands the Newtonian theory questions its truth. Any one who, understanding it, yet objects to the former theory, will find his reasoning admitted here. Any one who does not understand any particular point about either theory can ask here for explanation. But all our space would be wasted if we allowed correspondents—first, to set up burlesques of scientific theories; secondly, to overthrow these shams; and, thirdly, in the triumph of an imagined victory, to invent vague theories of their own, in establishing which they assign to imaginary forces inconceivable effects.

A chemist in New Zealand once asked me, as a believer in the Newtonian theory, how I explained a balloon's ascending. Before I replied, he went on to say that, to his mind, the ascent of a balloon proved that the earth exerts a repulsive as well as an attractive influence. There was a pair of scales on the counter, in one of which I put a half-ounce weight, and as the other scale went up, I asked if its motion was due to terrestrial repulsion. "The cases are different," he said. "The cases are similar," I answered; "the light scale ascends because the heavier one has a greater tendency to descend, and cannot descend unless the light one rises; the lighter gas in the balloon ascends (taking its silk coat along with it), because the heavier air around it has a greater tendency to descend, and cannot descend unless the lighter gas rises. The heavier scale pulls up the lighter, which would else tend downwards; the heavier air thrusts up the lighter gas, which would otherwise descend as certainly as a globe of lead would." Whether the chemist understood or accepted my explanation, I do not know; but this I know, that if he had based on his misapprehension of the effects due to gravity a theory of polarity and magnetic repulsion, I should not have felt bound to discuss his views with him. In a similar way, acting according to my lights, and with the object of occupying the columns of KNOWLEDGE so as best to serve the wants of readers, I must decline to assign more than a very modest allowance of space here to paradoxical theories.

"Sirius" asks for an explanation of the "Precession of the Equinoxes." As this is a subject of general interest, I do not put this letter among the other queries, but reserve it for an answer in an early number. I note here that the change is *not* such as to cause the northern pole to be inclined from the sun in June 13,000 years or so hence. Nor is the motion of the poles such that 6,500 years hence, or at any time, the axis of the earth will be at right angles to the ecliptic. The inclination of the axis varies only within a very narrow range, and the seasons remain almost unchanged in each hemisphere, throughout the long precessional period of nearly 25,900 years.

J. E. Okill remarks that the "nest of illusions," p. 71, is illusive to him, as respects the apparent superposition of the space between the two heavy circles, only when he expects to find an illusive effect. If he looks along the lines as a carpenter looks along a piece of wood, they seem straight, and the circles seem oval. (This, of course.) He describes as an illusion what is in reality an effect of diffraction. Closing one eye, look at the edge of a window with the other, covering this eye gradually with the hand till only a fine slit is seen, when the colours of the spectrum are displayed. He remarks on letter 114, p. 121 (Brain and Brain Case), that the two doctors are in agreement. Naturally, the skull takes first a form determined by the growth of the brain, while later the brain may shrink away from its bony enclosure. Also, "may it not be that the son of a man noted for skill in some art or science may inherit the shape of his skull," but, for want of study, the brain

[illegible] spandrel of the brain-case. He replies to Query 27, that the greater number of deaths among unmarried [illegible] be due to evil living, which the married, having [illegible] avoid; also, in part, to neglected ailments. [illegible] it is, however, whether the married [illegible] in itself a prophylactic. We want better statistics than we [illegible]. The greater death-rate among widowers seems to support the views of Bertillon and Stark; but, manifestly, a cause may act in their case which affects the statistics apart from any influence married state may have. RICHARD A. PROCTOR.

JUPITER—DENNING'S COMET.

[122]—I trust that some of your readers will obtain observations of a remarkably bright spot situated slightly S. of the equator of Jupiter and on the N. edge of the great S. belt. I have observed 61 transits of this spot since October, 1880. The following are the approximate times when it comes to the central meridian of Jupiter:—

Dec.	H.	M.
17	10	40
18	6	20
19	11	51
20	7	31
21	13	1
22	8	41
23	4	22
24	9	52
25	5	32
26	11	2
27	6	43
28	12	13

Its period of rotation is 9h. 50m. 6s.6., or $5\frac{1}{2}$ minutes less than that of the red spot, which is 9h. 55m. 35s. It is in the same longitude as the middle of the red spot on December 25, and these conjunctions occur at intervals of $41\frac{1}{2}$ days.

The last time I saw the bright spot it was very plain. This was on the night of December 7, when it was noted exactly on the central meridian, at 4h. 41m. and 14h. 33m. These double transits are not unfrequently visible now that Jupiter is above the horizon during the greater part of the night.

The new comet (of 1881) was observed here on November 25, at 10h., and again at midnight. It was pretty bright and large, with ill-defined and apparently flashing edges. There were many faint stars in the background, so that I could not be certain as to the character of the nucleus.—Yours, &c., W. F. DENNING.

Ashley-down, Bristol, *Dec.* 8, 1881.

THE TRUE SPIRIT OF SCIENCE.—WATER SPHERULES.

[123]—Let me venture to express a hope that the concluding portion of the lines from which you have taken your motto may, as I doubt not they will, find expression in your magazine,—

"Let Knowledge grow from more to more,
And more of *reverence* in us dwell."

The spirit of reverence and of humility, the spirit of Newton, Herschel, and Faraday, seems gradually vanishing in many high scientific circles, and a spirit of self-sufficiency and of arrogance growing in its place. It is sad to see how frequently the student of nature of to-day, in the consciousness of his acquisitions, referred to yesterday, forgets the absurd littleness of his knowledge referred to the sum of all things.

But I must not stay to pursue this thought, as my object in writing to you was to refer to a question asked in the copy of KNOWLEDGE sent to me, No. 3, p. 60, where a correspondent asks what supports or causes the spherules of water which are often seen rolling on the surface of water. The explanation hypothetically given in your columns is, as you suspect, wholly untenable. The probable cause is a *difference of temperature* between the drop and the surface on which it rests.

Apropos of this, the accompanying short paper may, perhaps, interest some of your readers. It was read at a meeting of the Royal Dublin Society on Dec. 15, 1877, but, so far as I know, has not been published out of the proceedings of that Society.—Yours, &c., W. F. BARRETT.

[The paper shall appear shortly.—ED.]

THE WEATHER FORECASTS.

[124]—Taking great interest in the daily weather forecasts as efforts made by the director of the Meteorological Office and his staff to predict the weather in advance, I have for about two years past been in the habit of noting briefly the characteristics of each day, and comparing them at night with the predictions as published in the *Daily News* [illegible] discrepancies—not all the newspapers printing the same tables. The variations in phraseology are, however, generally unimportant, consisting in the elision of the word "very," or making trifling variations in the direction of the wind. For the period of the last six months my notes have been fuller than formerly, so that I will confine my observations to these more particularly.

As the main conclusion, I am bound to say that they are about as often wrong as right in the metropolitan area, to which my attention has been almost exclusively confined. There is great difficulty in drawing a sharp line between "successes" and "failures," for often the temperature, force, and direction of wind have proved right; but the forecast as to rain or fair weather has frequently been altogether unsuccessful, so that it is not easy to state the percentage of successes.

I have noted particularly eight occasions of excessive rain or thunder-storms happening within the months of last summer, not one of which had been predicted in our daily forecasts, although, strange to say, clear warning had sometimes been sent from America, as was noticed by a writer in the *Times* of July 9 last, signing himself "Observer." The dates I now refer to are—May 28, when we had a thunderstorm and torrents of rain in the afternoon, certainly not predicted in the forecast, "cloudy, but *fair* and warm;" June 5, when we had more than half an inch of rain—not predicted; June 25, eight hours' rain, from 10 a.m. till 6 o'clock in London—not predicted, but coming after a week of gloriously bright weather. Then came the heavy thunderstorm of July 5–6, to which I have already referred, bringing down the temperature from about 92° F. to 70° F. in one day; a disturbance not foretold by our Meteorological Office, although a warning from the *New York Herald* had already appeared in all the newspapers. July 28, when the prediction was "*fine*, hot cool in the morning." Right as regards temperature, but wrong as failing to give notice of the torrents of rain which fell before midnight. August 8, the weather predicted was "cloudy or dull at first, then clearing again, and *fine and warm*," instead of which the weather in London was fine until 4 p.m., followed by six hours' rain, which brought down the temperature so low, that at noon next day the thermometer only registered 55° F. August 12, the prediction "cool, changeable, showery," did not prepare the sportsman for continuous rain from morning till night (sixteen hours, at least, without ceasing) and not well described by "changeable" or "showery." The direction of the wind was wrongly predicted, proving East and North-east, instead of "Westerly to North-westerly," and almost calm in London, instead of "strong and squally." Passing over August 16, when we had a fine morning, succeeded by a wet evening (the reverse of the prediction), and August 18, a fine calm day, not "squally" or "showery," as predicted, we come to August 25, raining all day all over England (see the column of reports in the *Daily News* of the following day), and certainly not fulfilling the predictions "fine generally," or "greatly improved weather" promised for the English districts Nos. 2 to 5 inclusive, but, on the contrary, everywhere spoiling the harvest prospects, and remaining bad weather for the rest of the month.

Later instances of "bad shots" are in my note-book, but for the purpose of discussion the above may suffice, and with every desire to keep within the bounds of fair criticism, I wish only to point attention to the facts, in the hope that the causes of failure may be discovered and eventually remedied. Mr. R. H. Scott, in his article on "Forecasting the Weather," published in *Good Words* of July and August, admits his "inability to form any estimate of the amount of rain which is likely to fall on any day," although continually professing to take account of rain in his daily forecasts. The whole subject gained importance from the question asked last August, in the House of Commons, by Mr. Earp, M.P., as to the facilities for the promulgation of those weather-forecasts which the Postmaster-General might be inclined to give? and the seaports and agricultural towns have now the chance of getting information on very easy terms. It remains to be seen whether the local authorities rightly estimate the efforts of the Meteorological Office to provide this much-needed information. The end can only be attained after many failures, for, as Professor Huxley says, "all true science begins with empiricism." In what directions, then, are we to look for improvement in the character of the weather-forecasts? And what new means can be devised to give them greater accuracy? These are questions which appear to be well worth discussion in the pages of KNOWLEDGE.—I am, yours, &c.,

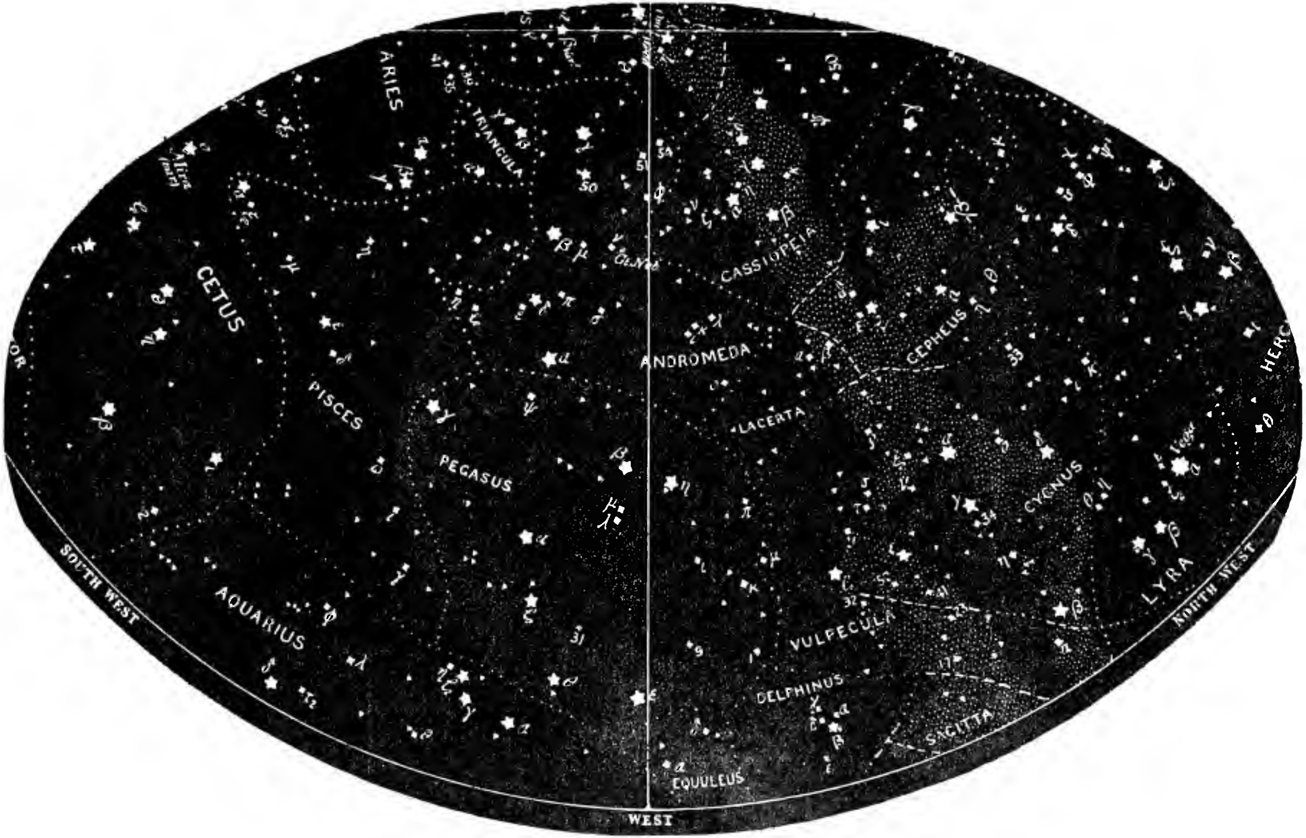
JOHN SPILLER.

WHY RATS GNAW THROUGH A WATER-PIPE.

[125].—The moisture of the atmosphere is condensed on the outside of the pipe during a change of temperature (the cold water in the pipe and the moist warm atmosphere), which induces

[*Continued on page* 143.

THE WESTERN SKIES IN DECEMBER.



This Map shows the position of the stars in the Western Skies —

On November 30, at 10½ o'clock.	On December 8, at 10 o'clock.	On December 16, at 9½ o'clock.	On December 23, at 9 o'clock.	On December 30, at 8½ o'clock.
On December 4, at 10¼ o'clock.	On December 12, at 9¾ o'clock.	On December 19, at 9¼ o'clock.	On December 27, at 8¾ o'clock.	On January 3, at 8¼ o'clock.

The stars in the western skies are passing downwards towards the horizon, moving aslant from left to right; but those shown on the right, or northern side of the map, will not set, but pass over towards the north, there to pass below the pole.

*** Stars of the first magnitude are shown with eight points, those of the second with six, of the third with five, of the fourth with four, of the fifth with three.

Continued from page 110.]
them to drink, and as the supply is only small, they naturally gnaw through the pipe. This, in my opinion, is the more correct view than to imagine they reason from hearing the fluid pass through the tube. W. MARSHALL, F.B.S.E.

ELEMENTARY ELECTRICITY.—SCIENTIFIC MEETINGS.—MYSTERY OF "PSYCHO."

[126]—Having read letter (No. 73), signed "Daniel Jones," I should like to point out that it seems to me very undesirable that you should use your valuable columns for any such papers, which can be read in any good text-book on electricity.

May I suggest that you should find room in your columns for a notice of times and days of the meetings of scientific societies during the ensuing week, with the titles of the papers to be read.

I should also like to ask through your columns whether a solution of the mechanism employed in the automaton "Psycho" has ever been given?—Yours, &c. H. W. B.

[We quite agree with our correspondent on the first point. As to the second, we believe that only a small proportion of our readers care to know the days on which the various scientific societies meet; and that all who do care, must be in the way of learning those dates elsewhere. It seems, therefore, better for the greater number—by far the greater number—to save for other purposes the space which such notices as "H. W. B." suggests would occupy. —ED.]

NAMES OF STARS.

[127]—I am making a collection of the ancient names of stars. In this I have been much assisted by the list given in your larger Atlas. I should be very much obliged to you if you could point out any books in which I could find more of these names, as well as any traditions about them. A friend of mine, who is an Arabic scholar, has promised to translate those which are not too much corrupted. It seems a pity that these names should stand a chance of being forgotten, as much of the popular interest in stellar astronomy dies out when stars are only designated by a letter in a shapeless constellation. Apologising for thus troubling you, Yours, &c., J. H. THOMSON.

R. A. Mess, Shoeburyness, *Nov.* 28, 1881.

[There is much that will interest Mr. Thomson in "Smyth's Bedford Cycle," the old edition. From the new one we understand the matter in question has been removed.—ED.]

DONATI'S COMETS.

[128]—In Figures I. and II. on page 49 we have two pictures of Donati's comet, as also, judging from the stars given in the diagrams, the distance and direction travelled by the same. Now, if the stars depicted on the plate have relative places in the firmament, the comet must have been travelling in a side direction, and consequently the tail must not have been pointing from the sun.

A YOUNG ASTRONOMER.

[The picture is right. There is no such difficulty as "Y. A." supposes. The comet's path is not on a line towards the sun.—ED.]

PASTEUR'S PLATES OF GERMS, &c.

[129]—Would Dr. A. Wilson, if possible, kindly give us Pasteur's plates in connection with the subject. He may intend to do so; if so, my suggestion is premature. I am sure they would be interesting. A friend, a few years ago, lent me Pasteur's plates on the germs generated in brewing, and many a pleasant hour have I spent in trying to obtain the different germs by experiments on beer, yeast, &c.

I have read a good many of the scientific papers of the day, but have never been so interested in any of them as I have in KNOWLEDGE, which, I feel sure, has only to be known to be a complete success and boon to the public.—Believe me yours, &c.

F. C. S.

COLOURS IN ANIMALS AND PLANTS.—DARWIN ON WORMS.—REASONING ANIMALS.

[130]—The tendency of men of science to run investigation to extreme tenuity is become a danger. Knowledge whittled to a point is knowledge reduced to nothing. Casual inquirers recoil from infinitesimals and the overwrought.

Dr. Wilson, in KNOWLEGE, No. 2, labours to show that the colours of wild birds and beasts are designed by Nature to obscure the animals from their enemies, and, when predatory, from the eyes of their prey. It might be affirmed that the colours of flowers are designed to put the flowers conspicuously before the eyes of men and insects. Nature is not given to special amenities and providences. The dark colours of the sole and the flounder are no protection. When in repose, they are concealed by a covering of sand.

Mr. Darwin's theory of worm action on the soil is, to my mind, an elaboration of exceptional minutiæ to huge paradox.

The paper on Intelligence in Animals is in another category. Anyone capable of accurate observation, and daily among them, knows that animals do reason. I could name many instances, but one shall suffice. A duck came opposite our day-room window, which looks into the orchard. Mrs. D. noticed her uneasiness and persistent waiting, and concluded that it indicated hunger. She took madam duck some corn. The food was simply looked at, but the look was followed by movements that plainly meant "come with me." The request was accorded to, on supposition that the duck was shut out of her cote. But, no; the cote was open, and the duck urged "come on, come on." She led to a hole in the hedge that fenced the stackyard from the orchard; and there, on the other side, was a lame duck, that could not make passage of the hole, in a fever of despair. Mrs. D. went round and brought the poor bird into the yard. Meanwhile, the kindly-hearted duck made her way to where she knew her mistress would return, and the rest of the flock gathered with her. The meeting of the two ducks was a little drama. Between them there was a wonderful talk, soon joined in with by the whole flock; and they marched off with what was taken to be loud cheers.

Reason! Surely animals, furred and feathered, reason; some better than others, but all reason. The fox is a rapid and acute reasoner.

I have but limited intimacy with pigs. So far, however, as I am acquainted with them, nothing is so manifest to me as that they reason; and they are humorous and inclined to fun, when at liberty. A well-known sportsman had a pig for pointer. Dogs all reason, the high-bred sheep-dog in particular; and more, understand sentences of speech addressed to them directly, or in their hearing, and in the latter case, they will, if so minded, circumvent you on the knowledge they have gained.

Ducks and dogs have the human propensity to sulk with you, or with one another.

Memory, no observer will deny them. But is not an effort of memory reasoning; calling together ideas previously known, and linking them, absolutely or tentatively, in sequence, till the desired lost idea is regained?

Professor Jevons' logical abacus, in work, really represents the mode of an effort of memory, as well as a logical concatenation.—Yours faithfully, B. DONBAVAND.

Picton, Chester.

ATMOSPHERIC ABSORPTION AND TERRESTRIAL RADIATION OF HEAT.

[131]—In his "Heat a mode of motion," (1880) page 317, Prof. Tyndall says:—"I never on any occasion suffered so much from solar heat as in descending from the 'corridor' to the grand plateau of Mont Blanc on Aug. 13, 1857. Though Mr. Hirst and myself were at the time hip-deep in snow, the sun blazed against us with unendurable power." Mr. W. Mattieu William's letter 68, page 96, and B.M., F.R.C.S., No. 36, page 77, bear similar testimony. I think Prof. Tyndall explains the whole matter (page 317). "The beams of the sun penetrate glass without sensibly heating it; the reason is, that having passed through our atmosphere, the heat has been in a great measure deprived of those constituents liable to be absorbed by glass." Prof. Tyndall refers to the invisible (the hottest) rays, see page 316. It seems to me too much stress is placed on the atmospheric absorption of terrestrial radiation. From the foregoing references a large proportion of the sun's invisible rays are intercepted before they reach the earth; and then all those which do impinge on the earth raise its temperature, and by "convection," more heat is transferred to higher altitudes of the air, so that only a comparatively small proportion of the heat is radiated from the earth compared with what enters the atmosphere as solar radiation. J. A. L. R.

[A large portion of the invisible solar rays remain (compare figs. 112 and 113 in Tyndall's book) after passing through the air, and to these, constituting at least twice the visible rays, the reasoning of Mr. Dyer (in letter 20, p. 56) applies.—ED.]

"KNOWLEDGE" AND THE SCIENTIFIC SOCIETIES.

[132]—I see that some of your readers are asking for Reports of the Proceedings of the Learned Societies, and that you have replied very practically by showing them a sample of what the most meagre outline would amount to.

In confirmation of your decision, I may state my own practical experience. When my genial friend, the late George Dawson, started the *Birmingham Morning News*, I undertook the functions of

"Our London Scientific and Educational Correspondent," whose work was to supply a weekly column or two, by means of which the readers of the paper should be kept acquainted with the general progress of science and scientific education. My original programme included an account of the proceedings of the Scientific Societies, but I met with a rebuff at the outset. The secretary of the Royal Society informed me that I should not be admitted to any of their meetings if I reported any portion of the discussions or conversations following the reading of their papers, and that they would rather not have any newspaper anticipations of the official publication of the paper themselves. The titles of the papers were at my service. I began by heading each of my communications with a list of the papers read at the principal London Societies, and supplementing this by a short notice of some of those likely to be popularly interesting; but alas! how few were they—not 5 per cent. of the whole. This was in January, 1871. By the middle of May, I gave up even the titles of the papers, finding them about as readable as the Post-office Directory, and practically worthless as a record of scientific progress, seeing that London is not all the world. If I had added the titles of papers read at foreign societies, my weekly column would have been filled with these alone.

To give your readers an idea of what would become of Knowledge if you gave abstracts of current scientific papers, I have counted the number included in one month's *Journal of the Chemical Society*, wherein abstracts of papers read outside of the Society are given:—

General and Physical Chemistry	26	papers
Inorganic Chemistry	25	,,
Mineralogical Chemistry	74	,,
Organic Chemistry	113	,,
Physiological Chemistry	7	,,
Chemistry of Vegetable Physiology and Agriculture	10	,,
Analytical Chemistry	23	,,
Technical Chemistry	23	,,
Making a sum total of	371	papers

in one month (June last) on one branch of science only. A staff of thirty experts, Fellows of the Society, are engaged in making these abstracts. The annual volume of abstracts (all very short) averages above 1,000 pages, besides the transactions, which usually run to about 800 pages.

To the professional chemist these are invaluable; to the general public they are mere waste paper.

W. Mattieu Williams.

THE MISSING LINK.

[133]—This question seems to generate new ideas (and not always reasonable ones) as time passes. One correspondent, "Another Ignoramus," says: "Common ignorance (?) will continue to ask the unfortunate question." Why "unfortunate" is more than I can imagine. In my previous answer I tried to show that the phrase "missing link" was a misnomer. It implies that there was but *one* link connecting man in the past with lower life, and it thus implies an amount of knowledge which honest science declares it does not yet possess. Persons whose demands are all for the "missing link," generally ignore the overwhelming evidence of evolution as a fact of nature that Mr. Darwin, Mr. Wallace, and others present. Nobody calls anybody else an "ignoramus" except those bigoted minds who object either to receive evidence, or who are unable to appreciate and weigh evidence of evolution or any other topic. I certainly called no one an "ignoramus." On the contrary, I did my best to assist a comprehension of the question concerning man's relationship to his lower neighbours. Any one may object to the views of another, but objections should be stated in full. Querulous complaint assists no cause, least of all that of scientific truth.

"Mitchell" likewise writes on the "Missing Link" subject. Has "Mitchell" any knowledge whatever of geology, or of the history of fossils? I ask this, because if (as I suspect) he expects to find every animal and plant species that lived in the past preserved as fossils, he is simply quarrelling with Nature's flaws, and neither with evolution nor with me. I refer him to the "History of the Horse," as disclosed by Professor Marsh in America, for an excellent illustration of the transformation (or evolution) of one order, genus, and species into another. He will find the account in Huxley's "American Addresses." To answer his second inquiry, he should acquire a knowledge of geology. "The imperfection of the geological record," as treated by Darwin in the "Origin of Species," will give him a capital outline of the whole case, and an acquaintance with geology as a whole will show him that the probabilities of evolution are overwhelmingly supported by the conclusions drawn from fossil history. People must know there is no "royal road" to the truths of nature, either in the pages of Knowledge or anywhere else. My advice to "Mitchell" and to "Another Ignoramus" is, to acquire a general knowledge of biology and geology in a classroom, or by a course of wide reading. Otherwise they will always be encountering difficulties, or, perchance, complaining of the lack of light in others.

Andrew Wilson.

DIFFICULTY OF OBTAINING "KNOWLEDGE."

[134]—Can you tell me why there is so much difficulty in procuring Knowledge? I never had so much trouble in procuring any periodical or paper before. So far, I have never been able to procure any of the numbers until perhaps six, seven, and eight days after date of publication, although it is regularly ordered (every day) by my bookseller here, who has a London parcel daily. Every time I call at the bookseller's I meet with the same disappointment—"not yet arrived; must be published irregularly, or perhaps it is dying." Now I think it fills a great want, and would be sorry if it ceased to exist, although I myself will be obliged to give up, as I cannot go on calling at the bookseller's eight, nine, and ten times for each number.

However, wishing Knowledge all success, as it deserves to succeed,—Yours, &c., H. Armstrong.

[We regret that our correspondent should have been so badly treated. Not knowing either his bookseller or the bookseller's London agents, we cannot tell where the fault lies. One or other must have neglected to do what he was asked to do. Knowledge is published exceptionally early, and with perfect regularity. So far from dying, it thrives amazingly.—Ed.]

THE EQUALITY OF THE SOLAR ILLUMINATION THROUGHOUT THE SYSTEM.

[135]—I have lately fallen in with a rather curious theory concerning the "Solar Illumination throughout the System." Its originator appears to be perfectly satisfied that he has completely disposed of the long-accepted law of inverse squares, and seems to think that he has conclusively demonstrated that the sun's light is equally intense throughout the system. According to his theory, scientific men have for ages been labouring under a great delusion. Their idea that light decreases as the square of the distance, is a mistake. In our atmosphere, he admits, there is a decrease with the distance; but this, he affirms, is due to absorption by the air, and he is prepared to prove (to his own satisfaction, at any rate) that in space, where there is no absorbing medium, light does not diminish as the square of the distance. This theory is so bold and startling, that I should like to have the opinion of the readers of Knowledge concerning it. For my part, the arguments the author of the theory (Mr. Collyns Simon) uses are not conclusive enough to warrant our rejection of long and firmly-established principles. The admission of the truth of the theory necessitates a number of absurd consequences. The stars should appear to us as bright as our sun. And if the sun's *light* follows this law, so also must his radiant *heat*, which amounts to saying that the sun's heat would remain constant, even although it were possible to approach to his surface. Hoping to hear other of your readers on the subject,—I am, &c.,

An Admirer of Knowledge.

[Our correspondent is right in rejecting the theory of Mr. Collyns Simon. It is utterly untenable. I have had many letters from him, as well as books and pamphlets. Analysing his case, I find he misunderstands the evidence which leads to the sound theory, that apart from absorption, a luminous body presents a surface of equal apparent brightness from whatever distance it may be viewed—so long as it presents a visible surface at all. If we receded from the sun till our distance was twice as great as it is, his disc would look just as bright, but only one-fourth as large, as it does. The correct inference is that we should get but one-fourth of the light we actually receive. But somehow Mr. Simon makes out that we should get quite as much as we do at present.—Ed.]

[Note.—We have been compelled to omit the Chess Column for this week, owing to the pressure of correspondence on the one hand, and the necessity of assigning a certain portion of our space to original matter on the other. Our correspondents must not be surprised at the compression in many cases, and the omission in others, of letters, many of which we should have been glad to publish in full, had space permitted. A long letter from our esteemed correspondent, "A Fellow of the Royal Astronomical Society," is deferred till next week. It contains answers to many of the questions which have been asked by correspondents.—Ed.]

Queries.

[92]—Falling Bodies.—Would a body, let fall from a height fall directly towards the centre of the earth or not? If not, would deviation be caused

(a) by the attraction of neighbouring bodies,
(b) by centrifugal force,
(c) by the velocity imparted by rotation round the earth's axis; or,
(d) by a combination of these causes?

Would not a body in the northern hemisphere fall to the S.E.; and one in the southern hemisphere to the N.E.?—$\frac{W}{g}$. [We have had greatly to shorten $\frac{W}{g}$'s question; but the above gives its purport. We answer it ourselves, to save space, and also the trouble of selecting between several answers. First, then, cause (a) is usually very slight, but in the case of a mountain or other great irregularity of contour or of internal structure of the earth's crust below the point of suspension, would produce a slight deviation from the vertical; (b) and (c) really relate to the same cause of deviation, which operates everywhere except at the pole. If h be the height of the point of suspenion above the surface, and r the earth's radius, then before falling, the body has a velocity due to rotation which bears to the velocity of the point on the earth's surface vertically below it, the ratio $(h \times r) : r$. Owing to this difference of velocities, the course of the body as it falls is not directly towards the point below but is, such as to carry the body somewhat to the east of that point (in both hemispheres). There would be no deviation north or south in either hemisphere, because a plane through the earth's centre and the point of suspension, and tangent to the circle in which this point is carried round the earth's axis, cuts the earth's surface at the point below the point of suspension in an east-and-west line. All the motions which affect the falling body, considered with reference to the earth, take place, during the fall, in this plane, and therefore cannot cause the body to leave that plane or to deviate from the east-and-west line. If the time of fall be t, the earth's rotation period P, height of the point of suspension h, and latitude of the place λ, the easterly deviation will be $2\pi h \frac{t}{P} \cos \lambda$.—Ed.]

[93]—Water of Ayr Stone.—I should be glad if any reader of Knowledge would tell me how water of Ayr stone is cut into slips ⅜ in. by 1¼ in. by 6 in.? They have the appearance of being sawn with a circular saw. If they are, is the saw blunt, or with teeth; if with teeth, coarse or fine cut? Also if sand or water is used in cutting, or both?—T. G. H.

[94]—Watch Pivots.—How is arbor held and revolved in turns to reduce a pivot? Also how to proceed in renewing a broken pivot?—T. G. H.

[95]—Watch Jewel.—What tools are used in putting in new jewels, and how to proceed?—T. G. H.

[96]—The Earth's Centre.—If pressure (or weight) at the surface of the earth depends on gravity, then at the centre of gravity of the earth this attraction will be upwards in every direction, and weight will consequently be nil. The region of greatest pressure will thus be situated somewhere between the centre and the surface. Will this have any influence on the relative densities of the earth's interior; and will not the matter at the centre be "light as air?"—J. A. L. R. [J. A. L. R. confounds pressure with attraction. Attraction vanishes at the centre, but the pressure, which results from the weight of all matter between the centre and the surface, is there at a maximum.—Ed.]

[97]—We know the inclination of the earth's axis produces the change of seasons, and that the northern winter occurs at perigee, and the southern winter at apogee. Also, that in about 12,000 years this condition of things will be reversed, and the northern hemisphere will then be most favourable to a glacial epoch, which undoubtedly the southern hemisphere now experiences. Is there any significance (bearing in mind the preceding statements) in the fact that the land predominates in the northern and the water in the southern hemisphere? And may we assume that when the climatic conditions of the two hemispheres change, the physical features likewise will change, and the maximum of water then be in the northern hemisphere? Or is it a mere coincidence?—J. A. L. R. [A French student of science, Adhèmar, started the theory that the relation referred to by "J. A. L. R." is not a mere coincidence, but there is no evidence to show that the hemisphere where there is most water is necessarily that where the earth's nearest approach to the sun occurs in the summer time. The changes in the eccentricity of the earth's orbit, and the position of the place of nearest approach, are not quite so regular as Adhèmar supposed. See my article "Astronomy," in the "Encyclopædia Britannica," pp. 795 and 796.—Ed.]

[98]—Kindly inform me when Mars will be at its next best point for telescopic observation, and the simplest method for arriving at the result.—Algol. [On Dec. 27 Mars will be most favourably situated for observation. What result do you mean?—Ed.]

[99]—Summer Days and Winter Nights.—My almanack tells me the sun reaches its most northern declination of 23° 27′ 14″ on June 21, when it rises at 3 h. 45 m. and sets at 8 h. 18 m.; that on Dec. 21 the declination south is 23° 27′ 12″, sunrise at 8 h. 6 m., sunset 3 h. 51 m. How is it, if (as I have always understood) the condition of things is exactly reversed on these days, that the length of the *longest* day and *longest* night are not equal? Why does not the winter day commence at 8 h. 18 m., the time the summer night begins? The June day has a duration of 16 h. 36 m., the December night a length of 16 h. 12 m. How is this?—F. F. [The difference is due to the refractive action of the air, which causes the sun to appear to rise sooner than he actually does, and to set later. This causes every day to be longer than it otherwise would. The actual interval between sunrise and sunset (for sun's centre) would be 16 h. 24 m. on June 21, and this would also be the interval between sunset and sunrise on Dec. 21, were it not for refraction.—Ed.]

[100]—Stings of Wasps, &c.—How do you account for the stings of hornets, bees, and wasps not affecting the toad when it eats them?—Ornithorynchus.

[101]—Industry of Ants.—Are ants in reality the models of industry which they are proverbially reputed to be?—Ornithorynchus.

[102]—The Unseen Universe.—Can you refer me to any book or lecture of Professor Tait, of Edinburgh (I think it was Tait, not Crum Brown), in which the wonderful theory was expounded, as part of the idea of conservation of energy, that our words and actions, whether good or bad, come not to an end on the spot, but, converted into some form of indestructible energy, wander away into space, there, in some passage of eternity, in their accumulation, to work for the good or ill of the producer? I am so certain that I have seen something like this from Tait, a sort of scientific demonstration of the possibilities of a future state, that an early reply in Knowledge would much oblige.—A. A. F. [Probably the "Unseen Universe," by Tait and Stewart, is the work A. A. F. requires.—Ed.]

[103]—Could you recommend me a good book on "Histology" (of animals), and one on the Undulatory Theory of Light, treated mathematically?—D'Artagnan.

Replies to Queries.

[29]—Garden Tripod for Telescope.—Let the top of the tripod be a circle, 1 in. diameter, and 1 in. thick; into this the brass leg which the telescope probably now has may be screwed, or it may pass through and be clamped at any height, preferably the last, but I could scarcely explain it without an illustration. The legs should taper from 2 in. square to 1¼ in., of whatever length you require, so that they touch the ground about 4 ft. apart. The upper ends must be bevelled, so as to fit accurately against the circular top, and had better be hinged on the inner side, that they may fold inwards when not in use. The whole must be firmly made, or it will be useless.—Paugul.

[30]—Solar Heat.—(1.) The difference in distance is as nothing compared with the distance of the sun. Any physical geography will explain that it is because of the less vertical direction of the sun's rays. (2.) Both the sun's direct action, and the reflection of his rays from bright objects, snow, sand, &c. Radiation from the earth plays no part in the matter.—Paugul.

[41]—The Planet Vulcan.—I don't believe that Lescarbault ever saw an intra-mercurial planet, but it appears that not only one, but three or four have been seen.—Paugul. [The existence of an intra-Mercurial planet is very doubtful.—Ed.]

[42]—B.Sc. and D.Sc., Lond.—Surely this "Enthusiast" has no conception of the vast store of learning for which he is asking when he speaks of the questions set in the D. Sc. Exam., Lond. If he intends to graduate in science, he should study at a university. If he will write to me, I will give him a long list of works which will cover a portion of the syllabus he mentions.—Paugul.

[50]—Wasted Energy?—The light which is absorbed is converted into heat, and raises the temperature of the body which receives it.—Alfred J. Shilton.

[52]—Microscope.—One of the kind called medical microscopes, having the body tube sliding in the stand, and in two parts. The objectives should be made to slide into position and not to screw—it saves time. This microscope permits a more rapid change of amplification, while still giving all other facilities for work as well as any other.—Paugul.

[54]—Chemical Questions.—The explanation of (1) given by C. T. B. is very good, but he is wrong in supposing that hydroferrocyanic acid has not been isolated. Mr. Theophilus Pitt accuses "Castor and Pollux" of being "inattentive" and "careless," adjectives which are very applicable to himself, as he has misread the question, or else he is entirely ignorant of the fact that ammonia and hydrosulphuric acid form ammonium sulphide; and he apparently thinks that ferrocyanide of potassium should give a precipitate with ammonia and ammonium sulphide, which is a serious mistake. With regard to question (2), C. T. B. suggests that the solution might be alkaline. How can a solution of potassium bisulphate be alkaline? The true explanation is that tartaric acid is unable to displace sulphuric acid from potassium bisulphate, although it can take up one equivalent of potassium from the neutral sulphate (K_2SO_4), forming bisulphate ($KHSO_4$), and bitartrate ($KHC_4H_4O_6$). Potassium iodide should give a precipitate if the solutions are strong. The tartaric solution ought to be very strong (1 part in 4 of water). A solution of platinum tetrachloride is a far more delicate and reliable test for potassium; it should be used in the same way as tartaric acid, and, like it, also precipitates ammonium salts.—E. F. H.

[55]—Tables of Meridional Parts.—The meridional parts inserted in most works on navigation, as in Norie, Bowditch, Inman, Raper, Chambers, &c., are very old, and were computed for compression 0, in fact, for the earth as a sphere; these differ among themselves only in so far as Chambers and Inman give the quantities to two places of decimals. Mendoza Rios used compression $\frac{1}{321}$, and these are the tables given by Riddle. Rümker used compression $\frac{1}{305}$, and are the same as given by Caillet; but, by far the best table of meridional parts is that given in the "Projection Tables" computed for the United States Coast Survey Department and Hydrographic Office, and reprinted in England in the last edition of Raper's "Nautical and Logarithmic Tables"; the compression is here taken as $\frac{1}{299{\cdot}1528}$, based on Bessel's determination.—W. H. R.

[56]—Roscoe and Schorlemmer's "Treatise on Chemistry" (Macmillan).—Alfred J. Shilton.

[58]—Sirius and Orion.—The statement was, of course, wrong. Betelgeux must have been meant.—Paugul.

[62]—Algol.—Perseus is represented as holding the head of Medusa; the stars he mentions are one.—Paugul.

[63]—Algol and Mira.—Mira was at its maximum early last July, and will be a month earlier next year. Algol every couple of days.—Paugul.

[65]—New Star in Cassiopeia.—We do not know. A star has been seen, or is said to have been seen, two or three times in or near Cassiopeia, at such dates that, if correct, it is nearly due now. That is nearly all we are able to say; a small star has certainly been seen lately in Cassiopeia where one was not noticed before, but it may have nothing to do with the supposed variable.—Paugul.

[77]—Horrible dreams are often due to the quantity and quality of food taken at supper. I think Pliny the younger tells you to eat heartily of roast pork just before retiring if you want horrid dreams. The worst dream I ever had was due to pickled cabbage and cheese just before going to bed. The results are thought to be due to pressure on the solar plexus of nerves in the abdomen, which set up some irritant action in the brain. To avoid bad dreams, eat light suppers, at least three hours before retiring; take a little gentle exercise if possible, and avoid startling romances at night.—T. R. Allinson, L.R.C.P.

[79]—Mental Physiology.—"S. S. S. S." should have stated how far he wished for works of a "specialist" physiological character. Outside these, I don't think he can have a later, or a better or more complete work on the *mental* side of the subject (and which gives references to all needful works) than "An Inquiry into the Process of Human Experience, &c.," by William Cyples, published by Strahan & Co., 34, Paternoster-row, price 21s.—S.

[79]—Mental Physiology.—Dr. Carpenter's "Mental Physiology" is a capital book. "S. S. S. S." should look over the list of the "International Science Series" (Kegan Paul & Co.) for several works on the brain and mind. Dr. Maudsley's books on mental subjects will also be found very useful by a student of mental science.—Andrew Wilson.

[83]—In answer to (*a*), Berthelot describes the powdery form of sulphur in vol. xlix. of "Annales de Chimie." It is formed also at the zincode of a battery, in the electrolysis of sulphurous and sulphuric acids. (*b*) Nitric oxide in meant.—C. T. B.

[83]—Sulphur Modification.—There are five modifications of sulphur known. The two crystalline forms, the plastic form, a dark brown powder left on treating the plastic form with CS_2, and a light yellow powder insoluble in CS_2 (carbon disulphide) deposited from solutions of sulphur in CS_2, on being allowed to stand. The above is from "Roscoe and Schorlemmer's Chemistry," Vol. I., paragraph 156. (C.) It should be nitric oxide.—A Greenwich Student.

[84]—Ancient Man.—"Clio" (Query 84, p. 123) desires some information as to the reliability of the researches of Professor Horner. The Professor published them in 1854. He states that he made ninety-five vertical borings in the alluvial deposits of the Nile valley, and at the depth of 39 ft. fragments of pottery were found, and the conclusion drawn is, to use the Professor's own words, "if there is no fallacy in my reckoning, these fragments of pottery must be held to be a record of the existence of man 13,000 years ago." The whole data of his conclusion rests upon the assumption that the deposit of the Delta was gradual and uniform. This he assumes from calculating the interval between B.C. 1150, the time of the erection of the statue of Rameses II., and the time at which the exploration took place. And judging from the thickness of the deposits around the statue, from its base upwards, he infers that the Delta accumulated, if my memory serves me, at the rate of two and a-half inches a century. It will be perceived that this calculation takes as its basis that the accumulation was successive; but we have no means of ascertaining how far the base of the statue stood above the reach of the inundation when first put up, and so have no basis for any calculation. Again, the water of the inundation having been for ages kept out, according to Egyptian custom, from the enclosure in which the statue stood, the accumulation of the deposit there was the more rapid when, in after times, the water was admitted. This accounts for the thickness of the sediment without any *successive* deposit. Herodotus (Book ii., p. 99) mentions that Menes, first king of Egypt, B.C. 2350, was supposed to have diverted the course of the Nile by a dam twelve miles south of Memphis, and thus to have dried up the old bed. It may be that the statue of Rameses II. stands on the old bed, and the fragments of pottery were deposited by after inundations. Less than a thousand years ago the Nile flowed close to the western shore of Cairo. It is now separated from it by a plain more than a mile in width. At a depth of 30 ft., fragments of pottery were found less than a thousand years old. Professor Horner says that "fragments of burnt brick and pottery were brought up from the lowest part, viz., 50 ft. from the surface." Now it is an undoubted fact that there is not a single structure of burnt brick from one end of Egypt to the other, earlier than the Roman dominion. Mr. Birch, the Egyptologist, refers the burnt brick to B.C. 1300. On these and other grounds I think it may safely be said that the evidence for man's existence as derived from the researches of Professor Horner is not reliable.—R. S. Couch.

[88]—Brain Injuries.—The brain is divided into two parts. One controls involuntary motion, as breathing, the heart's action, &c.; the other is used for thought, the reasoning powers, and passions. The brain is also double, so that, injure one part, and the other side takes up its action. This is true with the reasoning and thinking powers. Injure a part governing any member, and you see at once paralysis of that member. From this is inferred that injury to the intellectual part is not followed by any noticeable change, but injure any of the motor parts, and you get at once paralysis. The brain is the organ of feeling, in that it makes us aware of injuries inflicted anywhere on the body: so that we make an effort to remove the injured part from the irritant.—T. R. Allinson, L.R.C.P., &c., 2, Kingsland-road, E.

[90]—The best theory is, that the earth is an electro-magnet; currents of electricity travelling round at about right angles to the axis. It is based on the fact that lodes of metals generally lie in the same direction, and they would be able to keep up thermo-electric currents at the expense of the earth's heat. This theory does not explain magnetic variation.—C. T. B.

[105]—A Question of Grammar.—"A. T. C." will, perhaps, understand the phrases, "I can but think" and "I cannot but think," by remembering that the word "but" is used in three senses:—(1) As an adverb = "only," as in "I can but think"; (2) as a conjunction, its most common use; (3) as a preposition = "except," as in "I cannot but think"—this expression being elliptical, for "I cannot do any other thing but think." In this last case (which, by-the-bye, some older grammarians ignore) "but" is the representative of the Anglo-Saxon *butan* = without; and is used by Chaucer in the lines:—

"But meat or drinke, she dressed her to lie
In a dark corner of the house alone."

The broad Yorkshire dialect has such expressions as "I can do bout it"; where the "bout" is not merely a corruption of "without," but the old Anglo-Saxon "but."—J. H. L.

Our Mathematical Column.

THE WITCH OF AGNESI.

[Reply to E. H. R., Mathematical Query, 2.]—The Witch of Agnesi is a curve of the third order described in the "Analytical Institutes" of Maria Gaetana Agnesi (1748). It may be thus drawn geometrically :—

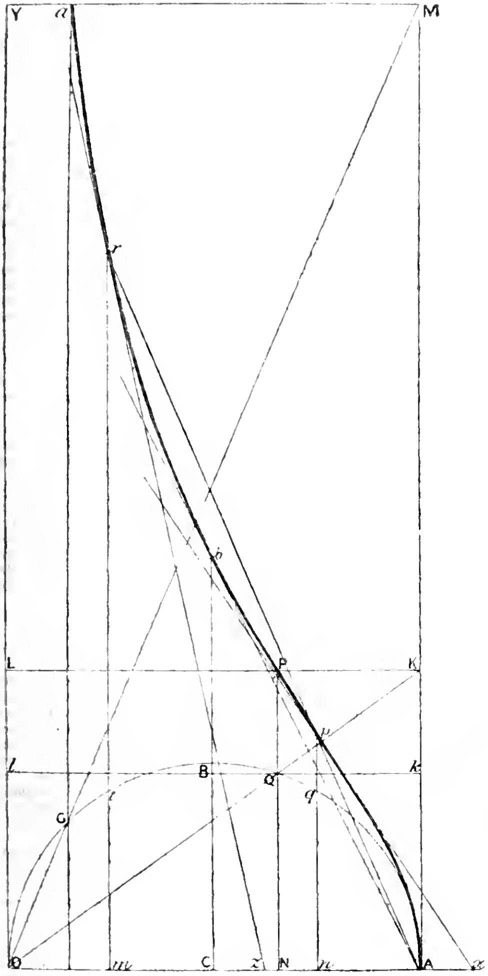


Fig. I.

Take a circle OBA and draw a diameter OA; then if QN perpendicular to OA be produced to the point P, such that

$$NP : NQ :: AO : NO$$

P is a point on the "Witch of Agnesi."

It is evident that the curve will extend indefinitely on either side of OA (we only show one branch); and that the tangent to the circle ABO, at O, will be an asymptole to both branches. Again, if C be the centre of the circle; OC, CA, bisected in m and n; and BC, qn, and tm, perpendicular to OA, are produced to meet the curve in b, p, and r, it is obvious that

$$bC = OA = 2a \text{ (putting } OC = a)$$

$$pn = \frac{OA}{On} \cdot qn = \frac{4}{3} qn = \frac{2}{3}\sqrt{3}a$$

$$rm = \frac{OA}{Om} \cdot tm = 4tm = 2\sqrt{3}a = 3pn$$

Thus, a straight line drawn from A to p and produced will pass through r.

Again, if we draw AkM a tangent at A and LPK, lQk parallel to OA, it follows from the fundamental property of the curve that

$$\text{rect. } LN = \text{rect. } lA,$$

whence Q is on the diagonal OK. Thus, we have a simple geometrical method of drawing the curve, as follows :—

On AM take any point K, and join OK cutting circle OBA in Q; then parallels KP, QP to OA and OY, intersect in P a point on the curve. (We give the construction for the point a on the curve, joining MO, which cuts OBA in G, and drawing the parallels Ma, Ga.)

For the equation to the curve, take OA as axis of x, and OL as axis of y. Then, we have—

$$ON.PN = OA.QN; \text{ that is}$$

$$xy = 2a\sqrt{2ax - x^2}; \text{ or } xy^2 = 4a^2 (2a - x).$$

From this equation to rectangular co-ordinates we can deduce the properties of the curve. Differentiating—

$$\frac{dy}{dx} = -\frac{2a^2}{x}(2ax - x^2)^{-\frac{1}{2}} \qquad \text{(i)}$$

$$\text{and} \quad \frac{d^2y}{dx^2} = \frac{2a^2}{x^2}(3a - 2x)(2ax - x^2)^{-\frac{3}{2}} \qquad \text{(ii)}$$

From (ii) we see that $\frac{d^2y}{dx^2}$ disappears when $3a - 2x = 0$, or $x = \frac{3a}{2}$; so that there is a point of inflection at p.

Putting x successively equal to $\frac{a}{2}$, a, and $\frac{3a}{2}$ in (i) we find :—

$$\frac{dy}{dx} \text{ at } pt. \ r = -4a\left(a^2 - \frac{a^2}{4}\right)^{-\frac{1}{2}} = -\frac{8}{\sqrt{3}}$$

$$\frac{dy}{dx} \text{ at } pt. \ b = -\frac{2a^2}{a}\left(2a^2 - a^2\right)^{-\frac{1}{2}} = -2$$

$$\frac{dy}{dx} \text{ at } pt. \ p = -\frac{4a}{3}\left(3a^2 - \frac{9a^2}{4}\right)^{-\frac{1}{2}} = -\frac{8}{3\sqrt{3}}$$

From these values, remembering that

$$rm = 2\sqrt{3}a; \ bC = 2a; \text{ and } pn = \frac{2a}{\sqrt{3}},$$

we see that the tangent at r cuts OA at a point z, such that $mz = \frac{3a}{4}$; the tangent at b passes through A; and the tangent at p cuts OA at a point x such that $nx = \frac{3a}{4}$.

A geometrical construction for finding the point x in which a tangent at a point p cuts the axis of x is easily obtained. For we have $\frac{dy}{dx} = -\frac{2a^2}{x}(2ax - x^2)^{-\frac{1}{2}}$ whence since $qn = \sqrt{2ax - x^2}$ it follows that

$$\frac{pn}{nx} = \frac{OA.OC}{On.\ qn}, \text{ or } \frac{pn.On}{nx} = \frac{OA.\ OC}{qn}$$

$$i.e.\ \frac{OA.\ qn}{nx} = \frac{OA.\ OC}{qn}; \text{ or } (qn)^2 = OC.\ nx.$$

Hence, if we take $nm = OC$, a perpendicular at q to mq will cut OA (produced, if necessary) in the required point x, such that xp is a tangent at p.

We have not by us any treatise bearing on the history of this curve, and are, therefore, unable at the moment to state the properties which led to its invention. In Brande's "Dictionary of Science and Art" the definition of the curve is given, and we have based on that the above investigation. Most probably the curve was one of those which mathematicians were fond of inventing (in the 17th and 18th centuries) for the purpose of squaring the circle. (Such a curve they would call a quadratrix.) It is manifest that the curve can readily be swept out mechanically. Thus, let there be a semicircular groove OBA (Fig. 2) and a straight groove OT square to OA, and let bars Nn', Ll slide between the parallels AT, Oa (square to OA) so as always to be parallel to AT and OA respectively. Then if a rod OG swinging around O, carry points along AT and ABO, which respectively bear with them the parallels Ll, $n'N$, a pencil so carried as to lie always at the intersection of these parallels, will trace out the curve APb.

It is readily seen that the curve is a quadratrix. For, take any two points, K, J, near together on OA, and about O as centre take circular arcs Kq', Jq to the semicircle AqO. Draw OqL, $Oq'L'$, as in fig. 2, and parallels $L'P'l'$, LPl, $mP'q'M$, $n'PqN$, giving two points, P', P, on the curve. Draw qn perp. to mM. Let $Oq\,Ll$, cut mM in e' and i respectively, arc $q'K$ cutting Oq in o. Draw the quadrant Ara about

O as centre, cutting OL, OL' in e, e' respectively, and join Jr, Kr' which are obviously perp. to OA (triangle AqO equal in all respects to triangle rJO, &c.) Then triangle $e'q'q$ is in the limit obviously isosceles, $q'o$ perp. to qe', and $e'q = 2\ q = 2KJ$.

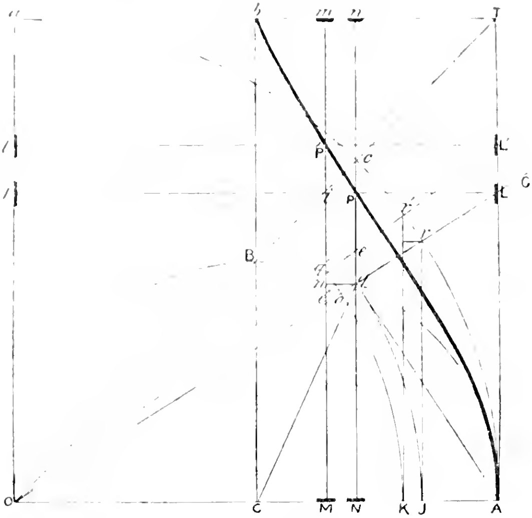


Fig. 2.

$$\text{Now area } PM = PN.\ qn = qN.\ \frac{OA}{ON}\ e'q.\ \cos.\ e\,q\,e'$$
$$= qN.\ \frac{OA}{ON}.\ 2KJ.\ \frac{ON}{Oq} = 2qN.\ \frac{OA}{Oq}.\ KJ$$
$$= 2qA.\ KJ = 2rJ.\ KJ = 2 \text{ rect. } rK$$

This being true of all such areas, as OL passes from OA to OL and onwards, it follows (proceeding to the limit) that

area PNA = 2 area rJA

and entire space between OA, Oa produced indefinitely, and the curve APb carried on indefinitely

= 2 quadrant $AraO$
= 4 semicircle OBA

Therefore, entire area between the "Witch of Agnesi" and its asymptote is equal to four times the area of the circle from which the curve is formed in accordance with its definition.

It may be noticed that we can thus (after a fashion) square the circle, without carrying on the curve to infinity. For, let CB perp. to OA meet the witch in b, and let OB produced meet the quadrant in c. Then we know that

Area $APbC$ = 2 area AcN.
= 2 (sector AOc − triangle cON).
= quadrant AOa − triangle OAT.
= circle ABO − its inscribed square.
= $\pi a^2 - a^2$.

So that if $APbC$ be regarded as determined by this mechanically constructed curve, $\pi a^2 - a^2$ is determined, too; or since a^2 is known πa^2 the area of the circle OBA is determined.

In passing, it may be noted that from the above construction follows a ready demonstration of the property of the tangent, established analytically above. For the limit PP' is a straight line, and the tangent of the angle which PP' makes with OA is ultimately the $\frac{dy}{dx}$ of our analysis.

$$\text{Now tan. } P'PL = -\frac{P'i}{Pi} = -\frac{LL'}{nq}$$
$$= -\frac{q'e'}{nq} \cdot \frac{OL}{Oq} = -\frac{q'q}{nq} \cdot \frac{OL}{Oq}$$

But

$$\frac{PN}{qN} = \frac{OA}{ON} = \frac{OL}{Oq}$$
$$\therefore \text{tan. } P'PL = -\frac{q'q.\ PN}{nq\ \ qN} = -\frac{Cq.}{qN}\ \frac{PN}{qN}$$
$$= -\frac{OC}{(qN)^2}.PN$$

This is the same relation as we have already obtained; for if a tangent at P meets AO produced, in a point x, we have

$$\tan\ P'PL = -\frac{PN}{Nx} = \frac{OC}{(qN)^2}PN \text{ as just shown.}$$

So that $\quad Nx = -\frac{(qN)^2}{OC}$ Ed.]

"G. P." considers that he has solved the very difficult simultaneous equations

$$x^2 + yz = a^2 \text{ (i)} \qquad y^2 + zx = b^2 \text{ (ii)} \qquad z^2 + xy = c^2 \text{ (iii)}$$

by showing, what is in truth obvious, that

$$x^3 - a^2x = y^3 - b^2y = z^3 - c^2z.$$

"It is now reduced to a cubic" (we fail to see this) "which might be solved in a variety of ways. For instance, if we suppose $x = ny$, we get :—

$$y = \sqrt{\frac{a^2n - b^2}{n^3 - 1}}\ ;\ x = n\sqrt{\frac{a^2n - b^2}{n^3 - 1}}\ ;\ z = \sqrt{c^2 - n\left(\frac{a^2n^2 - b^2}{n^3 - 1}\right)}$$

(Where we have written b^2, "G. P." writes 1, through some error.) From which by substituting in (i) (ii) and (iii), we find n, and hence x, y, and z. It is evident G. P. has not tried substituting in (i) (ii) and (iii). If he had, he would have found, first, that substitution in (i) or (ii) would suffice [(iii) giving only an identity], and that the resulting equation in n would be of the 6th degree!

MATHEMATICAL QUERIES.

[5]—Differential Calculus.—"G. P." next asks why, if we take the relation

$$\sin.\ x = \frac{2 - x}{2 + x}$$ and differentiate with respect to x, we get

$$\cos.\ x = -\frac{4}{(2 + x)^2} \text{ instead of } \cos.\ x = \frac{2\sqrt{2x}}{2 + x}$$

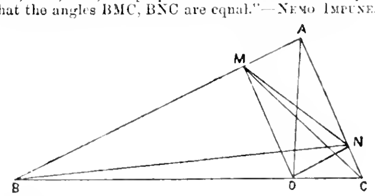
as by trigonometry. "Whence the contradiction?" Simply because "G. P." has applied the differential calculus the wrong way. His relation assigns a definite value to x and to sin x, and that being so, we cannot differentiate. Take a simpler relation, and we may find the differential calculus, applied in this way, contradicting common sense. Thus take $x = 1$, and differentiate both sides with respect to x, and we get $1 = 0$.

[6]—Indeterminate Equation.—Is it possible to solve

$$x = 12 + 2y?$$

to find the value of x and y, without *assuming* any numerical value. —J. A. L. R. [There must always be some assumptions in the solution of indeterminate equations.—Ed.]

[7]—Could you kindly favour me with a solution to the following rider on II. Book of "Euclid":—"The angle A of a triangle ABC is a right angle, and D is the foot of the perpendicular from A on BC; DM, DN, are perpendicular on AB, AC, respectively: show that the angles BMC, BNC are equal."—Nemo Impune.



[As a rider on Book VI., thus:—

Triangle BAC similar to triangle AND,
∴ BA : AC :: AN : ND (= AM).

Hence BA.AM = AC.AN, wherefore points B, M, N, C, lie on circumference of a circle,

∴ angle BMC = angle BNC.

But the problem can be as readily solved without the aid of any properties beyond those in Book III., thus:—Join MN. Then, triangle AMN being in all respects equal to the triangle NDA, the angle AMN is equal to the angle ADN, and therefore to the angle ACD (ACD and ADN being obviously equal, because each with the angle DAN makes up a right angle). Hence the angles BMN and BCN together are equal to the angles BMN and AMN together, or to two right angles. Therefore B, M, N, C are points on the circumference of a circle, and therefore as above

Angle BMC = angle BNC. —Ed.]

Our Whist Column.

FORCING AT WHIST.

Sir,—I gather from the character of your published articles that you presuppose in your readers some knowledge of the theory of whist. So much, in fact, has been written upon whist that it is difficult to find anybody absolutely ignorant of its principles. It is of great value, however, to the young player, to place before him in a condensed form the digested experience of the best players, and thus raise him gradually from domestic to scientific play. "When may I force my partner?" is a question frequently put. There are, undoubtedly, many positions in practice where the thoughtful but inexperienced player finds himself in difficulty. It is easy enough to understand the reasonableness of forcing an adversary who has shown great strength in trumps, or a partner who has shown great weakness. But suppose, for example, as an original lead, a player were to lead from manifest weakness, an honour having been turned to his right, that which in ordinary cases appears to be an invitation for a force, would, in fact, amount almost to a direction to lead through the honour. But I will endeavour to lay down the cases when a player, not having trump-strength, may, nevertheless, force his partner :—(*a*) When, with no indication of strength, he asks for a force. (*b*) When the position shows a cross-ruff. (*c*) When the adversaries have signalled. (*d*) To make the fifth or odd trick, or to save the game, when the hand of the forcing player, or the development of the game, does not raise a high degree of probability that the necessary trick may otherwise be made. But an interesting point relating to the force is where the player, in a position to force, has trump-strength amply justifying it. It often happens that a player renounces to the lead of his partner, who, with ample trump-strength, has no strength in the then declared suit. If he forces, and the declared suit be not headed by Ace King, or King Queen, the result is, after a force, a lead up to ruinous weakness. No trick is gained by the force, for another trick is lost in the suit. If, however, the player gives his partner his declared suit, the adversaries may infer that he has no strength in trumps, and lead trumps to their disadvantage. I send you a game illustrative of this position, and hope it may be interesting to your readers.

Frederic H. Lewis.

THE HANDS.

A.
Clubs—Q, 9, 4.
Hearts—6, 3.
Spades—A, K, 8, 7.
Diamonds—Q, 5, 4.

Y.
Clubs—Kn, 3, 2.
Hearts—A, 10, 9.
Spades—Q, 10, 5, 3.
Diamonds—A, K, 3.

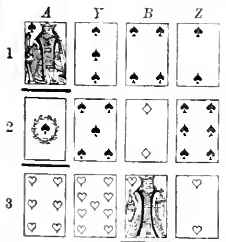
B.
Clubs—10, 7, 6.
Hearts—K, Kn, 7, 5, 4.
Spades—4.
Diamonds—Kn, 10, 6, 2.

Z.
Clubs—K, 8, 5.
Hearts—Q, 8, 2.
Spades—Kn, 9, 6, 2.
Diamonds—9, 8, 7.

B
Dealer. Z
Trump Card, King of Clubs.
A

Score:—A B = 1; Y Z = 4.

Note.—The underlined card wins trick, and card below it leads next.

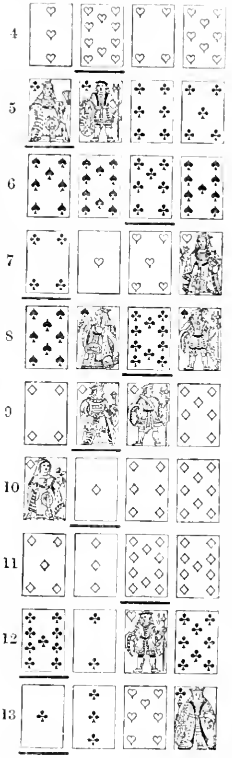


INFERENCES.

1.—*A* leads from his strongest suit.

2.—The fall of the cards, and the discard of Two of Diamonds, the lowest of the suit, shows *A* that no one is signalling for trumps.

3.—*A*, having here two honours in trumps, might very well force his partner, but, having only Six and Three of Hearts, prefers to give his partner his suit.*



4.—*B*, although having no strength in trumps, is here quite justified in returning the Heart. If his partner has avoided forcing him, in consequence of weakness, the chances are that the game is lost. If, however, a cross-ruff can be secured, such a position would be most advantageous.

5.—*Y* sees that a cross-ruff must be secured. He has the tenace in Spades, the best Heart, an honour has been turned; he has the command in Diamonds, and *A* has avoided forcing *B*. All this is too much for weak human nature, and he cannot resist a trump lead; he therefore leads the highest of his three trumps.

6.—*A* is now in a position to force his partner advantageously.

7.—The cross-ruff: conveying also to *A*'s mind, from the fall of the cards, that the two remaining Hearts are with *B*.

8.—*A* is in a position to give another force.

9.—*B* cannot lead one of his long Hearts, such play would be very bad, and accordingly he leads the best of his remaining Diamonds, treating the suit as though he had originally but three.

10.—*A* now has the tenace in trumps, but he requires three tricks to win the game. If he keeps the Queen of Diamonds he can make only two; he, therefore, cleverly throws the Queen, taking the chance of his partner having the Ten of Diamonds. The fact being so, he secures four by cards and the game. The rest of the hand plays itself.

* If he had forced him, the game would, probably, have proceeded as follows :—

	A	*Y*	*B*	*Z*
3.	S 7	S 10	C 6	S 9
4.	H 3	H 9	H 5	H 2
5.	D 4	D K	D 6	D 7
6.	D 5	D A	D 10	D 8
7.	D Q	D 3	D Kn	D 9
8.	S 8	S Q	C 7	S Kn
9.	H 6	H 10	H 4	H 8
10.	C 4	H A	H 7	H Q
11.	C 9	C Kn	C 10	C 5
12.	C Q	C 2	H Kn	C 8
13.	C A	C 3	H K	C K

and the result would be two, instead of four, by cards.

Answers to Correspondents.

*** *All communications for the Editor requiring early attention should reach the Office on or before the Saturday preceding the current issue of* Knowledge, *the increasing circulation of which compels us to go to press early in the week.*

Hints to Correspondents.—1. *No questions asking for scientific information can be answered through the post.* 2. *Letters sent to the Editor for correspondents cannot be forwarded; nor can the names or addresses of correspondents be given in answer to private inquiries.* 3. *No queries or replies savouring of the nature of advertisements can be inserted.* 4. *Letters, queries, and replies are inserted, unless contrary to Rule 3, free of charge.* 5. *Correspondents should write on one side only of the paper, and put drawings on a separate leaf.* 6. *Each letter, query, or reply should have a title, and in replying to letters or queries, reference should be made to the number of letter or query, the page on which it appears, and its title.*

W. We cannot afford space for your long letter, containing only assertions without evidence. Newton waited nineteen years before advancing his theory of gravitation, because, though it agreed in all other respects with observed facts, it would not stand one test—the reason being that the accepted measurement of the earth's globe was incorrect. Cannot Mr. Crosland's friends allow his astronomy of the future to wait until it has been shown that it accounts in measure and number for at least one single observed fact? We promise to give all the space you ask for vague generalities, for the first demonstrated case of the kind.—H. J. Pearson. (1) Strictly speaking, the exact sciences are those which are based on mathematics, as optics, astronomy, &c.; but the term is usually extended to all sciences depending on observation and experiment, so that it includes chemistry, and other physical sciences not strictly exact. The term is not a well-chosen one. (2) It cannot be "demonstrated" that the nearest way from one point to another is the straight line joining the two points; but we show that the latter path is shorter than any path made up of straight lines, and since a series of very short straight lines may be made to approximate as nearly as we please to a curved line, this can at any rate be demonstrated, that a curved line from one point to another differs by an indefinitely small amount from a length which is greater to an assignable degree than the straight line joining the two points.—Thos. Smith, Jun. Phrenology is not so young that, were it really a science, it should be unable to take its own part. It is much older than spectroscopy, which can stand alone very well. We did not, however, mean that phrenology is absurd, when we spoke of the absurdities of phrenology; there are reasonable features as well as absurdities in it. Much may be said in favour of a rational phrenology. Gall and Spurzheim were both scientific observers, and both made useful scientific discoveries; but their system of mapping out the cranium has now no adherents among men of science.—W. J. M. We have ourselves seen and heard a dog which was said to speak; but one had to make believe a

g... deal. Thus if ... (French dog) he would make a noise about as near to the sound of the word as the note of the cuckoo is to the word cuckoo, but no nearer. If one had been told he said "hurt," the noise would have done just as well for that word, or for half-a-dozen others. In some such way, doubtless, the dog "seen and heard by M. Leibnitz" could say, in German, tea, coffee, &c. We had a dog once which could sing, but that required a good deal of "make believe" too.—John Steele. Your explanation not necessary, but well received. Science no more questions the sun's heat than we question the heat of a fire which warms us.—Altair. Query 61 already answered. Thanks. As regards 62, Algol is shown in the maps for this month. It is scarcely a sufficient answer to 63 to note that Algol and Mira are brightest when at their maximum brilliancy. Note our reply to 61.—Q. E. F. Your own figure should have shown you that your solution is unsound, for the angle is obviously not trisected.—Ridiculus Mus. Your first result amounts to this, that if you have made a certain number of moves in the Fifteen Puzzle, you can come back to the position from which you started; your second, to this, that starting from a certain position and making any number of moves, you cannot work back to a certain other position, closely resembling, but not identical with, the former. But this does not deprive the puzzle of its interest as a mathematical enigma. You can hardly have proved "by numerous experiments" (only) that these results are invariable, for there are twenty billions of different positions which can be attained from one or other of your two starting positions. If you had, (that is, if you had worked at the puzzle ten times or so a day, during a life of some twenty thousand years) there would still remain the problem—pretty, though not difficult—of showing why these results can invariably be attained. Then there is the neat puzzle in every case, how to attain any assigned position from another of the same order, in the smallest number of moves. On the whole, it is as pretty a little puzzle, I think, as any invented since "Solitaire," which involves some very neat mathematical relations.—Chromo. Thanks; but questions sufficiently answered already.—M. E. Pendred. Your fears were expressed for others, but seemed to be yours. Anyhow, we know your letter would have raised most undesirable controversies.—F. H. F. The lunar illusion is not dependent on the vastness of the sun's distance. "M.'s" explanation was sound. The plane through the observer's eye and the horizontal sagitta due south passes through the east and west points on the horizon. Would print your letter did space permit.—H. Rayment. Hope shortly to insert your letter.—An Admirer. You are very good, but fear readers of Knowledge would not quite like it.—E. Burgess. Thanks for "Help to Chess Students." Crowded out at present.—Athos. Of course you are right in saying that the solar dark lines, as interpreted, prove the sun to be intensely hot. The argument would be above comprehension of the paradoxers, however.—Rob. S. Couch. Letter shall shortly appear, if possible.—G. W. Niven. Short notes on such tricks would suit well enough.—S. N. B. G. No space at present. Thanks for replies.—A. L. S. Thanks for account of lunar rainbow (why call it Iris Lunaris?). No room for it at present.—J. Fraser. Still hold the accepted theory of the tides. No earthly reason why the earth should fall into the moon. Your theory wholly untenable.—T. Thanks.—F. W. C. Cannot insert answers recommending instruments by particular makers. We are sure of your *bona-fides*; but others would claim right to do likewise with less worthy motives.—J. M. Many thanks; but the question had been answered sufficiently, though we could not find room at once for reply. W. F. Denning. Thanks; your letter shall appear.—Carlo F. Johnson. Thanks; but we take replies respecting comets as part of subject we are discussing in articles—if only we could get a line or two in edgewise. Other questions answered.—H. G. Hollingworth. Will try to find room.—D'Artagnan. Airy's Essay on "Undulatory Theory of Light" (in his mathematical essays), Lloyd's, and Potter's treatises were what we used to read at Cambridge.—Inquirer. Moore's "Lalla Rookh" is not a recognised astronomical authority; still "half retired" (from the earth) would correspond nearly enough with time when Venus "looks loveliest."—S. N. W. You evidently have not yet grasped the meaning of the modern theory of evolution. Your remarks apply well to such theories as Lamarck's and others; but they are dead; why play Falstaff at Shrewsbury over them?—J. D. "As fast as we fix a telescope," you say, "the earth's motion ought to displace it." That is just what the earth's motion does. It is this motion that the driving clock of a telescope is devised to correct, by constantly carrying the clock the other way.—F. H. S., J. Gottfried, and others. Thanks; "Magic Squares" crowded out for the present.—L. Ferrers. Received; thanks.—Mabel W. L. Pressure of time was the cause of printer's error; but it was of the less importance, as Brewster's ideas were entirely incorrect.—S. H. F.—We prefer our "happy laughter" to be independent of comparatively "low replies." Your method of studying the luminary in question is unsound.—J. Gottfried. Thanks for your magic squares; the method shall appear. There are others yet.—Andrew Aitken. Your answer to Cymro's question is sufficiently sound, but we have not room for second solutions. The first was correct. The first part of answer to Query 48 is also omitted, as the trisection of a right angle is familiar to all students of geometry. The other solution is really the same as that on p. 117, but if our space were not so crowded we would give it. Thanks for your communication all the same. We must ask you to consider our position with regard to space. Note beginning of our letter in correspondence column, and reply to W. R. W. below.—James Shaw. Article on "Right-handedness" shall appear shortly.—Eclecticus. We give you alone this *nom de plume* now. Have been obliged to much shorten your letter.—W. H. R. Thanks, but longer letter on meridional or meridinal parts could not be placed. There is no Latin word *meridionalis*. *Meridianus* is the word. Albeit we leave such points to the printers.—W. T. Lynn. Thanks, but the exact time when Halley's comet was first observed in 1759 is not likely to interest the majority of our readers, and space considerations trouble us. We put in your first letter, because it involved a correction of our own statement. Otherwise, these questions of dates, priority, manner of observation, and so forth, seem of little importance. How often has the error about Tycho Brahe and the new star been repeated, and what harm has it done? We must not be microscopical in such matters.—Sirius. To save space, have been obliged to incorporate your letter in our own.—Alex. J. E. Your 3-square puzzle is very neat.—Musicus. Why want to learn how to "get in the way of talking" while you play the piano? Did you ever hear anyone do so who had the least feeling for music? None such can play well when *others* talk. Probably, it is as easy for a mere "performer" to talk while he plays as to talk while someone else is playing, and as difficult (rather as impossible) for a real musician to do one as the other. To anyone with the slightest particle of musical feeling, there can be nothing much more trying than to hear people talk while a noble air is nobly rendered, unless it be to hear a skilful executant talking while the notes of a noble air pass without life or expression under his agile but not musical fingers.—E. F. H. Thanks.—Herbert R. Welter. Unquestionably a meteor. Some meteors move, apparently, quite slowly, and the larger often have the cometic appearance you indicate.—H. A. S. "Puzzled's" problem is sufficiently explained. It is much older than the publication you mention, though, doubtless, new to him. Your other problem, depending on half a live man being equal to a man half dead is amusing, but would be out of place here.—K. J. P. Why will correspondents take so much care to make their communications too long for insertion?—A Biologist. Your reply to No. 52, if inserted, would lead to others being sent, which might read like advertisements. Thanks equally.—J. H. W. L. We ... see when first half-yearly volume approaches completion. Thanks for getting new subscribers.—C. J. Chambers. Your suggested paper would range too much, we fear, over the ground covered by text-books of Acoustics.—F. J. Brodie. Correction inserted. Thanks. Should receive such notes as you suggest with pleasure. Brevity essential, you will understand.—J. F. S. Thanks for extracts; several we had already cut out, but correspondence crowded them out. Note letter from W. M. Williams, and consider the cheerful task of weeding out and boiling down such abstract reports.—Camille Flammarion. Thanks for your obliging offer, of which we gladly avail ourselves. Book safely to hand. Again thanks.—Edward P. Thompson. Thanks; yet the quotation would hardly add to our knowledge: it tells us only what everyone knows—that fancy often mimics reason.

> But misjoining shapes
> Wild work produces oft, and most in dreams
> Ill matching words and deeds long past or late.

G. Cuttle. Your reply to Query 90, No. 6, is hardly explanatory. The question really is why the earth acts as you say—justly enough—she does act. By-the-way, the magnetic poles are no longer where you say. Changes both in the declination and inclination all over the earth sufficiently prove that these poles have shifted largely since the northern was determined and the southern inferred.—Paugul. Many thanks for your replies; a few, as relating to queries already answered, we omit, space being crowded. The paradoxers think me too hard on them, instead of "too indulgent."—R. E. W. We omitted the third extract, but failed to omit the reference to it.—Rev. W. H. K. S. Your explanation of ice-yachts sailing faster than the wind is not available in many known cases, where the relative wind is less than the absolute wind, and the velocity of yacht exceeds that of either. The principle of the Rhine ferries is different. Thanks.—J. H. Lampkey. Unquestionably, the "Knowledge of the Ancients," would be full of interest for students of modern knowledge; and occasional papers on the subject might suit us. But we cannot, as yet, arrange for the systematic treatment of ancient knowledge here. If you send us a short paper you may rest assured it will receive due attention.—Rev. W. Houghton. Thanks for your interesting letter, which shall appear in full.—J. Taylor. Your suggestion attended to; see "Dates and Hours for Stars in November," in this column below.—W. R. W. Are you not a little unfair? There was, of course, less correspondence in No. 1, than later; it would not have been strange had there been none. In No. 6, the correspondence occupied too much space, but most of the letters were interesting. Two were specially written for us, and might have been put among the original articles, only their writers modestly suggested that the matter should be put among the correspondence. You must remember that we have to consider those who ask questions, or feel moved to make replies. You rather miscount the pages in No. 1 and No. 6, including the star map in one estimate and omitting it in the other. However, No. 6 was exceptionally overweighted with correspondence. Hereafter we must be more strict, though, as it is, we are rebuked for hard-heartedness.—R. G. We cannot undertake to give the accents of technical terms. Miss Kingsford's statements claim courage for fruit-eaters as well as strength. Roman and Spartan soldiers were not altogether wanting in pluck. Many cases are recorded of flesh-meat acting as an intoxicant, even to people in sound health. However, the Indians who gesticulated may have been intoxicated otherwise. The question whether it is the use of flesh-meat which has raised man above his fellow Primates is, of course, open to discussion. It scarcely seems probable, in the face of the admitted high intellectual and physical standing of many races which eat no flesh.—Rev. J. H. L. I have had very little to do with fixing the price of my books, and doubtless it is the same with other writers. I wish the publishers of books on science could find reason, in the increased number who take interest in science, to lower prices.—T. R. Allinson. Have slightly shortened your reply; space inexorable.—Arnold Ramsden. You can hardly do better than get Madler's smaller map of the moon, or Webb's "Celestial Objects for Common Telescopes," in which there is an excellent map, based on Madler's. Webb's book is about 10s.; Madler's map alone, from the German publishers, would cost, I suppose, a shilling or so.—Thomas Thorpe. Your ingenious instrument to appear in early number.—J. J. F. The mistakes of T. P. (A. K. C.) corrected elsewhere. Thanks.—Thomas H. Bamford. It is an open secret that Dr. Robert Chambers was the author of the "Vestiges of Creation." It would only waste time to discuss the claims of Lord Byron's daughter. By the way, no one who has read Lyell's remarks on the book in his "Antiquity of Man," could for a moment suppose he was the author.—G. C. D. M. We perfectly agree with your remarks about corporal punishment for boys. The cases are very few indeed where due obedience cannot be obtained without such punishment. But we cannot quite hold with you, that at birth the predilections for evil and for good in a child are exactly equal. Children differ singularly. Certainly George Eliot's skill as a novel writer would not of itself show that her brain was abnormally developed, but her novel-writing was only the means by which her real brain power was shown. Pray excuse non-appearance of your letter in full. The subject has now been pretty fully treated, and readers are asking that more space be given to articles.—Lionel T. Waller. Hope to give weekly weather reports shortly, if space can be found.—Wisray. Will see whether it can be done without injuring the maps. Doubt it.—A. J. Shilton, Not Castor and Pollux, and Others. Thanks. Query 54 sufficiently answered.—A Well-wisher, and others, repeat a request to which we have several times responded, that advertisements should be printed so that the bound volume need not contain them. This is already provided for: no advertisement page is now numbered so as to form part of the bound volume. Compare Nos. 1 and 2.—Ed.]

Dates and Hours for the Stars in November.—These are as follows in the first four numbers:—Oct. 30, at 10¼ o'clock; Nov. 3, 10¼ o'clock; Nov. 7, 10 o'clock; Nov. 10, 9¾ o'clock; Nov. 14, 9½ o'clock; Nov. 18, 9¼ o'clock; Nov. 22, 9 o'clock; Nov. 25, 8¾ o'clock; Nov. 29, 8½ o'clock; and Dec. 3, at 8¼ o'clock.

Contents of Knowledge *No.* 6.

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CONTENTS.

THE SO-CALLED ELEMENTS.

By Prof. C. A. Young.

My own spectroscopic observations bearing on the elementary constitution of matter have not been very numerous or important, as compared with those of several other investigators; at the same time, they have had a place in the discussion of the subject, and I cheerfully comply with the Editor's request to present a short account of them to the readers of Knowledge.

The idea that our so-called chemical elements are not really and absolutely elementary, but are built up either of atoms of some one universal matter, or of some few substances of a higher order of simplicity than themselves, is neither improbable nor new. As Mr. Lockyer points out, Dumas in 1836, and Brodie in 1867, not to mention others, have expressed themselves very clearly in this direction.

But the present special interest in the question is due mainly to Mr. Lockyer himself, who, in 1873, by an able and extensive induction from all available spectroscopic observations, put the theory in a new light, and brought together in its favour a great body of evidence, to which he has since then been continually adding.

In the discussion of the subject, certain observations of my own have been made use of along with those of others, always, I believe, with due and courteous acknowledgment, and in a manner to which I have nothing to object, unless, perhaps, that too much weight has sometimes been given to certain numbers which I had assigned only provisionally, and on a very short experience.

These observations of mine were for the most part made in July and August, 1872, during a six weeks' stay at Sherman, the summit station of the Union Pacific Railroad, at an elevation of 8,300 feet above the sea level. The party was connected with the organisation of the Coast Survey, and the results of the observations are published in the "Coast Survey Report" for 1872.

The work consisted mainly in a record of the bright lines observed in the spectrum of the solar prominences, and of the lines which were widened or otherwise modified in the spectra of sun-spots. The resulting catalogues contained respectively 273 and 156 lines, but only that portion of the spot spectrum lying between B and b was included. In these catalogues an attempt was made to assign numbers indicating the relative intensity of the lines and their proportional frequency of appearance; but, as has been intimated, the number of observations was far too small to make such figures very reliable.

Several remarkable facts were, however, very clearly brought out. In the first place, those lines of the spectrum of any particular substance (iron, for instance) which are most conspicuously and frequently seen, as *bright* lines in the spectrum of the chromosphere and prominences, are not generally the same as those most notably *widened* in the sun-spot spectrum; nor are either of these the ones which are most prominent in the ordinary solar spectrum or in the spectrum of the electric arc. Each of these four spectra selects and emphasizes, so to speak, its own set of so-called iron lines, and the same is true of other substances. Second: Where distortions and disturbances of the lines indicated rapid motions of the solar gases along the line of sight (approaching to, or receding from, the observer), it frequently happened that neighbouring lines, due to different substances, were differently affected; certain lines of iron might be violently displaced, while the adjacent lines of calcium were not disturbed at all. In some instances, also, it was noticed that lines, given in the maps as belonging to the same element, and actually in the same field of view, behaved very differently; one of them indicating violent motion of the gas concerned, the other showing nothing of the kind. But this observation seemed at the time so strange, and the instances were so few, that, in view of the possibility of mistake, it was not thought best to publish it without further confirmation, which it has since abundantly received.

In the third place, and most important, it was found that a very surprising proportion of the lines conspicuous in the spectra of the chromosphere and sun-spots are lines which on the maps are given as common to the spectra of two or more elements—lines which Mr. Lockyer has since designated by the term "basic." Nearly one-third of all the lines in the two catalogues belong to this class, while in the ordinary solar spectrum, they amount to less than four per cent. of the whole.

At that time the prevailing opinion, I think, was that these common lines were due to impurities in the specimens of the metals employed in mapping the spectra. If, for instance, in making a map of the spectrum of iron, the bits of iron used for the electric spark to play between (technically the "electrodes") were slightly contaminated with calcium, then we should expect the more important lines of calcium to appear faintly in the map of the iron spectrum; and the difficulty of getting perfectly, or even "spectroscopically," pure iron is enormous.

It was evidently possible also that these coincidences might be such only in appearance—cases of accidental juxtaposition too close for resolution by the existing spectroscopes.

But the instances were so numerous, that, on the whole, it looked more probable that the true explanation of such coincidences would be found in something common to the elements concerned; and at the time, I thought a similarity of molecular structure more likely than a community of substances. It appeared very possible that metals so closely alike in many of their properties as iron and chromium, for instance, might owe this general resemblance to such a likeness of their molecular architecture as would also carry with it certain identical vibration-periods. If atoms are vortex-rings in a continuous medium, the likeness might be in the size and configuration of the rings; if, on the other

hand, we are to think of the ultimate atoms as little hard balls, all exactly alike, and the chemical (elementary) molecules as aggregations of a greater or less number of these atoms, variously arranged, it would be easy to imagine some such likeness in the molecules of allied substances as would account not only for the spectroscopic facts, but many others.

Mr. Lockyer, however, after experimentally disposing of the theory that these "basic" lines are due to impurities, has preferred the hypothesis that what is common to two elements which show certain identical lines in their spectra, is some *constituent substance*, found in both, and liberated in greater or less proportion with change of temperature; and he finds in the existence and behaviour of these lines one of his strongest arguments in favour of the compound character of the chemical elements.

But I fear that the foundations of this argument are insecure, however it may be with the other evidence upon which he rests his case.

My observations in 1872 were made with a spectroscope inferior to very few then in use. It had a dispersive power equivalent to that of a train of twelve flint glass prisms of 60°, with collimator and view-telescope of 10 in. focal length. In optical perfection I have never seen, and do not expect to see, its superior: of course, it easily showed every line laid down upon the maps of Kirchhoff and Angstrom, and a multitude beside.

But soon after this time Mr. Rutherfurd's diffraction gratings began to come into use: and as they were made of continually increasing fineness and excellence, something like a new world opened before observers in respect to the details and structure of the solar spectrum. Up to 1877, no gratings were made with a ruled surface exceeding about one square inch in area; but in that year Mr. Chapman (Mr. Rutherfurd's mechanician), at my request, and with Mr. Rutherfurd's hearty concurrence, altered the machine so that it could draw a line $1\frac{3}{4}$ in. long, and ruled for me three plates on speculum metal, with diffraction surfaces of nearly four square inches. One of these new plates allows the use of so much larger object-glasses, that, by giving a suitable focal length to collimator and telescope, the available dispersion is increased four-fold over that obtainable from the smaller plates, with the same apparent brightness of the spectrum. Perhaps I may mention in an aside that one of our most distinguished American physicists has now in hand a machine, nearly completed, and bidding fair to be perfectly successful, which rules lines four inches in length. With this he expects to make gratings having a ruled surface 4 in. by 6 in., and with 20,000 lines to the inch, or even finer. *Quod faustum sit.* One would be almost ready to die after a good study of the solar spectrum with such a grating and accessories to match. But with one of Mr. Chapman's best gratings, $1\frac{3}{4}$ in. by $2\frac{1}{2}$ in., containing over 10,000 lines (17,300 to the inch), combined with collimator and telescope of 42 in. focus and 3 in. aperture, and a magnifying power of 250 (which is perfectly borne under the best conditions, when the grating is in good humour, perfectly flat and of uniform temperature throughout)—with this instrument, I say, one reaches about the limit of present abilities. Hundreds of lines shown as single in the older maps of the solar spectrum turn out to be double, triple, or multiple, and the vacant spaces of the spectrum are filled with crowds of fine lines and details of shading before undreamed of.

When attacked with this instrument, nearly every one of the "basic" lines of the solar spectrum is resolved; out of seventy such on Angstrom's map, only seven withstand its power, and three of these seven are probably on the "basic" list by mistake, as the numerical tables of Thalen are at variance with the map regarding them. With an instrument almost identical with my own, Professors Living and Dewar have recently investigated the electric-arc spectra of nearly all the metals involved. Their results accord with mine in almost every instance. They find that the apparent coincidences between the lines of different substances always break down under powerful dispersion; so that as matters now stand, I see no evidence to be drawn from such coincidences in favour either of Mr. Lockyer's view, or of the slightly-different hypothesis which I advocated myself.

The spectra of different metals, so far as we can now make out, have absolutely no points of exact agreement.

As to the remarkable and disproportionate number of these apparently common lines which are found in the catalogues of chromosphere and sun-spot spectra, may not the explanation be substantially as follows:—A line which is really composed of two or more belonging to different elements is much more likely to catch the eye than others. In the first place, this line will appear when either of the metals vigorously reverses its lines, even though the other does not; and again, in cases where both metals reverse their lines, but too feebly to be detected against the background of the atmospheric spectrum, this line, of double brightness, will be clearly seen. Since, probably, all, or pretty nearly all, of the lines are actually reversed close to the sun's limb, though only a few are usually bright enough to be caught by ordinary instruments under ordinary atmospheric conditions, it is easy to see that a slight advantage of the kind indicated above would give a composite line a great lift in the scale of relative frequency and brightness.

I have not yet been able to apply the highest obtainable dispersion to the examination of the spots and prominences, as the great spectroscope is too large to be attached to the eye-end of our 12 ft. equatorial. I have, however, used the same grating with collimator and telescope of 12 in. focus, giving about one-fourth the resolving power of the large instrument. Even this is more than four times as powerful as the prismatic spectroscope used at Sherman, and with it, it becomes perfectly clear that the catalogues of prominence and sun-spot lines contain many serious errors, the correction of which will be likely to remove some puzzling anomalies. In a number of cases, lines which are noted in the catalogue as bright in the spectrum of a prominence, or thickened in that of a spot, turn out to be entirely unaffected, the real culprit being a neighbouring line, so fine and so close to the larger one, as to be absolutely indistinguishable from it with the old instrument.

When our great equatorial (of 23 in. aperture, and 30 ft. focus) is mounted, as it will be next spring, it will be able to carry the large spectroscope without difficulty, and I shall then hope to review the catalogues with power enough to settle most of the questions of this sort. Until such a review is made, it seems to me that generalisations founded on our present data must be very cautiously handled.

WHAT IS A GRAPE?

By Grant Allen.

THEY make a beautiful picture, these big English hot-house black Hambros, with their purple bloom and their waxlike texture, clustered thickly together in rich luxuriance on their slender and heavily-weighted branching fruit stalks. Indeed, we have now cultivated them to such a pitch of excellence, that their old wild ancestors would hardly recognise them to-day for members of the same

original woodland family of Oriental climbers. Yet, after all, we have only been able to carry a little further, by careful selection and tillage, the peculiarities which Nature had long since produced in the primitive native stock. At best, man can only develope more fully what the plant itself has well begun. Our ornamental flowers are but the handsomest chosen wild blossoms; our cereals and edible roots are but the starchiest wild seeds and tubers; and our garden fruits are but the pick of the hips and haws and wayside berries, improved and altered by ages of cultivation.

The grape-vine, they say, comes to us originally from the shores of the Caspian. Even in its native condition it produces little sweetish acid grapes, hanging in purple clusters among its green festoons. The question is, then, of what use to the plant itself are these juicy fruits? For we now know that whatever use man may make of this, that, or the other organ in any particular plant or animal is, so to speak, an accidental after-thought; the organ always subserves besides some useful purpose in the economy of the plant or animal itself to which it belongs. Now, of course, the main use of all fruits is to produce or contain the seeds. They are merely seed-vessels, and, in most cases, they are dry and brown when ripe, like the pea-pod, the poppy-head, or the capsule of the mignonette. The problem we have to answer in the case of the grape is therefore this: Why should it be pulpy and prettily coloured, while these other fruits—and, indeed, the vast majority of all fruits—are mere dry and unattractive organs?

The analogy of red and white and yellow flowers affords us a good hint towards the solution of this problem. We know that flowers have acquired their bright hues, their honey, and their perfume, for the sake of attracting the insects which fertilise them by carrying pollen from head to head. Is there any way in which fruits can similarly benefit by alluring the eyes of any animal race? At first sight this would seem impossible; but a little consideration will show us a way out of the difficulty. Most plants, it is true, can only lose by allowing their seeds to be perceived and eaten by animals. In such cases the fruit, be it pod or capsule, is usually inconspicuous in colour, and drops its tiny little seeds quietly out upon the ground beneath. Those plants which best succeed in diverting the attention of seed-eating birds or mammals from their fruits, outlive, in the long run, their less adapted neighbours; and so the survival of the fittest has brought it about that ninety-nine kinds out of a hundred in our own day have unnoticeable little green or brown seed vessels, such as those of the chickweed, the pimpernel, and the clover tribe, which nobody but a botanist ever observes at all. Suppose, however, that any plant happens to have its seeds covered with a moderately hard and indigestible outer coat, would it not then be rather benefited than otherwise by having these seeds enclosed in a soft and juicy bed of edible pulp? For in that case birds and other animals might eat the seeds, fruit and all, for the sake of the pulpy covering; and as the hard little shell would protect the young embryo within, this vital part would not be digested, but would pass uninjured through the creature's body. By such an arrangement the plant would not only get its seeds dispersed—in itself a most important matter—but would also have the young seedling well manured and started in life under unusually favourable auspices. If such a tendency were ever to be set up even in the slightest degree by a mere sport or chance variation, we may be sure the variety in which it appeared would be so favoured by circumstances, that it would soon become a marked and distinct species.

As a matter of fact, it is pretty certain that such has been the origin of all edible pulpy fruits. Take, for example, these grapes here. If you cut one of them open, you will find inside a number of hard little seeds. Slice one of these again with a sharp penknife, and you will see that it consists of a tiny embryo plant in the centre, surrounded by a very solid bony shell. Each seed is in fact a miniature nut; and the kernel, so to speak, consists of the tiny plantlet within, together with the albumen on which it feeds when it first begins to germinate. Now, if any bird were to swallow and digest this vital part of the seed, the plant would, of course, be an obvious loser. But the hard shell prevents such a catastrophe from happening: and, therefore, the plant is benefited by the soft, eatable pulp which surrounds these little minute nuts. Observe, too, that the fruity part of the grape is sweet; it contains grape sugar. Now sugar is always laid up in those parts of plants which specially seek to attract the animal world. In flowers, the nectar allures the fertilising bees and butterflies; in fruits and berries, the sweet juices allure the birds which disperse the seeds; nay, even the pitcher plants secrete honey to wile flies into their insect-eating cups; and certain acacias store it up in hollow thorns to attract the epicurean ants, which, in turn, protect the tree by driving away their leaf-eating relations. In almost every case, one may say that where sugar is found in any organ of a plant, it is placed there for the sake of engaging the attention of some animal ally; while conversely, all flower-feeding and fruit-eating creatures always manifest a marked taste for sweet substances, dependent upon their long habituation to sugary food.

Not only, however, are the grapes sweet, but they are also brightly coloured. Naturally, among succulent fruits bidding for the attention of birds, those would best succeed which were most visible at some little distance. Accordingly, just as the insect-fertilised flowers have developed brilliant pigments in their petals, so the fruits which depend upon birds for the dispersion of their seeds have acquired prettily-coloured coverings. We all know how noticeable are the hips and haws, the holly-berries and rowan-berries, even among our northern woodlands; while the oranges, mangoes, and pomegranates of the tropics appeal even more vividly to the sharp eyes of monkeys, parrots, and toucans. At the same time, it is noteworthy that the tastes of birds with regard to colour seem to differ somewhat from those of insects; for, as Mr. Wallace points out, white, which is a common colour for flowers, is rare among fruits; while purple and bluish-black, which are seldom met with among flowers, may almost be considered as the ordinary colours of most wild fruits.

Looking closely into my cluster of grapes, again, I see that it still contains two or three unripe and stunted specimens. These, of course, are pale-green, like the leaves, and when I taste one of them I find it unpleasantly harsh and acid to the palate. This reminds me that grapes, like other fruits, are not at all stages of their existence sweet and brightly coloured. While the seeds are still immature, they would only lose by being eaten, because they are not yet fit for germination. In this stage, therefore, the skin is filled with green colouring matter, and the cluster is quite inconspicuous among the foliage which surrounds it. It does not *want* to attract attention in its present stage. Furthermore, the pulp at this period is filled with tartaric acid and other sour juices, to repel any too-inquiring or too-impatient visitor. But as the seeds mature, the fruit ripens—that is to say, a chemical change goes on in the pulpy portion, which results in the formation of grape-sugar. At the same time, other chemical changes taking place in the skin result in the production of the

purple bloom, which advertises to the birds the presence of the sweet juice within. The whole process obviously aims at concealing the fruit and rendering it unpalatable while the seeds are immature, and at making it conspicuous as well as pleasant the moment the seeds are ripe for dispersion. Hence we are justified in concluding that the development of the grape is due to the long selective action of fruit eating birds. Originally, no doubt, the primitive ancestral vine produced smaller and harder seed-vessels, which were probably green when young, and brown when ready to fall upon the ground. But some of them happened to show a tendency towards producing larger and juicier fruits, and these were constantly favoured by the unconscious friendliness of the neighbouring birds. The colour and the sweetness would soon follow, as they have followed a thousand times over in the development of each separate edible fruit. A grape, in short, viewed from the standpoint of the vine itself, is merely a cunning device for ensuring the assistance of birds or mammals in dispersing the little, nut-like seeds of which man takes, as a rule, such scanty notice.

STUDIES OF VOLCANIC ACTION.

By G. F. Rodwell.

PART II.

Of late years the microscopic study of eruptive rocks has revealed many facts of importance. The most opaque black lavas and basalts are seen to be mainly made up of colourless transparent crystals, when a slice less than a hundredth of an inch in thickness is placed under the microscope. Polarised light enables us to distinguish the nature of the crystals, and the angles can be readily measured. Zirkel, Rosenbusch, and Rutley have done much to promote this branch of petrology. One of the most recent and beautiful works on the subject is the "Minéralogie Micrographique" of MM. Fouquè and Michel Levy, which contains more than fifty coloured quarto plates of rock sections seen under the microscope, usually by polarised light. The rocks are, for the most part, eruptive. A peculiarity of the book is an ingenious plan for recognising in a moment the different mineral constituents of the section; for this purpose each plate is covered by a loose piece of transparent paper upon which an uncoloured outline of the engraving is drawn, and each mineral has its own number placed within the outline. The same number is used throughout the book to designate the same mineral.

The frontispiece of Prof. Judd's "Volcanoes" shows six beautiful sections of eruptive rocks, in which the passage from the perfectly glassy to the highly-crystalline structure is strikingly illustrated. The first specimen is that of a volcanic glass or obsidian, a lava which was rapidly cooled from a condition of complete fluidity, and which shows nebulous patches scattered through a glassy base. A very high power reveals that these patches are composed of minute crystals, called *microliths* or *crystallites;* and we are forcibly reminded of the resolution of the heavenly nebulæ into thickly-clustered stars by the microscope's twin-sister. Sometimes the microliths are built up into germ-like forms within the ground mass of the lava; and again, as the crystalline forces come more and more into play, the microliths form radial groups about definite centres, and thus build up globular masses, called "sphærulites." Crystals are made up of microliths grouped about certain axes, and a completely amorphous glassy lava may [illegible]

and thus giving the molecular forces time to act in the grouping of the microliths. Most lavas, when viewed under the microscope, exhibit a glassy paste or ground-mass, containing microliths, among which distinct crystals are distributed. Or again, when lavas consolidate at a great depth beneath the surface, the ground-mass is made up of small crystals, through which larger crystals are distributed. And, finally, we arrive at the granitic structure, in which the rock is completely made up of large crystals without any ground-mass. One and the same rock may exist in each of these forms, according as it has been cooled slowly or rapidly, at a great depth beneath the surface, or near to it. Thus, while *basalt* represents the lava form of a volcanic rock, *gabbro* is its granitic or crystallised form, and *tachylyte* its glassy or obsidian form. They all have the same ultimate chemical composition.

The larger crystals probably separated from the amorphous masses beneath the volcano, and were carried up to the surface by the fluid material forming the ground mass of the lava. Crystals frequently furnish abundant evidence of having been formed under enormous pressure. When examined by high powers of a microscope the crystals of granitic rocks are sometimes seen to contain cavities filled with liquid or gas, or with two liquids and a gas. Sometimes the liquid is water, sometimes a hydrocarbon like the mineral oils which are found in abundance in deep-seated rocks in various parts of the world. Not unfrequently the inclosed substance is liquefied carbonic acid, a gas which requires a pressure of nearly 600 lb. on the square inch to liquefy it at the freezing point of water, and a much higher pressure at the temperature which exists a short distance beneath the surface of the earth. We have thus every reason for believing that the crystals of a rock have been formed in deep recesses in the earth, while the ground mass has solidified at the surface.

Of recent volcanic outbursts, there can be no doubt that the eruption of Etna in May, 1879, was the most considerable. It was studied very fully by Professor Silvestri, who considers that it was the fulfilment of the abortive attempt made by the volcano in 1874. On the 29th of August of that year a rift opened on the N.E. side of the mountain between the crater and Mojo, and thirty-five monticules were thrown up along its course, with one large crateriform mound from which lava was discharged. After seven hours of activity the dynamic forces suddenly decreased, and in two days all the effects ceased, but the rift remained open, and earthquakes were common in the vicinity. Silvestri then predicted that when the next eruption occurred the rift would prove the line of least resistance, and that lava would flow from it, and craters be opened along its course, and this prediction was completely verified in the eruption of 1879. The fissure then extended itself to a length of six miles, passing through the great crater. Eight eruptive mouths opened on the south side of the mountain, and discharged a small amount of lava; but the lava presently found an exit at a lower level on the north side, and on May 28 Silvestri observed a great column of smoke, soon followed by the gloomy leaden tint observed during an eclipse, and by showers of volcanic sand. In ten minutes he collected more than two pounds of this sand in an inverted umbrella. He then approached nearer to the scene of action, and, when about 6,200 ft. above the sea, he heard loud detonations and experienced considerable oscillations of the soil. As he approached the great rift, he noticed three new craters near Monte Nero, from which issued dense clouds of steam. From one of them lava flowed which formed a considerable stream, reaching to a distance

half a mile from the village of Mojo, and then had a breadth of 23 ft. and a height of 32 ft. When the lava stream was examined near its source by the spectroscope, it showed the lines of hydrogen, calcium, sodium, and potassium. Silvestri has embodied his observations in a monograph; he has also just finished an important work, illustrated by photographs showing various phases of eruption of Etna. The work will no doubt be in print before the end of this year.

From Sicily, we pass at one stride to that country of which the Danes say: "God made the rest of the world; the devil made Iceland." A few months prior to the outburst of Etna, Hecla, which had not been in eruption since 1845, showed signs of disturbance, and threw up a hillock about four miles to the N.E. of the Great Crater. A large quantity of very pumiceous lava was emitted, and, at the time when the writer visited it, great quantities of hydrochloric acid were discharged from crevices in the lava, but the eruption was otherwise unimportant.

Since 1872, Vesuvius has been only active at intervals. Palmieri has published a full account of the 1872 eruption, and in annual reports has given the history of the mountain since that date. The eruption of Santorin, which began in January, 1866, and lasted till October, 1870, has furnished results of great interest to vulcanologists, and it has led to the publication of M. Fouqué's magnificent "Santorin et ses Éruptions," in which the whole history of the volcanic phenomena and the products is given. The mineralogical results have been of especial interest. Also we may note the peculiarity that, in the most violent period of activity, inflammable gaseous exhalations, which took fire on coming into contact with red-hot lava, were emitted. The flames, when examined by the spectroscope, were found to consist of hydrogen, containing small quantities of copper, sodium, and chlorine. As the gaseous emanations of Santorin are emitted under water, they are not burned at the point of issue, and can hence be examined readily. As much as 56 per cent. of hydrogen was found in some of the exhalations, and the author concludes, we think without sufficient warrant, that the enormous quantities of water-vapour emitted in volcanic eruptions exist in the red-hot magnia of lava, not as water, but as dissociated hydrogen and oxygen.

The record of recent earthquakes will be fresh in the minds of our readers. Three calamitous earthquakes have occurred within the last eight months—at Ischia, in Chios, and at Agram. In the latter 4,000 persons perished, double that number were maimed, and 30,000 were rendered homeless. It commenced on April 3, but several months afterwards a minaret and a tottering wall were overthrown, and in all there were at least 250 shocks. In 1870, a great earthquake occurred in Phocis, north of the Gulf of Corinth. Professor Julius Schmidt, the Greek Astronomer Royal, has given a detailed account of it. It is asserted that during the first three days a shock was felt every third minute, while Schmidt himself counted nearly 2,000 in the twenty-four hours, four days after the great outbreak. Altogether, during five months of 1870, it is calculated that the shocks and detonations exceeded half-a-million. In October, 1870, the most severe shock of earthquake observed during the present century in the north-eastern states of North America was felt at Quebec. It was instantly telegraphed to Montreal, and the message arrived nearly half a minute before the shock. The Peruvian earthquake of Aug. 13, 1868, was felt the same evening in Hawaii, 6,300 miles distant.

We are as far as ever from the knowledge of the cause of earthquakes, but, undoubtedly, high pressure steam has much to do with it. The author of the article, "Earthquakes," in the July *Quarterly Review*, propounds a theory to the effect that electricity is the true cause, but in what manner he has not clearly defined. We do not consider that his arguments will bear criticism. Here is one of them:—"The vicinity of hot springs, volcanoes, mud lakes, regions of intense heat, and centres of electric influence, are the special haunts of earthquakes, and science has pretty well proved that heat and electricity are convertible."* One word in conclusion as to volcanic products. But few new minerals have lately been found. Professor Scacchi announced two years ago the discovery of a new element, which he termed *vesbium*, among the lavas of Vesuvius, but we have not heard that it has ever been isolated. The most remarkable volcanic product lately discovered is undoubtedly a substance from the crater of Volcano, analysed by Professor Cossa, of Turin. It was found to contain seven non-metals and eight metals, among the latter the rare and recently-discovered elements, cæsium, rubidium, and thallium.

OUR UNBIDDEN GUESTS.

By Dr. Andrew Wilson, F.R.S.E.

PART II.

It is much the same with the tapeworm-tribe as with the fluke (considered in Part I.) The common tapeworm of man (*Tænia Solium*) consists of a very minute "head," attaching itself by suckers and hooks to man's intestines; of a slender "neck," and of hundreds of "joints." Each "joint" is really a semi-independent *animal*; and the tapeworm is therefore a *compound* animal, and presents us with a *colony* of similar beings. A large tapeworm may measure 20 or 30 ft.; and new joints are continually being "budded" out from the head and neck. Hence the physican can never be sure that he has cured a case of tapeworm until he has seen the head and neck of the animal. If a man swallowed the *egg* of a tapeworm, he would not be infested thereby. The young worm has to pass its early life in the body of another warm-blooded animal; and in the case of the common tapeworm, it is "the gintleman that pays the rint," which acts the part of nurse or first host. Man, in other words, obtains his common tapeworm guest from the pig. When this animal swallows the egg of a tapeworm, the young worm bursts through the egg-case and bores its way to the pig's *muscles*. If the porker is affected by numerous embryos, that is, if it has swallowed a large number of *eggs*, it will become feverish and ill, and it will then be said to have developed "measles." The "measles" of the pig are the visitations of young tapeworms. In the muscles of the pig, then, these young worms rest. *Je suis ici; j'y reste*, is decidedly the motto of the young worm. It developes a little head and neck, and it also, by way of a tail, produces a little bladder or bag. Before naturalists knew its true nature, it was regarded as a special kind of parasite, and was named a "cystic worm."

If the pig dies a natural death and is respectably interred, or if the pig should live long enough, these youthful tapeworms will respectively perish, or will degenerate and disappear from the tissues of the aged porker. But assuming that the usual Nemesis of the pig race overtakes the animal, then, in the form of pork, it will gladden the heart of certain members of the human race. Now, let us suppose that a man eats a portion of the "measly pork." Let us further suppose that the pork has been

* The article is utterly without scientific value. How it found its way into a magazine of good position is a mystery.—Ed.

imperfectly cooked; then comes the "tide of fortune" to the young worms. For when the young worm has been eaten by the man, the bladder-tail drops off. Each little head and neck finding itself in the human stomach, recognises its lawful *habitat*. Each attaches itself to the lining membrane of the human intestines, and each by a process of budding produces joint after joint, until man is presented with his matured "guest."

If we tabulate matters thus, the history of the tapeworm will become clear:—

1st Epoch.	Stage 1. The egg derived from parent tapeworm of man. " 2. Swallowed by the pig: developing. " 3. The "Resting Larva," or cystic worm, in the pig's muscles, and forming "measly" pork.	Passed in the pig as host.
2nd Epoch.	" 4. Swallowed by man. " 5. Development of the head and neck, and attachment to man's intestine. " 6. The production by budding of the adult worm.	Passed in man as the host.

As a last piece of parasite biography, we may glance at the history of a form which now and then attracts the notice of even Imperial Parliament itself. This form is the famous *Trichina*, which acquires an unenviable notoriety, in that it may, unlike the tapeworm race, cause the death of its "host." Each trichina is a minute worm, coiled, in its immature condition, within a little sack or bag, which in numbers may be found again in the muscles of the pig. Where the pig gets its trichinæ from is hardly certain, but rats are believed to be the sources of supply for the pig race. In the muscles of the pig each trichina is, as already remarked, in an imperfect and youthful condition. If the pig lives long enough, the trichinæ will disappear from its muscles, or will degenerate to become mere specks of lime. But should a man eat a portion of trichina-infected pork, the youthful worms will undergo, in his digestive system, a rapid and extraordinary development. The trichinæ will develope enormous numbers of young; and the young brood will now naturally seek the muscles of man for a habitation, as their parents before them sought those of the pig. Then ensues the tug of war for the afflicted human. The pain caused by the boring of these microscopic worms from stomach to muscles is intense. It is this pain and attendant symptoms that constitute the disease known as trichiniasis. If the patient's strength holds out, he is safe whenever the young trichina-brood enter the muscles. There they rest, and remain to degenerate—unless, indeed, cannibal instincts were represented in the human race; in which case the cannibal world experience a few of the tortures and troubles which are said proverbially to afflict the just and unjust alike.

The great lesson to the learned, from our survey of parasites, is care in the choice and increased care in the cooking of our food. It should be remembered that the germs of these parasites are killed by a sufficiently long exposure to heat. Hence, whilst underdone meat may have its charms, it has likewise its grave dangers. Pork, in any and every fashion, should at all times be thoroughly cooked. In this latter case, the parasitic horde may not merely be destroyed, but may even contribute in a microscopic way, to human nutrition.

Mr. S. L. M. Barlow, one of the trustees of the Metropolitan Museum of Art, New York, has presented to that institution, through its director, General di Cesnola, a collection of vases lately discovered

SOLIDS, LIQUIDS, AND GASES.

By W. Mattieu Williams.

PART V.

As already stated, in Part 3 of this series, page 88, the conversion of water into steam under ordinary atmospheric pressure demands 966·6° of heat over and above that which does the work of raising the water to 212°, or, otherwise stated, as much heat is at work in a given weight of steam at 212°, as would raise the same quantity of water to 1,178·6° if it remained liquid.

James Watt concluded from his experiments that a given weight of steam, whatever may be its density, or, in other words, under whatever pressure it may exist, contains the same quantity of heat. According to this, if we reduced the pressure sufficiently to bring down the boiling point to 112°, instead of 212°, the latent heat of the steam thus formed would be 1,066·6° instead of 966·6°, or if, on the other hand, we placed it under sufficient pressure to raise the boiling point to 312°, the latent heat of the steam would be reduced to 866·6°, *i.e.*, only 866·6° more would be required to convert the water into steam. If the boiling point were 412°, as it is between 19 and 20 atmospheres of pressure, only 766·6 more heat would be required, and so on, till we reach a pressure which raises the boiling point to 1,178·6°, when the water would become steam without further heating, *i.e.*, the critical point would be reached, and thus, if Watt is right, we can easily determine, theoretically, the critical temperature of water.*

Mr. Perkins, who made some remarkable experiments upon very high pressure steam many years ago, and exhibited a steam gun at the Adelaide Gallery, stated that red-hot water does not boil; that if the generator be sufficiently strong to stand a pressure of 60,000 lb. load on the safety valve, the water may be made to exert a pressure of 56,000 lb. on the square inch at a cherry red-heat without boiling. He made a number of rather dangerous experiments in thus raising water to a red-heat, and his assertion that red-hot water does not boil is curious when viewed in connection with Dr. Andrew's experiments.

I cannot tell how he arrived at this conclusion, having been unable to obtain the original record of his experiments, and only quote the above second-hand. It is worthy of remark that the temperature he names is about 1,170°, or that which, if Watt is right, must be the critical temperature of the water. Perkins' red-hot water would not boil, as he states, being then in the intermediate condition.

So far, we have a nice little theory, which not only shows how the critical state of water must be reached, but also its precise temperature; but all this is based on the assumption that Watt made no mistake. Unfortunately for the simplicity of this theory, Regnault states that *his* experiments contradict those of Watt, and prove that the latent heat of steam does not diminish just in the same degree as the boiling-point is raised, but that instead of this the diminution of the latent heat progresses 30½ per cent. more slowly than the rise of temperature, so that, instead of the latent heat of steam between boiling-points of 212° and 312° falling from 966·6° to 866·6° it would only fall to 895·1° or 69·5° for every 100°.

If this is correct, the temperature at which the latent heat of steam is reduced to zero is much higher than 1,178·6°, and is, in fact, a continually receding quantity never absolutely reached; but I am not prepared to accept these figures of Regnault as implicitly as is now done in text

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* Watt's own figure for the latent heat of steam at 212° was 950°,

books (I was nearly saying "as is now the fashion"), seeing that they are not the actual figures obtained by his experiments, but those of his "empirical formulæ" based upon them. His actual experimental figures are very irregular; thus, between steam temperature of 171·6° and 183·2° a difference of 11·6°, the experimental difference in the latent heat came out as 4·7°; between steam temperature of 183·2° and 194·8°, or 11·6°, again the latent heat difference is tabulated as 8·0°.

Regnault's experiments were not carried to very high temperatures and pressures, and indicate that as these advance the deviation from Watt's law diminishes, and may finally vanish at about 1,500° or 1,600°, where the latent heat would reach zero, and there, according to the above, the critical temperature would be reached. Any additional heat applied after this will have but one function to perform, viz., the ordinary work of increasing the bulk of the heated body without doing anything further in the way of conferring upon it any new self-repulsive properties.

Our notions of solids, liquids, and gases are derived from our experiences of the state of matter here upon this earth. Could we be removed to another planet, they would be curiously changed. On Mercury water might rank as one of the condensible gases; on Mars, as a fusible solid; but what on Jupiter?

Recent observations justify us in regarding this as a miniature sun, with an external envelope of cloudy matter, apparently of partially condensed water, but red-hot, or probably still hotter within. His vaporous atmosphere is evidently of enormous depth, and the force of gravitation being on his visible outer surface 2½ times greater than that on our earth's surface, the atmospheric pressure in descending below this visible surface must soon reach that at which the vapour of water would be brought to its critical condition. Therefore we may infer that the oceans of Jupiter are neither of frozen, liquid, nor gaseous water, but are oceans or atmospheres of critical water. If any fish-birds swim or fly therein they must be very critically organised.

As the whole mass of Jupiter is 300 times greater than that of the earth, and its compressing energy towards the centre proportional to this, its materials, if similar to those of the earth and no hotter, would be considerably more dense, and the whole planet would have a higher specific gravity, but we know by the movement of its satellites that, instead of this, its specific gravity is less than a fourth of that of the earth. This justifies the conclusion that it is intensely hot, for even hydrogen, if cold, would become denser than Jupiter under such pressure.

As all elementary substances may exist as solids, liquids, or gases, or, critically, according to the conditions of temperature and pressure, I am justified in hypothetically concluding that Jupiter is neither a solid, a liquid, nor a gaseous planet, but a critical planet, or an orb composed internally of dissociated elements in the critical state, and surrounded by a dense atmosphere of their vapours, and those of some of their compounds, such as water. The same reasoning applies to Saturn and the other large and rarefied planets.

The critical temperature of the dissociated elements of the sun is probably reached at the base of the photosphere, or that region revealed to us by the sun-spots. When I wrote "The Fuel of the Sun," thirteen or fourteen years ago, I suggested, on the above grounds, the then heretical idea of the red-heat of Jupiter, Saturn, Uranus, and Neptune, and showed that all such compounds as water must be dissociated at the base of the sun's atmosphere, but being then unacquainted with the existence of this critical state of matter, I supposed the dissociated elements to exist as gases with a small solid nucleus or kernel in the centre.

Applying now the researches of Dr. Andrews to the conditions of solar existence, as I formerly applied the dissociation researches of Deville, I conclude that the sun has no nucleus, either solid, liquid, or gaseous, but is composed of dissociated matter in the critical state, surrounded, first, by a flaming envelope due to the recombination of the dissociated matter, and outside of this another envelope of vapours due to this combination.

THE EVOLUTION OF MAN.

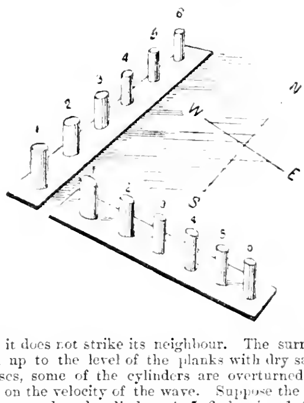
By Dr. A. Wilson, F.R.S.E.

In letter 103, "G. M." asks, "has evolution effected any change in man? If so, what change? If not, why not?" These are weighty questions, and "G. M." will find difficulty in answering them. Let him remember firstly that evolution has only been studied—rather nature, in the light of evolution, has only been studied—for some twenty-five years or so. That is, of course, a mere fractional space in the history of human thought. If we refuse to admit (as science does) that man was created a perfect being, and then became degraded, there exists only another supposition—that of evolution. If man has arisen from a savage to a civilised state, that surely is evolution. We do not yet know, because such knowledge is difficult to acquire, if the human frame is subject to the same influences as those of lower animals. But there is little doubt that elevation from savagery to civilised life means and implies "evolution," and that of considerable extent. Mentally, man's evolution cannot be doubted; the ever-widening sphere of thought has sprung from small and rude beginnings, like language itself. But man's ways of life, his power of adaptation to his surroundings, and countless other circumstances, have made the facts and course of his "evolution" very difficult to trace. If "G. M." will read Tylor's "Anthropology" (Macmillan) he will find there summarised facts and phases of human life which go powerfully to answer the interesting queries he puts to Knowledge.

MALLET'S SEISMOMETERS.

The "Encyclopædia Britannica," 9th edition, "Earthquake," mentions and describes a seismometer of extreme simplicity:—

"Its construction, which is due to Mr. Mallet, will be understood by reference to the figure. Two sets of right cylinders are turned in some hard material, such as boxwood. The cylinders are all of the same height, but vary in diameter. Two planks of wood are fixed to a level floor, one having its length in a north and south, and the other in an east and west direction. The cylinders stand upright on the planks in the order of their size, with a space between each pair greater than their height, so that when one



pillar falls it does not strike its neighbour. The surrounding floor is covered up to the level of the planks with dry sand. When a shock passes, some of the cylinders are overturned, the number depending on the velocity of the wave. Suppose the shock knocks over the narrow-based cylinders 4, 5, 6, leaving 1, 2, 3 standing,

then the intensity of the horizontal component must have been greater than that needed to overturn No. 4, but not great enough to overturn No. 3."

Then follows a formula for determining approximately the velocity—which, perhaps, it may not be necessary to trouble you with.

A. T. C.

THE FAURE ACCUMULATOR.

By W. Lynd.

THANKS to Professor Sylvanus Thompson, who has just sent me the results of his latest experiments with secondary batteries, I am able to give a brief sketch of the Faure accumulator.

So far back as 1860 M. Planté constructed a secondary battery, consisting of nine cells, in each of which two long and wide strips of lead, separated by coarse cloth, were rolled together in a spiral form and immersed in dilute sulphuric acid. A few months later he modified this form by placing side by side in a rectangular box two series of lead plates, alternately connected together, each plate being about eight inches high. He recurred afterwards to the spiral form as being more convenient, but replacing the coarse cloth by narrow strips of gutta-percha. But the cells thus constructed were not ready for immediate action. Two clean lead plates give no current of their own; they are only intended to receive and store up what is sent into them from some external source; and at first, while the lead is bright, when a current is sent through the cell from some suitable source, such as three or four Grove or Bunsen cells, the separated oxygen and hydrogen gases bubble up to the surface, for the most part leaving only a very small percentage as an adherent film, and, in consequence, yielding only very transient secondary currents. The plate of lead by which the current enters is, however, attacked by the oxygen, and becomes covered by a thin layer of brown peroxide of lead, and this film, though thin, is powerfully electro-negative towards metallic lead and towards the film of hydrogen on the Kathode plate. The cell in this condition will therefore produce a current, and in so doing, the peroxide is partially reduced to the metallic condition, and assumes in its reduction a spongy or loosely crystalline texture. If now the cell be again charged, and charged in the opposite direction, the other plate of lead becomes in like manner peroxidised, while the hydrogen bubbles are less freely evolved, for the atoms of gas unite as fast as they are liberated with the oxygen of the peroxide and reduce it to the metallic condition; every time the charging current is thus reversed, the films of peroxide, as of spongy metal, become thicker, until the lead to a considerable depth is bitten into. And every such operation increases, therefore, the power of the cell to store up in this electro-chemical fashion the energy of the currents sent into it. M. Planté ascribes the process of "forming" to a sort of electro-chemical *tanning*. The first day the alternate charging should be done at intervals of a quarter to half an hour, the cell being discharged between each operation. The next day the duration of the alternate charges may be increased from a quarter of an hour to a whole hour; the day after to two hours. After repose for a week or a fortnight, the charges may last several hours; and by the end of several months, the cell will be well "formed;" after which, it should, when used, be charged in one direction only, otherwise the whole thickness of the lead plates will be bitten into, and transformed into peroxide. These magnificent researches were carried on by Planté through more than twenty years, and it seems remarkable that even in the scientific world, that gentleman's claim to the discovery of the accumulator are not duly acknowledged.

M. Camille Faure, who has been awarded by the public press the lion's share of the glory, conceived the idea of constructing a secondary battery, in which, though the tedious process of "formation" is modified and shortened, the ultimate result is the same; namely, to produce upon lead plates, immersed in dilute sulphuric acid, a coating of peroxide of lead that can readily be reduced to the loosely crystalline metallic condition.

The Faure accumulator, of which we have heard so much lately, is simply a modification of the Planté secondary battery, and is constructed as follows:—Eleven sheets of lead, of such thickness as to weigh about 2 lb. to the square foot, are cut to the size of 12 in. by 10 in., an ear-piece being burnt on at one corner. Or six sheets are taken, five of them being twice the above size, and folded double. These are painted thickly with red lead on both sides, and against each side is pressed a piece of felt, the face of which is also thickly coated with red lead, there being about 17 lb. of lead and 23 lb. of red lead altogether. These sheets are placed side by side in a water-tight case, alternate sheets being connected together by the projecting flaps. The cell is filled up with dilute acid, the total weight being about 50 lb.

When thus prepared, the cells are formed by a process of charging by means of the current of a dynamo-electric machine, the current being sent through them for six or seven days without intermission before they are ready for use. The red lead is reduced gradually on one side to the metallic state, and on the other assumes the condition of peroxide; but the cell does not attain its fast condition for some weeks. Such is a brief résumé of the wonderful accumulators which are destined to work a revolution in electrical science. Those who desire to gain a thorough knowledge of the theory of the secondary batteries cannot do better than purchase a copy of Professor Thompson's work on electricity and magnetism. It is published by Macmillan & Co.; the price is only 1s. 6d.

THE WYANDOTTE INDIANS.

By Miss A. W. Buckland.

AS bearing upon the subject discussed in KNOWLEDGE, Are women inferior to men? the account given by Mr. John W. Powell, vice-president of the American Association for the Advancement of Science, of the form of Government among the Wyandotte Indians, will probably be found interesting.

In the Wyandotte Government, says Mr. Powell, four groups are recognised—the family, the gens, the phratry, and the tribe.

The family is nearly synonymous with household. The head of the family is a woman.

The gens is an organised body of consanguineal kindred in the female line. "The woman carries the gens," is the formulated statement by which a Wyandotte expresses the idea that descent is in the female line. Each gens has the name of some animal—the ancient of such animal being the tutelar god.

There are four phratries in the tribe, and this division seems to be used chiefly for religious purposes, in the preparation of medicines, and in festivals and games. The eleven gentes, as four phratries, constitute the tribe. Each gens is a body of consanguineal kindred in the female line, and each gens is allied to other gentes by consanguineous kinship through the male line, and by affinity through marriage. The family or household is not a unit of the gens or phratry, as two gentes are represented in each—the father must belong to one gens, and the mother and her children to another. The civil government belongs of right to a system of councils and chiefs. In each gens there is a council composed of four women. These four women councillors select a chief of the gens from their brothers and sons, and this chief is the head of the gentile council. The tribal council is composed therefore of one-fifth men and four-fifths women.

The four women-councillors of the gens are chosen by the heads of households, themselves being women. There is no formal election, but by frequent discussion it is decided that, in the event of the death of any councillor, a certain person will take her place. When a woman is installed as Councillor, a feast is given by the gens to which she belongs, to which all the members of the tribe are invited. The woman is painted and dressed in her best attire, and the sachem of the tribe, who is chosen by the chiefs of the gentes, places upon her head the gentile chaplet of feathers, and announces in a formal manner to the multitude, that the woman has been chosen a councillor.

The gentile chief is chosen by the council women, after consultation with the other women and men of the gens. At his installation, the council women invest him with an elaborately ornamented tunic, place upon his head a chaplet of feathers, and paint the gentile totem on his face.

Meetings of the gentile council are very informal, but the meetings of the tribal councils are conducted with due ceremony. The chief of the wolf gens, who is of right the herald and sheriff of the tribe, calls the assembly to order, fills and lights a pipe, sends one puff of smoke to heaven and another to the earth. The pipe is then handed to the sachem, who, filling his mouth with smoke, and turning from left to right with the sun, slowly puffs it over the heads of the councillors, who are sitting in a circle. The pipe is then smoked by each person in turn. The sachem then explains the object of the meeting, and each person tells what he thinks should be done. If the majority agree, the sachem simply announces the decision; but if there is a tie, the sachem is expected to speak.

It is the function of government to preserve rights and enforce the performance of duties.

These rights are:—1, Rights of marriage; 2, Rights to names; 3, Rights to personal adornment; 4, Rights of order in encampments and migrations; 5, Rights of property; 6, Rights of person; 7, Rights of community; 8, Rights of religion.

Marriage between members of the same gens is forbidden. Polygamy is permitted, but the first wife remains the head of the household. A man seeking a wife consults with her mother, and the mother of the girl tries to obtain the consent of the women council-

dies, the children belong to her sister or nearest female kin, the matter being settled by the council women of the gens. Once a year the council women of the gens select the names for the children born during the year, and the chief of the gens proclaims them at the festival. No person may change his name, but by honourable conduct he may win another.

Within the tribal area, each gens occupies a tract for cultivation. The women councillors partition the gentile land among the householders. The ground is repartitioned once in two years. Cultivation is communal, that is, all of the able-bodied women of the gens take part in the cultivation of each household tract.

The wigwam, or lodge, and all the articles of the household belong to the woman—the head of the household; and at her death are inherited by her eldest daughter or nearest of female kin. The matter is settled by the council women. If the husband die, his property is inherited by his brother, or his sister's son, except such portion as may be buried with him. His property consists of his clothing, hunting and fishing implements, and such articles as are used personally by himself. Usually a small canoe is the individual property of the man. Large canoes are made by the male members of the gentes, and are the property of the gentes.

Each gens has a right to the services of all its women in the cultivation of the soil. Each gens has the right to the service of all its male members in avenging wrongs, and the tribe has the right to the service of all its male members in time of war.

This is a short abstract of Mr. Powell's paper, as far as it relates to the status of woman among the Wyandottes, and I feel sure it will be a surprise to many to find that the despised Indian squaw holds a position so honourable; one to which her civilised white sisters may never hope to attain; but the truth is, we find here, among the Wyandotte, the survival of a social state once very widely spread, and which probably existed wherever we find kinship traced on the female side—that is, through the mother instead of the father. It was, perhaps, the earliest of all forms of government, especially among people of Mongoloid affinities. Among the Semites, the government was patriarchal and paternal; but even among the Hebrews, we find prophetesses, possessed of no small amount of authority. It remained for the Aryan races to deprive woman of every shred of real power, whilst professing to treat her with chivalrous deference, and it is to this probably that we may attribute the diminution in the size of the heads of women in modern times, as compared with those of men—a difference which is not to be found in the more ancient skulls, as the late Professor Rolleston and other able anatomists have so often pointed out; and which it might be safely affirmed would not be found among the Wyandotte councillors; for there can be little doubt that the brain develops by use, and that in a tribe or nation wherein the burden of government is divided between the sexes, there will be no disparity in the brain power of men and women, nor in the skulls wherein those brains are contained.

NOTE ON THE SPHEROIDAL STATE.

By W. F. Barrett, F.R.S.E.

(Professor of Experimental Physics in the Royal College of Science, Dublin.)

In a paper read before the Royal Dublin Society, Mr. G. Johnstone Stoney has given a new and beautiful explanation of the so-called spheroidal state of liquids, wherein he showed that the force detected by Mr. Crookes, and which is the cause of the motion of radiometers, was also competent to explain the phenomena of the spheroidal state. A liquid drop is said to be in the spherodial state when falling upon a hot body it does not come into contact with the surface but rolls over it as a flattened spheroid. A mobile elastic spring evidently buoys up the drop until such times as the hot body cools, when, with a sudden rise of temperature and generation of steam, the drop comes into contact with the surface below it, spreads out into a film, and rapidly disappears into vapour.

Hitherto this phenomenon has been regarded as due to the fact that the proximity of the hot surface converts a portion of the liquid into vapour, the elastic force of which sustains the drop. There are, however, several phenomena, applied to the spheroidal condition, to which this generally received explanation gives no solution,—such, for example, as the mobility of light powders in a hot crucible, or the formation of globules on the surface of water and other liquids. Mr. Stoney's explanation, on the other hand, embraces the whole of these outstanding and hitherto enigmatical phenomena. Briefly stated, this theory is based on the fact that whenever two bodies at *different temperatures* are brought sufficiently near each other, a modification takes place in the molecular structure of the layer of gas or vapour between them, given rise to the so-called "Crookes' force," wherein there is an excess of pressure in the direction joining the hot and cold surfaces over the pressure in transverse directions. Now this excess of pressure depends partly on the quantity of heat making its way across the intervening layer of gas or vapour, and partly on the proximity of the two surfaces,—a proximity not to be estimated absolutely, but with reference to the length to which a molecule of the gas will travel in the intervals between its encounters with other molecules. Hence there are obviously three modes whereby the excess of pressure, this Crookes' force, may be developed or augmented:—

1st. By lengthening the paths of the molecules between the warm and cool surfaces, accomplished by attenuating the gas.

2nd. By bringing the hot and cold surfaces very near together.

3rd. By increasing the difference of temperature between the two surfaces.

Now if the support of the spheroidal drop be due to this Crookes' force a difference of temperature must exist between the drop and the surface over which it stands, and the greater this difference of temperature the larger the drop that ought to be supported, and the more persistent the phenomenon. Mr. Moss has shown (Proc. R. D. S., Dec., 1877) that by securing a continual difference of temperature a globule of ether may be supported on the surface of its own liquid for upwards of an hour, until in fact some accidental derangement occurs. The conditions of the two theories being thus defined, it is easy to see that several crucial experiments might be devised which should help to decide the question at issue.

The following experiment the author has made with this object in view. Upon the surface of the ordinary petroleum of commerce, liquid globules of transient duration can readily be formed, simply by removing a small quantity of the liquid in a pipette and carefully depositing a drop on the surface of the liquid. These drops are clearly in the spheroidal condition, and they are easily and abundantly formed by dipping a vibrating tuning-fork into the liquid, or by drawing a fiddle bow over the edge of the vessel containing the liquid. According to the ordinary explanation the drops are supported by the elastic force of the vapour of the liquid, which would, of course, be greater the higher the temperature of both liquid and drops. According to Mr. Stoney's theory the drops are supported by the Crookes' force, generated by the proximity of the drop and liquid, and by the fact that they are at different temperatures. Evaporation rapidly cools the drops jerked up from the liquid, and thus a slight difference of tempera- instantly comes into play. If, however, Mr. Stoney's theory be true, then a drop of cool petroleum would be more easily and longer sustained on a surface of warm petroleum, or *vice versâ*, than a drop taken from the mass of liquid below it, where only a slight temperature difference is created.

Two beakers were filled with petroleum from a common source, one (A) at the temperature of the air, the other (B) at a temperature of 100° F. With a pipette some liquid was taken up from A and a drop carefully deposited on its own surface, a globule was formed, floated for a fraction of a second, and then disappeared. The same occurred with a drop from B placed upon B. A drop of B was now removed and deposited on A, a large globule was easily formed on the surface, floated about from 10 to 20 seconds, and then disappeared. A drop of A was now placed on B, the same thing occurred, but the duration of the drop was not quite so great, owing to the greater density of the cool drop tending to sink it below the surface of the warm liquid, thus rapturing the Crooke's layer and destroying the difference of temperature.

There is no doubt or uncertainty whatever about this experiment, and it shows that, if the ordinary explanation be correct, the second case, where B rests on B, should give the best result, whereas the reverse is the case. Further, the experiment wherein the best result is obtained, is such as best fulfils the condition of Mr. Stoney's theory.

The limit of formation of these spheroids, when the liquid is uniformly dropped through a gradually-increasing height, may be employed to test the relative degrees of force which sustain the globule, and careful experiments made by the author in this direction still further corroborated the truth of Mr. Stoney's views.

The Light of the Stars.—For a number of years the special work carried on at the Harvard Observatory, under the direction of Professor Pickering, has been the measurement of the intensity of the light of the heavenly bodies. Some of the results presented at a recent meeting of the Society of Arts, at the Institute of Technology, Boston, indicate measurements almost incredibly fine. The light which falls upon the earth from the satellites of Mars, for example, is about equivalent to what a man's hand on which the sun shone at Washington would reflect to Boston. The labour of measuring the brightness of all the visible stars was begun two years ago. It has since gone on at the rate of about 40,000 a-year, and will be completed next fall.—*Scientific American.*

AN INSTRUMENT FOR DRAWING THE CONIC SECTIONS.

By Thomas Thorp.

THE instrument shown in the accompanying sketch, Fig. 1, may be used for tracing figures of the conic sections. It is quite original, I believe, and can be made without any great difficulty.

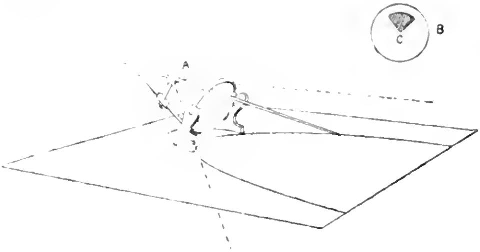


Fig. 1.

The rod through the ball and socket joint at A should be as enlarged at B, the edge in line with the scribing-point being exactly central, as shown at C.

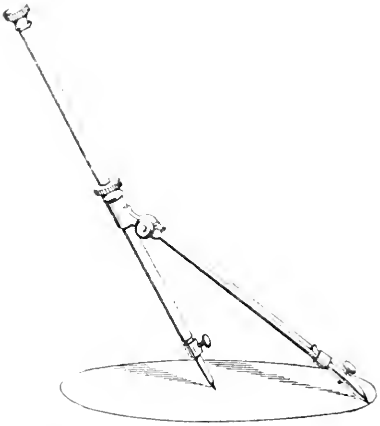


Fig. 2.

The instrument is sketched as describing a parabola, but is equally suited for any other of the conic sections. Further details if required.

For describing ellipses, the instrument shown, Fig. 2, is extremely useful, and may not be known to many of your readers.

Our Next Number. We have been obliged to defer to next week two illustrated articles, one by a "Fellow of the Royal Astronomical Society," on Work with a Small Telescope; the other by the Editor, on the Great Pyramid. In compliance with the wishes of many readers, the star maps, which have hitherto appeared weekly, will be combined into a single map (on the same scale), which will appear in the first week of each month. In the other weeks some subsidiary maps will be given, illustrating the motions of the heavenly bodies during the month. There will also be given each month a paper dealing with star names, constellations, figures, &c. — Ed.

Errata.—The comet referred to in Mr. Denning's letter of last week (letter 122, p. 140), was not the one which bears his name (comet *f*, 1881), but comet *g*, 1881. The letter *g* was misread. In Reply to 55, p. 136, last line but one, [illegible]

Letters to the Editor.

[The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with their writers. All communications should be as short as possible, consistently with full and clear statements of the writer's meaning.]

All Editorial communications should be addressed to the Editor of Knowledge; *all Business communications to the Publishers, at the Office, 74, Great Queen-street, W.C.*

All Remittances, Cheques, and Post-Office Orders should be made payable to Messrs. Wyman & Sons.

**** All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.*

All Letters or Queries to the Editor which require attention in the current issue of Knowledge, *should reach the Publishing Office not later than the Saturday preceding the day of publication.*

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

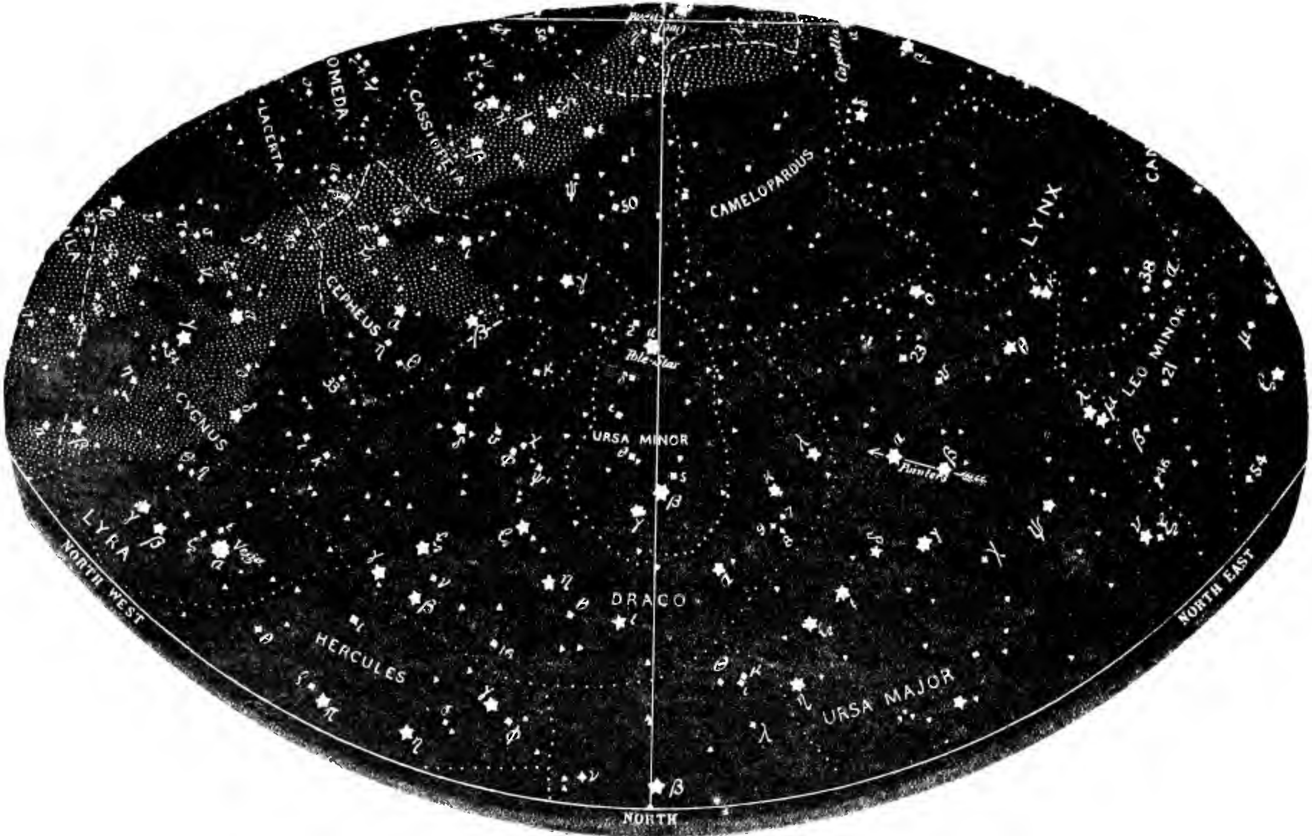
Our Correspondence Columns.

PLANS FOR THE NEW YEAR.—DARWIN AND DESIRE.—SPIRITUALISM.—PLANETS HIDING STARS.—VIBRATIONS OF LIGHT IN ÆTHER.—VECTORS VESTIGES OF CREATION.—NEWSPAPER SCIENCE.—THE GREAT PYRAMID.

[136]—With the first number of the year 1882 we shall somewhat modify our plan, experience and the suggestions of many correspondents having shown that some features which we have not yet introduced would be desirable, and that others which we had thought desirable are either not so, or inconsistent with some presenting greater advantages. The task of selection is not altogether easy. We have been reminded much of the old man with his donkey and two sons in the fable, whose fate, it will be remembered, when he tried to please every one, was to be thrown into the river, along with his three companions. We are urged—(1) to widen our space for correspondence; (2) to keep out correspondence; (3) to encourage paradoxers to disport themselves in our pages; (4) to silence them utterly; (5) to explain simply the principles of every science on the face of the earth, all at the same time; (6) to leave all such explanations to text books; (7) to increase our size; (8) by no means to do so; (9) to publish all the four maps for the month at the beginning of each month; (10) to publish them weekly, as we do, but with a page or two of explanation, &c.; (11) to have the stars punched out of the paper; (12) to have the names of all the stars; (13) to have as little writing on the maps as possible; (14) to leave out the small stars; (15) to letter them as well as the others; (16) to give also weekly zodiacal maps, with paths of planets, moon, comets, &c.; (17) to announce all astronomical phenomena; (18) to remember that the almanacs do that for us; (19) to have more astronomy; (20) to have less astronomy; (21) to be rather less simple; (22) to be more simple; (23) to give reports of societies; (24) to give digests of reports; (25) to give no reports at all; (26) to announce meetings of societies; (27) to devote space to no such purpose; (28) to limit the mathematical column to the discussion of really difficult problems; (29) to use it to explain matters for beginners; (30) to omit it altogether; (31) to extend the whist and chess columns; (32) to shorten them; (33) to leave them out; (34) to have exchange columns, sixpenny sale columns, &c.; (35) to have none of these; (36) to offer prizes, have essay columns for girls and boys, &c.; (37) to eschew such allurements; (38) to have longer articles; (39) to let no article exceed a page, and few be longer than a column; (40) to have articles on ancient philosophy, modern scientific biographies, art and the drama, puzzles, enigmas, hints for collectors of shells, medals, seaweed, &c., &c., &c., *ad infinitum*. To all which, all that we can reply is that we will do our best to please the greatest number.

Mr. Newton Crosland, accusing me of verbal shuffling, claims from me an apology. Darwin, he says, does speak of the "endeavour," though not of the "desire," of the rattlesnake to frighten away its enemies; and the two mean the same. Truly they differ little; but what Mr. Crosland said was that "Darwin attributed the

THE NORTHERN SKIES IN DECEMBER.



This Map shows the position of the stars in the Northern Skies:—

n November 30, at 10½ o'clock.	On December 8, at 10 o'clock.	On December 16, at 9½ o'clock.	On December 23, at 9 o'clock.	On December 30, at 8½ o'clock.
n December 4, at 10¼ o'clock.	On December 12, at 9¾ o'clock.	On December 19, at 9¼ o'clock.	On December 27, at 8¾ o'clock.	On January 3, at 8¼ o'clock.

The stars in the northern skies are carried from left to right under the pole, and from right to left above the pole, descending to the left or west of the pole, and ascending to the right or ast of the pole. They are carried, in fact, round the pole in a direction contrary to that in which the hands of a watch move.

*** Stars of the first magnitude are shown with eight points, those of the second with six, of the third with five, of the fourth with four, of the fifth with three.

Continued from page 160.]

evolution of the rattlesnake to the desire—or endeavour—of the animal to frighten its enemies," and it is just here that he misunderstands Darwin. The theory he attacks is Lamarck's, not Darwin's. Darwin and Lamarck may agree in saying that the rattlesnake uses his rattle with the desire (or in the endeavour) to frighten its enemies; but whereas Lamarck attributed to such desire or endeavour, and the resulting "habit," the development of the rattlesnake, Darwin would do nothing of the sort (rather attributing the habit to the development). If Mr. Crosland will read Darwin's remarks on the views of Lamarck (preface to "Origin of Species," and elsewhere), and remember that Lyell became a convert to the general theory of evolution only when the Darwinian theory replaced Lamarck's, he will see that there is an important difference between the two. Mr. Crosland's diatribe on Dr. Darwin would only be permissible if he had been violently and personally attacked by that eminent geologist, which, I take it, has not happened. May I venture to remark that if Mr. Crosland thought me really guilty of shuffling, he would not "expect to receive an apology from me." He has quite wrongly accused me of shuffling, but I do not want an apology, and certainly I do not expect one.

"Tentative" thinks we do wrong in excluding spiritualism. He thinks it would be "a great gain if we would encourage a searching inquiry into the causes, electric or other, of the singular mind problems continually presented to us." We will do so, by publishing the first scientific results of such inquiries which may reach us. A story he relates seems to illustrate only the effect of an excited imagination.

Ebenezer Kelby writes respecting my remarks about the Great Pyramid, that if certain singular coincidences were noted, and those who had noted them were told it was mere accident, they would say, with a sceptical sneer, "it is a very strange accident," and many readers of KNOWLEDGE say the same of my "opinion with regard to Mr. Baxendell's wonderful calculations showing the wonderful correspondence of pyramid measures with astronomical data." If Ebenezer Kelby will wait awhile, he may find I can give reasons for my remark that the very closeness of some of the coincidences noted by Mr. Baxendell, and of some other coincidences which he has not mentioned, disproves the argument from coincidence.

"O. R. B." asks why occultations of stars by planets, which must be continually occurring, are not alluded to or tabulated? They occur far less often than he imagines. Very few are recorded, and still fewer have been properly observed. I know, indeed, of only one really satisfactory case, viz., the occultation of a sixth magnitude star in Aquarius by Jupiter, well observed by Ellery and Turner with the great Melbourne reflector. The star disappeared gradually, and was visible (just before it disappeared) at a depth of more than 500 miles below the apparent surface of Jupiter—doubtless, therefore, through a great range of the planet's cloud-laden atmosphere. "O. R. B." is mistaken in supposing the size of a star could be determined by noting how long it was in disappearing behind the comparatively slow-moving planet. If a planet's outline were sharply defined, the disappearance even of the largest and nearest star would be to all intents and purposes instantaneous.

"Practical" sends an interesting letter on the use of flesh meat, which shall appear as soon as we can find space for it.

"G. P." asks how it is that if the elasticity of the æther is almost infinite, vibrations set up in it by solar influence have such short duration? Why should darkness so soon follow sunset? The vibrations may be compared to those on the surface of water. When a stone has been thrown into water, the vibrations travel all around, ceasing first at the place where they began, and thence all round as the wave circle expands. The elasticity of the water shows itself in the wide expansion of the wave circle, not in the continuance of the oscillation at any point or points traversed by the wave. So with the æther of space, its elasticity is shown by the great distances to which light travels without appreciable extinction. The æther cannot at the same time carry on the light impulse from each point passed, and continue its vibrations there. Another question by "G. P." has not been answered, that respecting the use of vectors in Maxwell's little book on Matter and Motion. I venture to express the opinion that in a work of that kind, the use of vectors is as entirely out of place as the use of Latin and Greek technical terms would be in a treatise on domestic medicine. To the mathematician, vectors are of use to shorten reasoning and simplify statements, precisely as technical terms are of use in science. But they should have no place in elementary treatises.

We have received many letters besides those published, relating to the "Vestiges of Creation." Pressure on our space, and the comparatively small importance of the subject, prevents our publishing these. It is well known that Robert Chambers was the author of the work, but that he had excellent reasons when he wrote it, and during many years after, for not desiring to acknowledge that it was his. Lyell, in his Antiquity of Man, says of the work, that "written in a clear and attractive style, it made the English public familiar with the leading views of Lamarck in transmutation or progression, but brought no new facts or original line of argument to support those views, or to combat the principal objections which the scientific world entertained against them." Darwin says, "from its powerful and brilliant style, the work, though displaying in its earlier editions little accurate knowledge, and a great want of scientific caution, immediately had a very wide circulation; in my opinion it has done excellent service in this country in calling attention to the subject, in removing prejudice, and in thus preparing the ground for the reception of analogous views." Lieut.-Col. Ross writes us that he has overwhelming evidence to show that Sir C. Lyell *must* have written it; but Lyell opposed the Lamarckian hypothesis; also that Chambers could not have written it, but in the later years of his life Chambers acknowledged to many that he had done so. A.T.C., Eclecticus, and others, have written very fully on this subject. "Eclecticus" regards Darwin's work as a fit and complimentary (query complementary) sequel to the "Vestiges," which reads to me very much as though one should say that Newton's "Principia" was a fit sequel to Kepler's "Prodromus." The question of scientific value, be it understood, is not as between the work of Darwin and Chambers, but as between the theory of Lamarck and that of Natural Selection.

It is pointed out by a writer, who desires that his name may not be published, that a newspaper paragraph recently commented upon in our columns somewhat unfavourably, correctly represented the statements of a French chemist in the *Comptes Rendus*, and that newspaper science does not deserve all the vilification it receives. As I have myself written a good deal about science in the newspapers, I need hardly say that I am not disposed to regard *all* newspaper science as unsound. Still, it remains true that newspaper science cannot be regarded as trustworthy, simply because the general reader cannot distinguish the sound from the unsound, and has no means of ascertaining to whom particular statements are due, while it is well known that some editors of leading daily papers have themselves no knowledge whatever of science, and would as soon insert a column of utter nonsense, if foisted on them as the work of a known science student, as the most carefully-reasoned article by a Darwin, a Tyndall, an Airy, or a Huxley. From my own experience I know that many newspaper editors have no idea whatever of the progressive nature of science. Articles which I have written, the appearance of which has been delayed for one reason or another, would have been inserted just as they were written, had I not insisted on the proofs being sent to me for changes rendered necessary by the lapse of time. In one case, an article which I wrote for the *Times* immediately after the eclipse of June, 1878, was recast by me in this way several times, at great cost of labour, until at last, when I left England for America in the autumn of 1879, I gave up further attempts to make the article fit for reading at the time of publication. I had supposed my labour lost, but, to my horror, I saw that very paper quoted in an American newspaper in December, 1879, and thus first learned that at last it had appeared in the *Times*. Other papers were delayed with the intention of eventually using them, until, at last, I gave up all idea of their being patched into fitness for later dates. Some articles I have seen in the daily papers suggest to me, by their time-worn aspect and other evidence of decrepitude, that all writers are not honest enough to remind the editor of the deterioration a scientific article is undergoing as time passes (or has already undergone), preferring that an article should appear long after it has ceased to have any value than that they should lose the money which is only paid after an article has appeared. In America, the editors of leading papers adopt a plan which is at once more sensible and—in my judgment—more honest, making the question of payment (so my esteemed friend, the editor of the *New York Tribune*, tells me) independent of the use of an article which has been accepted and sent in type to an author for correction. This surely is better than the system of some of our English leading papers (the *Times* amongst others), by which an unskilful editor, after inviting and accepting more contributions than he can find room for, calmly suffers the loss to fall on the authors, and selectively on those who may be honest enough to tell him that their papers no longer have the value they originally possessed. It is because all writers are not careful to do this that we so often see old truths presented as novelties, and exploded errors presented as accepted truths, in our daily papers.

Mr. Baxendell has written a letter, in reply to Mr. Ranyard's remarks, letter 85, p. 113. We would insert the letter, though it does little more than express Mr. Baxendell's unchanged confidence in his views, were it not for passages showing that Mr. Baxendell has entirely misapprehended Mr. Ranyard's remarks respecting fortune-telling, weather prediction, alchemical promises, and so

forth. Mr. Ranyard, we are sure, had no thought of citing Mr. Baxendell as an example of the objectionable class of science workers (or rather of persons who want to work science) to whom he referred. Mr. Ranyard's letter has been regarded by quite a large number of correspondents as unduly considerate towards paradoxers, while others consider it as very gentle towards them, but not unwarrantably so. RICHARD A. PROCTOR.

SIR RICHARD PHILLIPS.

[137]—"Delta" offers to send weekly paragraphs, giving startling and important theories propounded by Sir Richard Phillips, which he believes would be read and studied with profound interest by readers of KNOWLEDGE. After reading the specimens "Delta" forwards, I decide that only one paragraph relating to Sir R. Phillips shall appear in these columns so long as they are under my charge. It is not one of his own writing, but by the late Professor De Morgan, and runs thus:—

"Sir Richard Phillips had four valuable qualities—honesty, zeal, ability, and courage. He applied them all to teaching matters about which he knew nothing, and gained himself an uncomfortable life and a ridiculous memory." RICHARD A. PROCTOR.

RETROGRADATION OF A PLANET.—ORRERY.—SOLAR HEAT.—ASTRONOMICAL SLIDES.—DENSITY OF NEPTUNE.—VULCAN.—THE GRAVITY OF SIR R. PHILLIPS.—PLANETARY RINGS.—THE GREAT BEAR.—SIRIUS AND ORION.—ALGOL AND MIRA.—STAR LETTERS AND NUMBERS.—NEW STAR IN CASSIOPEIA.—VENUS IN SUNSHINE.—VELOCITY OF LIGHT.—EVOLUTION.—RAINBOW.—SOLAR STORMS.—ANTIQUITY OF THE PYRAMIDS.—MENTAL PHYSIOLOGY.—THE ANTIQUITY OF MAN.—TOADS.

[138]—As no one, so far, appears to have replied to the Query (17) of "Astronomicus," on page 60, perhaps I may say, shortly, that the seeming retrogradation of a planet is a differential phenomenon, arising from the fact that the earth moves less rapidly in her orbit and describes a larger ellipse than the interior planets Mercury and Venus; and more rapidly than those exterior to her—Mars, Jupiter, Saturn, Uranus, and Neptune, whose orbits must obviously increase in size as they recede from the sun. If "Astronomicus" will describe a series of concentric circles to represent the paths of the planets, and suppose them to travel round such circles in a direction opposite to that of the hands of a watch, and at rates proportioned to their periodic times (to be obtained from any Primer of Astronomy whatever), he will soon see how, taking the case of Mars, for example, when the earth is between that planet and the sun, she, as it were, overtakes him. Evidently in doing so Mars will appear to go back, as it were, among the infinitely distant stars; as will, in a similar manner, the planets outside him when in and near opposition. So far the appearance of retrogradation may be called subjective. I may now add, what I abstained from saying in connection with Mercury and Venus, for fear of confusion, that they have also an objective movement of retrogradation, when they come between the sun and the earth, as your correspondent may see from his own figures. Mars is the planet which describes the largest retrograde arc, the reason for which will also be apparent from his diagram.

"Vulcan" (query 29, p. 80) may see an orrery at South Kensington.

If "Warmus" (query 30, p. 101) possesses a pair of compasses, he may clear up part of his difficulty by constructing a simple figure; or he may even do so by the aid of a bronze halfpenny. Let him, then, describe a circle one inch in diameter, and from the top and bottom of this draw two parallel straight lines, between which he may rule as many more as he likes strictly parallel to them. These he may take to represent the sun's rays. Now, let him get a strip of card one-inch long, and hold it square across his series of lines. Obviously they will all fall upon it. If, however, he holds it slantingly across them, he will see that fewer and fewer of them touch it as its obliquity increases. Now this is just what happens on the earth. Within the tropics, where the sun is always somewhere overhead, all his rays fall square to the observer's horizon. Where, however, as in high northern and southern latitudes, the sun never, even at noon, reaches any great elevation above the horizon, his rays necessarily fall obliquely, and we receive comparatively few of them. With reference to the concluding part of "Warmus's" question, the gentleman who gets tanned by the sun in a tropical country has his complexion spoilt by the direct and immediate action of the sun on the *rete mucosum*, or under layer of the skin. It is this that sunlight enters directly, and not the earth first!

If "C. J. S." (query 33, p. 101) only wants his slides for one night, he will find that patent plate glass, densely smoked by burning camphor, will give him an absolutely opaque ground, on which very effective diagrams may be made by the aid of etching and sewing needles and compasses with a needle point. He must gum a little bit of paper on before smoking his slide, as a centre for one leg of the compasses to rest on, as otherwise they slide about the glass and make undesirable lines and scratches.

Assuming that we are looking at the outline of the *solid body* of Neptune, then I may tell "S. S. S. S." (query 38, p. 101) that his density must be something like 0·96; that of water being 1. Only one satellite is known. Its period is 5 d. 21 h. 3 m.

In answer to "S. S." (query 41, p. 101), there is a practical concensus of opinion among astronomers that Vulcan has no existence whatever. I am not denying that there may be a planet, or planets, between Mercury and the Sun, but the pseudo-observation of Lescarbault was sufficiently definite to enable computers to calculate the orbit of the alleged body. This having been done, it *must* have reappeared on the sun's disc on several occasions since, when the most skilled observers, employing powerful instruments, have wholly failed to see anything. It is the "Mrs. Harris" of the solar system.

"Meter" (query 45, p. 101) appears to be unaware that scientific men pay about the same kind and amount of attention to the lucubrations of Sir Richard Phillips that they do to those of, say, "Parallax," or Mr. John Hampden.

The same correspondent (succeeding query) should buy and read "Saturn and its System," by the Editor of this journal.

Mr. St. Clair (query 57, p. 101) will find that the principal stars in the Great Bear all set in latitudes below 25° North. The second part of his query is, in one sense, meaningless; because in the case of a circum-polar constellation, what is east below the Pole, becomes west above it, and *vice-versâ*; but, in the sense that the right ascension of Benetnasch exceeds that of Dubhe by some 2 hours 47 minutes, of course the latter star is the westernmost, as by "longitude" your correspondent obviously means right ascension. I may say that Ursa Major straggles over six hours in the sky, as nearly as may be from *o* on one side to Σ 1830 on the other. I quite fail to understand the meaning of the constellation having "a cycle of 2,700 years;" nor am I in any better position to clear up the difficulty as to the supposed connection between Sirius and either Ursa Major or Minor.

In answer to query 58, p. 102, Sirius was one of Orion's hounds, but that it was ever placed on the "shoulder" of the giant is news to me.

The great comet spoken of by "S. C. H." (query 61, p. 102), has no other designation than comet *b*, 1881. Its orbit was seemingly parabolic, and the parabola—as "S. C. H." doubtless knows—is a curve which does not return into itself.

"S. C. H." (query 62, same page) will find Algol marked on the map on p. 97. It is in the head of Medusa.

"F. H. S." (query 63, p. 102) asks when Algol and Mira are at their brightest? In the case of the first-named star, he should rather ask of the date of its minimum, inasmuch as it shines as a star of the second magnitude for about 2d. 13h., diminishes to the fourth in three or four hours, remains as a fourth magnitude for about twenty minutes, and again increases in brightness until it regains the second magnitude. It passes through the entire cycle of its changes in 2d. 20h. 48m. 55s. For its next minimum, "F. H. S." must really watch for himself. Mira Ceti should attain its next maximum about June 16 or 17, 1882.

Stars, I may say, in answer to query 64 (p. 102), are both lettered and numbered in their order of right ascension.

By the periodical star, concerning which "F. H. S." puts his next query (65), his informant probably meant to imply that if the one which blazed out somewhere about RA 0h. 19m., and Dec 63° 24' N. in November, 1572, was identical with those which appeared in or near the same spot in 945 and 1264, it may possibly soon reappear now.

Next, I would tell him (query 66) that Venus is always visible to the naked eye in bright sunshine to anyone who knows where to look for her—at and about the time when the *Nautical Almanac* gives the intimation "♀ at greatest brilliancy."

It may be worth while to remark, in connection with letter 100 (p. 116), that the latest and best determination of the velocity of light, gives it as 186,380 miles a second. Hence, as the pace of the fastest express never exceeds sixty miles an hour, it must travel $\frac{1}{60}$th of a mile in a second, so that the velocity of light must be in reality about 1,118,000 times that of the train.

Mr. Mitchell (letter 107, p. 117) might advantageously study Professor Huxley's lecture on the convincing evidence, from fossil remains, of the direct descent of the horse from the Hipparion, delivered before the Zoological Society in December, 1880.

"G. S. M." (query 67, p. 122) should read Tyndall's "Six Lectures

on Light," published by Longmans, for an explanation of the rainbow. It is too long to give here. Unless the same correspondent (query 68) knows something of the theory and action of the spectroscope, I am afraid that he will not be much wiser when I tell him that the velocity of solar storms is measured by the displacement of certain lines in the spectrum of the sun's surroundings.

I am not familiar with Rawlinson's researches in Egyptian history, but "Actinolite" (query 72, p. 122) will find an exhaustive argument in Bunsen's "Egypt's Place in History," tending to fix the date 3620 B.C. as that of the Pyramids.

"S. S. S. S." (query 79, p. 122) cannot possibly do better than get Carpenter's most interesting and amusing "Mental Physiology."

The researches of Mr. Horner, concerning which "Clio" puts query 84 (p. 123), were undertaken between the years 1851 and 1854, partly at the cost of the Royal Society, but mainly at that of the Viceroy, Abbas Pacha. No less than ninety-five shafts were bored through the alluvium of the Nile Valley, through sediment which the French engineers calculate is formed at an average rate of 5 inches in a century. At depths varying from 16 to 24 feet, "jars, vases, pots, and a small human figure in burned clay, a copper knife, and other entire articles were dug up." "Pieces of burnt brick and pottery, Sir Charles Lyell moreover tells us, were extracted almost everywhere, and from all depths, even where they sank 60 feet below the surface." If then we divide 60 feet, *i.e.*, 720 inches, by 5, we get 144, and as the estimation of the French engineers appears entirely trustworthy, that 5 inches of Nile alluvium are deposited in a century, this shows that pottery and brickmaking were practised by the Egyptians at least 14,400 years ago.

In connection with query 87 (p. 123), I should like to put one myself. *Do* toads ever exist "for many years, enclosed in blocks of solid matter?" I fear that "Arachnida" is the victim of the "Great Gooseberry" column of some local newspaper.

A Fellow of the Royal Astronomical Society.

"THE COMMON STAR-FISH, CROSS-FISH, OR FIVE FINGERS (*URASTER RUBENS*, LIN., AG.), OCCASIONALLY THE FOOD OF THE SUN-STAR (*SOLASTER PAPPOSA*, LIN., FORBES)."

[139]—Sir John G. Dalyell, in his excellent work, "The Powers of the Creator displayed in the Creation" (Vol. i., p. 3), in speaking of the common sun-star, states that "the fishermen believe that this animal devours the *Asterias glacialis*" (*Uraster glacialis*, Ag., Lin., Spiny Cross-fish). I am able to confirm this assertion, so far, at least, as it relates to the closely-allied, but far more common species, the *Uraster rubens*, or common five-fingers. Whilst on a visit for some weeks at Colwyn Bay, North Wales, during the months of September, October, and November last, I had ample opportunities of observing some of the habits of the sun-star, great numbers of which I found amongst the sea-tangle (*Laminaria*) beds, which, forest-like, waived their fronds in the tidal waters close to the fishery weir at Rhos, in the possession of Mr. Parry Evans. At the time of the low equinoctial tides, this locality abounded in specimens of *Solaster papposa*; it was the commonest species present. I could have collected wheel-barrows full. Of course, the common star-fish, or five-fingers, was there also—it has "a finger in every pie"; but it did not abound to the same extent as the sun-star. Here and there I met with *Cribellæ*, both *Oculata* and *Rosea*, as well as other members the Asteroidean and Ophiuridean order of the Echinodermata. It was a very common thing to notice within the stomachs of the larger individuals of *Solaster*, rays of the five-fingers; sometimes there were two, but more generally one ray in a stomach. Now the ray was fresh, having been recently swallowed, and unaffected by any digestive process, now in a state more or less pulpy from the results of that process.

It has been long known that the food of star-fish consists, in a great measure, of the succulent portions of different kinds of molluscs, such as mussels, oysters, scallops, &c., small crustacea, and other animals, which they kill and devour; but so voracious are they, that they will sometimes make a meal of one of their own kind. It is difficult to keep sun-stars alive for any length of time in an artificial state; they soon die, so that I was unable to discover by experiment in what form of diet their propensities might be supposed to lie. Neither was I able to ascertain whether the sun-star attacks the whole living five-fingers, by first of all seizing the pointed part of the ray and gradually sucking in its whole portion, thus causing it to break off from the disc, or whether the remains found in the stomach consisted of rays which had been, either voluntarily or otherwise, detached from the body. The destruction which the five-fingers cause to mussel and oyster-beds is well known, and statements to this effect are corroborated by ample testimony. Fishermen and others practically interested in oyster fisheries, when examined before appointed Commissioners on this subject, bear testimony, one and all, to the havoc occasioned by these star-fish. Captain G. Austin, some time ago engaged in the oyster business at Whitstable, gave evidence before the Commissioners in the following words:—"They (the five-fingers) come like a flock of gulls, and, unless the beds were well dredged, they would soon destroy the spat. There is one kind that will eat an oyster itself, yet it is a singular thing with regard to them that after they have been dredged for a time, they roll themselves up and float away. So much is that the case, that in places where the fishermen have caught ten bushels of five-fingers one day, they will go out the next day and not catch one." (See Report of the Commissioners appointed to Inquire into the Sea Fisheries of the United Kingdom. Vol. II., 1865, p. 1,363.) There is every reason to believe that the sun-star is also destructive to oysters; if these asteroids are found, which is often the case, on oyster beds, it is presumable that they are there for some purpose, and when we know that other closely-allied members of this order possess in an eminent degree, and when opportunity offers gratify, oyster-eating proclivities, it is almost certain that their presence on the oyster or mussel beds has a gastronomical explanation. Sir J. G. Dalyell has recorded instances of sun-stars exhibiting cannibalism, and has been an eye-witness of larger specimens devouring their smaller brothers. The destructive agency of the sun-stars with respect to oysters must be small compared with that wrought by the ubiquitous five-fingers, for though the former are widely distributed round our coasts, they are not so generally abundant as the latter. That the sun-star frequently feeds upon the five-fingers I have lately had proof, but whether the good they do in this respect is outbalanced by the evil they cause to oysters, or whether the good has any appreciable effect on the natural increase of the five-fingers—these are questions I cannot answer.—Yours, W. Houghton, M.A., F.L.S.

WORD-CHOICE.

[140]—I hold that an Editor who amply lends his pages for the actual use of the public at large, as well as for delight and teaching, has a right to look for every aid that public can give him. Each writer offering matter for insertion should be asked to consider the *length* of every word he sets down. While thus sparing the printer's costly time, and leaving blank the space otherwise needlessly filled, he will at the same time be serving his own ends, since more space, more letters. Not only so, but in another way he will be a gainer. He will soon find that he is getting to write better Saxon English. Rather write *enough* than sufficient, for instance; *begin* than commence; as well as letting us have *using* instead of employing when the sense permits. You can *use* your time, though you must *employ* a workman. In the above few lines I had myself tripped. I was about to write "entertainment and instruction." Besides being shorter, "delight and teaching," are, I think, better.

Word-lover.

ARE WOMEN INFERIOR TO MEN?

[141]—I have just seen Knowledge for December 2, and have read with much regret a letter (numbered 61), signed "Susan G.," relating to your interesting article "Are women inferior to men?" If the writer is to be considered a representative of her sex, I fear she has injured the cause she wished to advance. In the first place, some of her statements are not accurate. She says "in a girl's education the brain is but slightly exercised." Now in point of fact, in the present day, girls' education is quite as comprehensive as boys', with less relaxation in the way of cricket, football, &c., to counterbalance the study. "Music and needlework," your correspondent thinks, "scarcely exercise it (the brain) at all." I wonder what Haydn, Handel, and Bach would say on that point? If music *is* music, and not mere mechanical "strumming," it cannot be called a brainless study. And yet it has to be worked at in the hours that boys devote to football.

As to "Susan G.'s" theory that whipping would develope brain, it appears to me that people with brain do not want whipping. The two great incentives to study are "interest in the work" and "love of the master," and if those who have the training of boys and girls can inspire these, as every good teacher can, the brain results will be much better than those shown by a girl of eighteen taken from her natural pursuit (?)—"husband-catching," and whipped into a course of study for which she has no inclination.

Having entered a protest against the whipping theory, may I ask you, for my own information and that of others, whether a comparison, to be quite fair, should not be between those whose surroundings are similar? Do you not think that as the mind of a man having the care of motherless children or of a sick wife, becomes insensibly domesticated, and almost womanly (as distinct from effeminate), so the mind of a woman called upon to enter a profession or manage a business becomes widened and strengthened by

contact with the outer world, for the work before it? But whilst the lot of most women (without being doomed to "suckle fools and chronicle small beer") is yet to fill, as intelligently and happily as may be, the domestic posts of daughter, sister, wife, and mother, would it not be unfair to call upon them to exhibit qualities for which they would have no exercise, and which, if ever needed, come with the need? The "Jack Sprat" story of our nursery days would be falsified if men and women were made exactly alike, and I fear the result would sometimes be that the domestic platter would not be "licked clean." I may be wrong, being

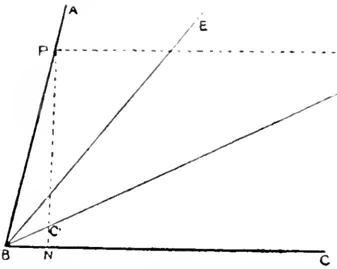
ONLY A WOMAN.

P.S. Let me add one word of grateful thanks for your paper. May it have the success it deserves.

Edinburgh, *Dec.* 9, 1881.

ANOTHER TRISECTION OF AN ANGLE.

[142]—I send you another method of trisecting an angle, more suitable for practical purposes. Of course it requires the ordinary mechanical contrivance, but as the point P is arbitrary, and CD is double BP, these measures can be permanently marked on the scale. To trisect any angle: ABC is the angle. Take any point



P in AB, draw PM parallel and PN perpendicular to BC; from B draw BD so that $C'D=2BP$; bisect PBD. The lines BD and BE trisect the angle. The proof I leave to the ingenuity of your readers. It is very simple.—Yours, &c., S. B.

15, Bessborough-street, St. George's-sq., S.W.

[This is the method invented by Pappus, of Alexandria. From it we can easily see that the hyperbola is a trisectrix.—Ed.]

VARIABILITY OF PENDULUM.

[143]—There is a misapprehension here founded on want of precision in ideas. The writer points out "a difference of velocity," but omits to specify to what point he refers as the standard. Apparently, however, the centre of the sun is intended, for which his statement is approximately correct; but the motions of the pendulum are controlled by the mass of the earth, generally reckoned as a force acting from its centre, and with reference to this latter point, no such inequality of velocity exists; so there is no ground for anticipation of any diurnal variation dependent on this (supposed) cause. COGITO.

ANTIQUITY OF THE PYRAMIDS.

[144]—It is impossible in the space which could be allowed in KNOWLEDGE to give adequate reasons for believing some of the Pyramids to be 6,000 years old. If "Actinolite" (query 72, p. 122) wishes to know the best authorities on the subject, he will find Brugsch's "History from the Monuments," Rawlinson's or Erasmus Wilson's "Egypt," and a work published by Macmillan, entitled "A Ride in Egypt," by Mr. Loftie, give him ample information.

If Egyptologists are correct in assigning the Pyramid of steps at Sakkara (which, by-the-way, differs from all others in *not* following the cardinal points of the compass) to Vanephes of the first dynasty, Sir J. Lubbock would probably be within the mark. The Pyramid of Senerferoo, eighth king of the third dynasty, is at Maydoum, and may well be as old as 6,000 years. For the date of Menes is fixed as 5702 B.C. by Boeckh, 5,000 B.C. by Prof. Owen, Marriette, and Manetho, 4,455 by Brugsch, for which he gives excellent reasons, 4,500 B.C. by Maspero, 4,000 B.C. by Chabas, and 3,892 B.C. by Lepsius. English authorities prefer a shorter chronology, Mr. Stuart Poole even favouring 2,717 B.C.

It may be said that all the accumulating discoveries go to prove, with slight exceptions, the validity of Manetho's list and dates. For instance, one of his supposed mythical assertions with regard to a King Apappus, that he was a giant, and reigned one hundred years, has recently been confirmed by a monument now at Boulaq, mentioning the one hundredth year of his reign, and his hieroglyphic name Papi means a giant. This longevity is surpassed by that of Assa and his son Ptah-hotep of the fifteenth dynasty, who, according to the Prisse papyrus, must have lived over 130 and 110 years respectively. See "Smith's Bible Dictionary," I., 322, where these facts are used as arguments in favour of the long lives of some of the patriarchs.

It is important to recollect that if the monuments are held to confirm the old supposed chronology of about 4,000 years from the creation to the time of Christ, they witness in favour of the plurality of the human race, because they show quite as great a divergence between the negro, Egyptian, and Asiatic types on the earliest monuments as at the prefent time, and so there could not possibly have been a sufficient period for such a differentiation from the progeny of a single pair.

A MEMBER OF THE SOCIETY OF BIBLICAL ARCHÆOLOGY.

SOME SUGGESTIONS RESPECTING "KNOWLEDGE."

[145]—1. I think it would be a great advantage to many of your readers if in your notices of books (which I hope to see soon) you stated the price as well as the publisher's name and address.

2. *Sale Column.*—Do not you think that a sale and exchange column (such as in the *English Mechanic*) would be a good addition to KNOWLEDGE? Through it subscribers could exchange and sell scientific works, telescopes, aquaria, &c.

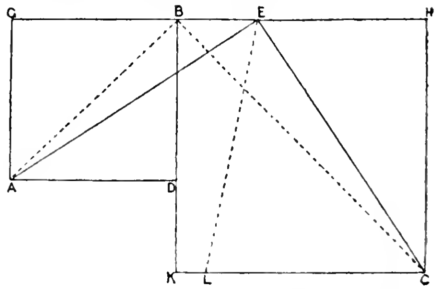
3. *Collections.*—Shall you have papers on making collections of all kinds of natural objects, such as a herbarium, butterfly collections, geological, bird and fish stuffing, skeletonising, insect taxidermy, &c.?—[Freshwater aquaria.—Ed.]

4. I hope you will foretell all natural phenomena, such as eclipses, meteoric showers, &c. (as far as you are able), and how best to be seen, by naked eye or telescope. E. R. C.

THE THREE-SQUARE PUZZLE.

[146]—Mr. Langley's note (No. 97) reminds me of an old puzzle which was set to me in Germany a good deal more than forty years ago. It is, in fact, a variant of his, and shows how his five pieces are obtained. It also always seemed to me to be the best proof of the celebrated 47th proposition of Euclid's first book.

Take any oblong piece of paper and fold down two corners, as along AB, BC in the figure; then, by cutting along AD and CK, you obtain two adjacent squares, AGBD and KBHC. Now, the puzzle is by one cut with a pair of scissors to cut these two squares into three pieces which will themselves form a square. Let those who are fond of puzzles attempt it without reading what follows.



Solution.—Fold the paper so that the point C lies on the point A. Then the fold will be along the line EF, giving the point E, where, of course, EH = GB. Cut with the scissors through both of the folded sides from A to E, and C to E, by one cut. Then you have the two triangles AGE, CHE, and the shapeless remainder. This done, I may leave any child to fit them into a square, and any young geometer to prove that that square must be that on the hypotenuse of a right-angled triangle of which the sides are also sides of the two squares. Observe, that if you also cut along BD, you get Mr. Stanley's five pieces.—Yours, &c., ALEXANDER J. ELLIS.

THE ORIGIN OF BUTTERCUPS.

[147]—I am sure that most of our readers could not fail but notice the clear and highly descriptive article upon "The Origin of Buttercups," a valuable treatise upon a botanical subject, without botanical technicalities, and rendered in such a manner that even the least unscientific person reading the article would have his knowledge considerably extended respecting buttercups.

I cannot, however, understand Mr. Grant Allen's theory of evolution respecting cut leaves. He says, "As a rule, just in proportion as vegetation is thick and matted, do the plants of which it is composed tend to develope minutely divided and attenuated foliage." I hold that the rule is the very reverse, for in traversing any waste piece of land where the struggle for existence is left to itself, one sees the very plants that predominate are those with entire leaves, viz., grasses, daisies, primroses, docks, &c., &c., and hold their own against the finely-cut leaved species; indeed, one scarcely sees the buttercup with its cut leaves growing in any profusion on uncultivated ground.

It is only in protected meadows that it flourishes in abundance; whereas the farmer carefully uproots those having entire leaves, knowing well that under their broad leaves no grass will grow.

Those plants having cut leaves are, as a rule, "tall growers," and have a weak and slender stem compared with the height of the plant. Have not these developed cut leaves in order that the wind may not break them down, but allow it to pass freely through? If, on the contrary, they had to develope entire leaves, they would be at a disadvantage in even an ordinary breeze. Upon the same principle many tall plants have entire leaves, but as they approach the top of the stem, the leaves are invariably smaller. Consequently, where the plant is weakest the leaves are smallest.

Again, many plants having entire leaves throw up a flowering skin entirely devoid of leaves, probably learning by experience that if they had to produce leaves there, snap would go their stems —viz., plantain, foxglove, hyacinth, grasses, &c.

It may be argued that few of our large forest trees have cut leaves, and that, therefore, my theory respecting the wind would fail. But I hold that they possess advantages equal to cut leaves in having their blades placed at the end of petioles, and articulated to the stem, so that they may bend and allow the wind to pass without injuring the tree. I hold that petioled leaves are merely a modification of cut leaves. Few of our radical leaved plants possess petioles to their leaves, not being required for such a "lowly" position. West Riding.

BEES AS FLOWER FERTILISERS.

[118]—In the suggestive article by Mr. Grant Allen in Knowledge No. 4, on the "Origin of Buttercups," an error has crept in with regard to bees. I am a bee-keeper on a somewhat large scale, and, consequently, interested in knowing from what flowers bees make their collections; but, though we have abundance of buttercups here, the hive bees do not visit them. Certainly buttercups are not included by bee-keepers among honey plants.

I do not, however, take exception to the assertions of scientific men, that bees are important aids in the cross fertilisation of flowers. When I say that the number of good honey-producing plants are comparatively few, and bees select the best yielders for the time being, I mean the probabilities appear to me to be very much against flowers being dependent upon bees for the fertility of their seeds.

Another circumstance is worth notice. When hive bees make collections of pollen from osiers, they fly straight to and from the tree, without passing from the male to the female flowers, a fact which may be easily verified in almost any district where bees are kept. It appears to me that bees might be of service as an aid in cross-fertilisation in this case; but it is fair to add that pollen is much needed in early spring when osiers bloom. This shows that the presence of honey in flowers is not the only attraction for the industrious bee.—Yours, &c., Alfred Donbavand.

Picton, Chester, *Dec.* 16, 1881.

Professor Nordenskjold is going to Russia, where the Government wishes to consult him on the best method of opening North Siberia to European navigation.

At the late Archæological Congress, held at Tiflis, Professor Samokvasoff gave an account of his discoveries in the graves near Pyatigorst. He excavated about 200 places of interment belonging to the stone, bronze, and iron periods. In the larger graves, bronze implements were found with stone ones. As there were in these graves, besides the bones of sheep, several split human bones not belonging to skeletons, he inferred that during the bronze-period the people in that part of the Caucasus were man-eaters.—*Frank Leslie's Magazine.*

Queries.

[104]—Moons of Mars and Saturn.—1. Can you inform me if Mars has any satellites? If so, how many? What is their period of revolution, and at what distance are they from the planet?—[Mars has two moons, discovered in August, 1877; periods of revolution, 30h. 14m., and 7h. 38m.; distances from the planet's centre, about 6,000 and 15,000 miles.—Ed.] 2. What are the names of Saturn's two outmost satellites?—[Hyperion and Japetus (the latter the outermost of all).—Ed.]

[105].—Fungi and Lichens.—How are specimens preserved, and which are the best books on the subject?—J. S.

[106]—Pneumatic Bell.—Will M. Tester (Letter 79, p. 100) kindly favour me with instructions, "plainly-worded, exactly-described," for making and fixing (down staircases, &c.) a pneumatic bell; taking for granted—as is really the case—that I have very little native ingenuity? Also stating the price of materials used. If he could possibly supply a simple diagram, where necessary, I should be greatly obliged.—E. R. C.

[107]—Name of Star.—How would you pronounce "Betelgeux," one of the stars (a) in the constellation of Orion. [The question is one for Arabic students. The name Betelgeux is, we believe, a corruption from *Ibt al Jauza*, the giant's shoulder.—Ed.]

[108]—After-Images.—These seem to be best obtained, not by closing the eyes and keeping them closed, but in the *act* of vigorous winking. When I do this after looking at the sun, I soon obtain an after-image of the following nature: An interior green disc; round that a narrow circle of red, and beyond, an undefined space of violet or dark blue. Has this been noted and sufficiently explained, and, if so, what is the explanation? Is there any significance in the fact that those three colours are the fundamental colours?—Max.

[109]—Railway Collisions.—From the history of these, are any practical rules deducible for the case of imminent collision (rules as to position and attitude in the carriage, &c.), the observance of which might somewhat limit the power of railway companies to maim and murder us?—Max.

[110]—The Coal Age and the Earth's Internal Heat.—As an ignoramus, may I ask you whether the earth's own internal heat had anything to do with the gigantic flora of the coal age, on the principle of a hothouse? Is it supposed that the earth was at one time in a state of intense heat, and will gradually cool like the moon has done? Does not the temperature increase the deeper you sink into the earth? What is a good text-book on this subject?—Ignoramus.

[111]—Axes of the Planets.—Will you inform me if the axes of the planets are all inclined the same way—that is to say, if the sun and all the planets could be placed in a line at their respective distances, would all the axes be inclined in the same general direction?—Orion.—[The axes of the planets have no community of direction. Our earth's axis points towards Ursa Minor, Jupiter's to a point in the constellation Draco, not far from Omega, Saturn's to a point near the northern extremity of Cepheus, and so on.—Ed.]

[112]—Optical Illusion.—A remarkable optical illusion is caused by holding the hand, with the fingers close together, near a gas flame. One seems to see through his fingers. What causes the illusion? Does the light shine through the skin at all?—G. P.

[113]—Illusory Figures of Transparent Solids.—How is it that in looking at the figures of transparent solids that it is possible for you to imagine the figure in two positions? And what adjustment takes place in the eye whilst so doing? For instance, in the case of the cube it is possible for you to see it as having a certain face as the front one, and without moving the eyes you can make the opposite face appear as the front one; or in the prism, the edge xy can be made to appear either at the back or front, just as you please; but no motion of the eyes need take place. Yet there is a slight effort felt as you change, so to speak, the position of the cube or prism.—P. B. S.—[Mr. Foster will discuss such illusions.—Ed.]

[114]—Marine Boiler.—What is the increasing temperature on the furnace crown of a marine boiler required to keep the steam at a pressure of 100 lb. per square inch; with the scale formed by the deposit of salt within the boiler increasing by sixteenths, from $\frac{1}{16}$ to $\frac{1}{2}$ in. thick? How can this be calculated?—Crankshaft.

[115]—Screw Propeller.—Is there a vacuum on the following side of a screw propeller blade when revolving? If so, does it materially affect the power of the engine? Any information on either of the above will greatly oblige.—Crankshaft.

[116]—John Bull.—Can any reader refer me to the origin and meaning of the term "John Bull."—G.

[117]—Parallelopipedon or Parallelepipedon.—Why do

English writers spell "Parallelopipedon?" Both in German and French this word is always spelt "Parallelepipedon" and *parallélépipède* (comp. Dictionnaire de l'Académie and Littré's Dictionnaire), and one of the greatest authorities in classical philology assures me that the *o* of "parallelos," being an un-essential termination, *must* yield to the initial *e* of "epi."—G. L.

[118]—How to Make Cotton Waterproof.—Will any of the readers of Knowledge be good enough to indicate a process by means of which raw cotton wool, just as the plant furnishes it, can be made impervious to water, without changing the fibre or colour of the cotton?—Cottonian.

[119]—Electro Plating.—By mistake, nearly six ounces of a cyanide of copper solution have been thrown in two gallons nearly of a double cyanide of silver solution. Would any of your readers be kind enough to tell me how to purify my silver solution?—W. Van Eys.

[120]—Transit of Venus, Dec., 1882.—As the above transit will take place in December next year, I should very much like to know how the calculations are made in order to ascertain the sun's distance from the earth. I have a slight knowledge of how the work is performed, but should like to understand the system properly.—Hesketh. [Will, before long, give a simple account of the matter.—Ed.]

[121]—Latitude and Longitude.—I am anxious to be able to determine the latitude and longitude in a practical manner. Will you kindly, at your earliest convenience, give me the necessary instructions in Knowledge?—Hesketh. [The best advice we can give "Hesketh" is to obtain Loomis's excellent treatise on "Practical Astronomy," in which the best methods of determining longitude and latitude are clearly and fully explained.—Ed.]

[122]—Blood Analysis—Is there any method by which the amount of the albumen, or, at least, of the saline constituents of blood, can be estimated quantitatively, in cases where only a small quantity can be obtained, say 5 c.c.? Can the spectroscope be applied to quantitative analyses in such cases?—*ἄιμα*.

[123]—Fermentation in Beer.—Will you kindly allow me to ask if any of the readers of Knowledge will explain to me the process of fermentation in beer?—In Re.

[124]—The Coffee Leaf.—Have you heard anything lately of any experiments made in this country to introduce the coffee leaf among us? My reason for asking is that I think our labourers would derive a great benefit from its use, if it could be introduced.—F. C. S.

[125]—Indigo.—Is it possible to prepare indigo on a large scale artificially, and at the same time profitably? I have heard rumours that there is a manufactory in France for doing so; but have looked through, as they have arrived, my society's journals to see if I could find an account of the experiment, if any has been made, but it has been without success. If you could enlighten me on the above you would oblige.—F. C. S.

[126]—Flora, and Plants, and Insects of the Channel Islands.—Why are the plants of the Channel Islands included in the British flora, while the insects are usually considered French?—B. J.

[127]—Double Refraction.—I should be very glad indeed of information upon the following points:—When a spot of light is viewed through a piece of Iceland spar, two spots are seen, as a consequence of the double refraction of the crystal; and if I understand rightly, one of them consists of vibrations, say, in vertical, and the other of vibrations in horizontal planes. Now, if we look at these through a second piece of spar, we see four spots of light, each of the former pair being doubled or "resolved." What I want to know is the meaning of "resolved." If one of the first pair consists of vertical vibrations, where do the horizontal ones come from to form its second image? and if the other consists of horizontal vibrations, how do we come by the second image? Each pair behaves, when the spars are revolved, in the same manner as the first pair would if seen through a Nicol prism.—Polar. [It depends how the second piece is held. If it is held with its principal section parallel to the other's, only two images are seen, one by ordinary, the other by extraordinary refraction. The same if the principal sections are 180° from each other, unless the crystals are of equal thickness, when there is but one image. If the principal sections are at right angles, only two images are seen, which are (1) the ordinary image after extraordinary, and (2) the extraordinary after ordinary refraction. In intermediate positions, four images are seen, because neither the ordinary nor the extraordinary refracted rays would have, in order to pass through, to turn through quite a right angle, which, and which alone, would involve total extinction.—Ed.]

[128]—The Eye as one of the Senses.—I wish to give an essay, or a reading on this subject at a Working Man's Mutual Improvement Society. I should be extremely obliged with any information that would enable me to get a cheap pamphlet, or anything that will help me to prepare such an essay on "The Eye, one of the Gateways of Wisdom."—T. T.

[129]—Medical Botany.—Will you kindly tell me through your valuable paper the name of the best medical botany, with diagrams. Henry Hawkes-Spink.

Replies to Queries.

[26]—Orrery.—In the Museum of Philosophical Apparatus, at University College, Gower-street, London, was, and probably now is, the Orrery made by the celebrated self-taught astronomer, James Ferguson.—W. H. Hardy.

[66]—Venus Casting Shadow.—In India I have repeatedly seen Venus at least two hours before sunset. In 1849 or 1850, while driving on a moonless night, we perceived on the road a distinct, though faint, shadow of a clump of trees, of two large poles, &c., cast by Venus. This was not far from Delhi, in a very dry, clear atmosphere.—Cogito.

[72]—Antiquity of the Pyramids.—Notwithstanding Sir John Lubbock's statement, there is no trustworthy evidence that the Pyramids "are at least 6,000 years old." The Great Pyramid of Geezeh is generally admitted to be not only the greatest, but also the oldest, and astronomical considerations based upon the position of its entrance-passage—which constitute evidence of a much more reliable character than that of ancient Egyptian traditions—indicate that its age does not exceed 4,051 years.—J. Baxendell.

[Mr. Baxendell fails to notice that the direction of the entrance-passage fulfilled the condition of pointing towards Alpha Draconis at its sub-polar passage, at two epochs during the last 25,000 years—one that which he mentions, the other some 6,000 years ago. Moreover, what no one seems to have noticed yet, the ascending passage, which 4,051 years ago would have been directed towards no important star, would have been directed towards the most interesting orb in the whole star-sphere—namely, Alpha Centauri—at its southern culmination, at the earlier epoch. It has been supposed by persons unacquainted with astronomy that the Pleiades occupied the corresponding position 4,000 years ago. But this is not the case. The relation indicated by Prof. Smyth was only a symbolical, not an observational one.—Ed.]

[83]—Chemical Queries.—In reply to "Theion" (*a*), there are several allotropic modifications of sulphur, as the following table shows (Valentin's "Introduction to Inorganic Chemistry, p. 53):—

	Sp. G.	Fusing Point.	Solubility in CS.2.
1. Octahedral.	2·05	115	Soluble.
2. Prismatic.	1·98	120	Insoluble before transformation.
3. Plastic.	1·95	Are converted into the octahedral modification.	Insoluble.
4. Amorphous.	1·95		Insoluble.

Another modification insoluble in CS_2 is contained in flowers of sulphur, and is of a light yellow colour (Roscoe and Schorlemmer's "Treatise on Chemistry," vol. i. p. 292). If "Theion" will carefully read Roscoe's "Lessons" again, he will find that the plastic modification, while *tenacious*, is insoluble in CS_2. When the tenacious form has become *hard* and *brittle*, a portion will dissolve in the CS_2, and leave a dark-brown powder ("Treatise," vol. i. p. 292). Miller and some other chemists call this a grey amorphous powder. This latter modification corresponds to No. 4 in the above table. (*b*.) The word *nitrous* should be *nitric*. It is stated correctly in the "Treatise on Chemistry," vol. i. p. 126.—C. W. D.

[84]—Ancient Man.—Mr. Horner's researches were published in the "Philos. Trans.," 1855 and 1858, and reviewed in the *Quarterly Review* in 1859. His argument for an extreme antiquity of Egyptian civilisation from finding fragments of pottery at considerable depths in the alluvial soil is now considered insufficient. Sir C. Lyell says, "it is not worth while to notice such absurdities." Other pieces of Greek origin have been exhumed at greater depths, and I believe the Greek honeysuckle ornament was found on some of Mr. Horner's. Stephenson turned up a brick of Mehemet Ali's, even at a lower level. Stone implements from Egypt have frequently been exhibited at the Anthropological Institute, by Sir J. Lubbock in 1874, Capt. Burton in 1875, and General Pitt Rivers in 1881, but with regard to many of these, there is great doubt as to whether they are hand-fashioned, and even those which are, do not prove the existence of a stone age in the Nile Valley, for Prof. Owen says, "Chert, chipped to an edge, or flint flakes struck off by percussion, being the ordained materials for circumcision and for the abdominal cut in mummifying, the finding of flint knives in Egypt requires evidence of the date when they were used, or of previous manufacture, before they can apply to the question of a

race anterior to the historical life of the country." Mariette says, "the use of stone and flint tools extended nearly 1,000 years, through thirty dynasties." Brugsch tells us, "Egyptian history throws scorn on the supposed periods of stone, bronze, and iron;" and Chabas shows that similar tools were in use at the latest historical period, and even now are not uncommon among the Arabs. —A Member of the Society of Biblical Archæology.

[84]—Antiquity of Man, as Shown by the Works of Art in Nile Mud.—Your correspondent "Clio" may find Mr. Horner's researches in the "Philosophical Transactions," 1855-8, or perhaps she will do better by consulting an admirable summary in Lyell's "Antiquity of Man," p. 35 and 41, 4th edition, 1873. The result is a simple rule of three sum. As is the thickness of Nile mud deposited by the annual inundations over certain historical monuments to their known age, so is the greatest depth at which the works of art occur in like mud hard by to their approximate age.—A Tramp.

[89]—Collodion Plates.—"Persevere" does not say whether the red stains appear after the plate is developed, or whether they are on the plates as received from the maker. In either case, it would be almost impossible to determine cause without inspection. The vagaries of gelatine plates are innumerable. If the negatives are not varnished, they are very liable to be spoiled by the silver from the paper; the gelatine readily absorbs moisture. The remedy is obvious. Evidently "Persevere" has not tried to answer his own question as to developing gelatine plates; let him try, and give his experience, if successful.—A. Brothers.

[91]—"Missing Link."—Dr. Andrew Wilson's article is on "Missing Links" (not on *the* "Missing Link"). It appeared in the *Gentleman's Magazine* for September, 1879.—Ed.

Notes on Art and Science.

Among recent finds at Pompeii were several amphoræ, on some of the largest of which was written the exact date of the extraction of the wine contained within, and on smaller ones the names of the wine.

It is announced that Dr. King, in charge of the Government cinchona factory at British Sikkim, has succeeded in manufacturing, for the first time in India, sulphate of quinine from local cinchona bark. The samples produced are said to bear comparison, on analysis, with the pure sulphate of quinine of commerce, and preparations are being made for undertaking the manufacture on a large scale.

The famous spring of boiling water in the middle basin of Hell's Half Acre, in the Yellowstone region, has lately become still more wonderful as a geyser. Four or five times every twenty-four hours it discharges a great column of water, freighted with stones and obscured by a dense volume of steam. The hollow formation for hundreds of yards around the orifice trembles under the upheaval, and the water is thrown to a height of a hundred feet.

The six healthiest cities of the United States are said to be in the order following:—Utica, Dayton, New Haven, Portland, San Francisco, and Lawrence. The unhealthiest are Charleston, Memphis, Cleveland, Chicago, and Lynn. St. Petersburg is the unhealthiest city in the world, and is followed by Charleston, Malaga, Alexandria, Warsaw, and Buda-Pesth.

M. Macagno, in *Les Mondes*, states that he has been making experiments on the influence of electricity upon the growth of the vine. An electric circuit was formed by copper wire between the extremity of a branch bearing fruit and its origin near the soil. More wood was formed in the branch, which contained less potash than the other parts, and the grapes ripened more readily, containing an excess of sugar.

It is said that a marked improvement has been noticed in the acoustic properties of the Grand Opera House, Paris, since the introduction of the electric light. A layer of heated gases acts as a screen for sound, hence the volumes of hot fumes arising from the old gas footlights obstructed and marred, to some extent, the voices of the singers. With the electric light, inclosed in air-tight bulbs, no fumes can be emitted, and very little heat is given off. Hence its benefits to the ear as well as to the eye.—*Frank Leslie's Magazine*.

The question of the existence of volcanoes in Central Asia, especially on the Kuldja frontier, has always been a matter of doubt and discussion among geologists and Russian explorers. The Governor of Semfretchinsk, General Kolpakofsky, had already fitted out expeditions to settle the question—one in 1878, and again in 1879; but owing to the difficulties of reaching the mountains, which the Chinese considered impassable, and also to disorders which were then taking place in Kashgar, both expeditions were unsuccessful. This year General Kolpakofsky again set himself to the task, and now reports that he has at last discovered the perpetual fires in the Thian Shan range of mountains. He telegraphs that the mountain Bai Shan has been found twelve miles north-east of the City of Kuldja, in a basin surrounded by the massive Ailak Mountains, and that the fires which have been burning there from time immemorial are not volcanic, but proceed from burning coal. On the sides of the mountain there are caves emitting smoke and sulphurous gas.—*Frank Leslie's Magazine*.

Our Mathematical Column.

THE USE OF LOGARITHMS.

In our last, we took a simple case of the multiplication of two numbers, each of six digits, and the division of the products by another number, also of six digits. Working this sum by logarithms seemed rather long, just as in practice nearly every one finds the first two or three sums he works by logarithms require more care and watchfulness than he afterwards finds necessary. We may now, however, proceed more freely.

Let us consider a few cases of taking powers, or extracting roots of numbers.

Take first the familiar problem of the horse with 24 shoe-nails, for which a price of $\frac{1}{4}$d. for first nail, $\frac{1}{2}$d. for second, 1d. for third, 2d. for fourth nail, and so on, doubling to the twenty-fourth nail, was to be paid: to find his price by the aid of logarithms. This is a case somewhat unlike those usually dealt with, where an answer exact to so many decimal places is required, not an answer absolutely exact. However, it can be readily solved by logarithms. Thus, for the first nail, amount is $\frac{1}{4}$d.; for first and second, $\frac{3}{4}$d.; for first three, $1\frac{3}{4}$d.; for first four, $3\frac{3}{4}$d. Start from the fifth, for which nail 4d., or a third of a shilling, was to be paid, and let the third of a shilling be our unit (the reason being that if we take pence or farthings, we should not be likely to get an exact result). Then there is to be paid for fifth nail 1, for the sixth 2, for the seventh 4, or 2 to power 2; for the eighth 8, or 2 to power 3; and so on: and for the twenty-fourth, 2 to the power 19, while we know that the total paid for all the nails from the fifth to the last is twice this, less 1, or $2^{20}-1$.

Now, turning to the tables we find—

log. 2 = 0·3010300
Multiply by 20 20

log. 2^{20} = 6·0206000 = log. 104856

log. 10485 = ·0205681
316
difference for 70 = 292 — This part of the calculation is simply working out the logarithm.
240
difference for 6 = 250

We know that 2^{20} must be a whole number, and can end only with one of the digits 2, 4, 6, or 8; so we take with confidence the number 1048576. This, less 1, is the number of 3rds of a shilling for the nails from 5th to 24th inclusive, and the first four give us one-third of a shilling, less a farthing. So the answer is 1048576 thirds of a shilling, less a farthing.

Or 349,525s. $3\frac{3}{4}$d.
Or £17,476. 5s. $3\frac{3}{4}$d.

This is an unfavourable example, because of the necessity for an exact instead of an approximate result. If we had only required to know the amount roughly, that is, within a shilling or two, we might have proceeded thus:—The amount in farthings is $2^{24}-1$, or in shillings the amount is $\frac{2^{24}}{48}$, neglecting the farthing.

Now log. 2 = ·3010300
log. 2^{24} = 7·2247200
log 48 = 1·6812412

log. ($2^{24} \div 48$) = 5·5434788

log. 34952 .... ·5434720
difference for 50 62
5 6

5·5434788 = log. 34952·5
Ans. = 34952·5 shillings.
= £17,476. 5s. 6d.; and the conditions of the problem show that the pence really amount only to $3\frac{3}{4}$d.

In nearly all problems requiring the use of logarithms, however, we do not require exact accuracy, but may be content with approximation to the third or fourth place of decimals.

Let us take a case not scientific, but practical.

Required the amount of £1,828 at compound interest, five per cent. per annum (payable yearly), at the end of ten years.

Any sum at the given rate of interest, is increased in the ratio $\frac{105}{100}$ at the end of the first year; therefore, at the end of two years, it is increased in the ratio $\left(\frac{105}{100}\right)^2$; and so on; and at the end of ten years it is increased in the ratio $\left(\frac{105}{100}\right)^{10}$. Thus we have to find the value of the following expression:—

$$1828\left(\frac{105}{100}\right)^{10}$$

Now log. 1828 = 3·2619762
Ten times log. 105 = 20·2118930
Sum 23·4728692
Ten times log. 100 = 20·
Log. (answer) = 3·4728692 = log. 2970·771
20
15,420
12
5,040

There is an error in the addition. log. (answer) = 3·4738692. True answer somewhat greater than stated.

Answer is £2,970. 15s. 5d.

Here is another question relating to compound interest:—In what time will a sum of money, say £100, be doubled at 5 per cent. per annum, payable yearly, compound interest?

If x be the required number of years, we have

$$(100)\left(\frac{105}{100}\right)^x = 200$$
$$\text{or } 105^x = 2 \times 100^x$$

This is the same as saying that

$$x \text{ log. } 105 = \text{log. } 2 + x \text{ log. } 100$$
$$\text{or } (2\cdot0211893)\,x = 0\cdot3010300 + 2x$$
$$\therefore \quad x = \frac{3010300}{211893}$$

We can again use logarithms to determine the value of this fraction.

We have log. 3010300 = 6·4786098
log. 211893 = 5·3251167

difference = 1·1524931 = log. 14·207
12

2·484

Answer is 14 years $2\frac{1}{2}$ months, very nearly.

For greater exactness, multiply ·207 by 365, giving 75·555, and making answer, 14 years, 75 days, 13 hours, 19 minutes.

The student of our subject is advised to go carefully through each computation. He will note that in taking out the logarithm of 211893, we put down for the first three figures not 325, but 326, though the logarithm is found in a section which seems to have 325 for its leading digits. In this section we find 325 followed by numbers continually increasing up to 9875; then on the same line comes $\overline{0}080$, which, of course, means that the logarithm has increased from 3259875 to 3260080. The student must be careful on this point, especially in the earlier part of the tables, where the changes are more rapid.

One other example illustrating an application of logarithms, in which great time is saved.

Suppose we want to find the cube-root of a number, say, 21,793, correct to the third or fourth decimal place. By the ordinary arithmetical process this would be a long job, and we should have carefully to test the result to insure accuracy. But by logarithms the process is very easy, thus:—

Log. 21793 = 4·3383170
Divide by 3 giving 1·4461057 = log 27·93221.

Thus the cube-root of 21793 is 27·93221.

Take a more complex case, the solution of which by ordinary arithmetical processes, with the same degree of accuracy, would take half-a-day at the very least, even in the case of an arithmetician knowing how to take out the fifteenth and seventh roots of numbers.

Find the value of the expression:—

$$\frac{(1828)^{\frac{2}{15}}(0\cdot1763)^{\frac{1}{3}}}{(745)^{\frac{1}{7}}(0\cdot0051)^{\frac{1}{5}}}$$

log. 1828 = 3·2619762
2-15ths or 1-30ths of this = 0·4349302
log. 0·1763 = $\overline{1}$·2462523
1 third of this, or of −3 + 2·2462523 = $\overline{1}$·7487508

0·1836810 (A)

log. 745 = 2·8721563
1-seventh of this = 0·4103080
log. 0·0051 = $\overline{3}$·7075702
1-fifth of this, or of −5 + 2·7075702 = $\overline{1}$·5415140

$\overline{1}$·9518220 (B)

A − B = 0·2318590
Answer = 1·705529

The student will notice how the negative characteristics are dealt with in such problems. We must always add enough to the negative characteristic to make it exactly divisible by our divisor, treating the number thus added as a positive characteristic for the rest of the division.

Our Chess Column.

GAME recently played at Simpson's Divan between Mr. A. P. Barnes, of New York, and Mr. Gunsberg.

Queen's Gambit declined.

	White. Mr. Barnes.	Black. Mr. Gunsberg.
1.	P. to Q.4.	P. to Q.4.
2.	P. to Q.B.4.	P. to K.3.
3.	P. to K.3.	Kt. to K.B.3.
4.	P. to Q.R.3. (a)	P. to Q.B.4. (b)
5.	P. takes P.	B. takes P.
6.	P. to Q.Kt.4.	B. to K.2. (c)
7.	P. to Q.B.5.	Castles. (d)
8.	Kt. to K.B.3.	P. to Q.R.4.
9.	B. to Q Kt.2.	P. to Q.Kt.3.
10.	P. takes Kt P. (e)	P. takes P.
11.	B. to K.2.	P. takes P.
12.	B. takes P.	B. takes B.
13.	R. takes B.	R. takes R.
14.	Kt. takes R.	Q. takes P.
15.	Castles.	B. to R.3.
16.	Q.Kt. to Q.Kt.5. (f)	Kt. to K.5. (g)
17.	Q. to Kt.3.	Kt. to Q 2.
18.	R. to Kt.sq.	R. to Kt.sq.
19.	Q. to R.3.	Kt. to Q.3. (h)
20.	Q to Kt.4.	B. takes Kt.
21.	B. takes B.	Q. takes B.

White resigns.

NOTES BY "MEPHISTO."

(a) This is preparatory to advancing the Pawns on the Queen's wing. We cannot approve of such a course with all the White pieces yet undeveloped.

(b) In most openings, where the first player opens up the Queen's wing first P. to Q.B.4 (to be followed, if feasible, by Kt. to Q.B.3) will be found effective, as it attacks the centre pawns, which threaten to domineer over Black's game.

(c) The Bishop is sometimes withdrawn to Q.B.2 *viâ* Kt.3, the idea being that on B.2 he is available for attack on the King's side (this is problematical). We prefer B. to K.2, for, in the first instance, it affords some protection against B. to Q. Kt.2. Secondly, we consider the hostile Queen's wing weakened, and from K.2 the Bishop will render assistance in attacking the White Pawns successfully (this is positive).

(d) Necessary before beginning the attack. Many good games are often thrown away through rashness in attack and insufficient regard for one's own safety.

(e) There is nothing better: if 10. P takes R.P., then 10. P. takes B. P., and the Rook's Pawn is lost; or if 10. $\frac{\text{Q. to B.2.}}{\text{P. takes B.P.}}$ 11. $\frac{\text{P. takes B. P.}}{\text{Kt. to K.5.}}$

(f) If B. takes Kt., then Q. to B.3. would win the piece back. White intended to bring his Knight to Q.4, but it would have been much simpler to have brought him *viâ* B.2.

(g) Taking the proper advantage of White's weak move. Black threatens to win a piece by B takes Kt., or, if the Knight retires, by Kt. to B.6.

(h) This ends the struggle. Black now wins the Knight, for if 20. Q. Kt. to B 3. then 20. Q. takes R., and the White Knight cannot take the Queen on pain of mate in 4 moves. If 20. K.Kt. to Q.4 then P. to K.4. wins.

Our problem No. 4 (wrongly numbered 3) in No. 5 is solved by R. to Q.R 8.

We have received correct solutions from Gamma, Arkansas, E. F. K., Caissa, A Young Player, Try Try Again, S. D. P., R. M., Afternoon, Worcester, Etoniensis, D. See. Others incorrect.

Edward Sargant points out that Mr. Healey's problem is unsound. If black Pawn becomes a Rook, white cannot win. This is so. The point appears to have been noted in "Westminster Papers" several months later. By putting the black Rook's Pawn one square further forward the flaw is corrected.

From the *Glasgow News*.
By Mr. C. R. Baxter, Dundee.

Black.

White.

White to play and mate in three moves.

This is the problem to which we referred in No. 6. Mr. Baxter tells us that he had never seen our older problem. We had not supposed he had. Next week, or later, we shall give an instance in which we were anticipated. He notes the resemblance of Carl Eggert's problem, in *Illustrated London News* for November 26, to his own, which appeared the same day.

Our Whist Column.

By "Five of Clubs."

H. P. H. points out a mistake in our discussion of this matter. Lord Y. should have wagered 1,827 to 1, not 1828 to 1, the chance being 1-1828th, and the odds, therefore, 1,827 to 1. Of course "H. P. H." is right. The numbers representing the odds and the chance are so nearly the same in such a case as this, that we were not careful about a point which in others of our papers on chance we have insisted on clearly and often. In dealing with another point, "H. P. H." misconstrues us. He says, "the chance that a Yarborough will not happen in any deal is *not* the same as the chance that it will not happen in a given hand in four successive deals; for in the former case one hand depends on the other to a certain degree, whereas in the latter, the chance of any combination happening is quite independent of any combination which may have preceded it. I agree that the chance of a Yarborough is $\frac{1}{1828}$, and consequently the chance of a Yarborough in four consecutive deals is $\frac{1}{1828^4}$. Following your principle, this would be the chance of four Yarboroughs in one deal, which is a manifest absurdity, for we ascribe" (thus) "a mathematical chance to a clear impossibility." The question we were really considering was what odds should be offered to each member of a party of four at whist that his hand would not be a Yarborough; and we (practically) affirmed that £1,827 to £1 should be offered to each. "H. P. H." seems to consider that this is the same as offering £457 to £1 (roughly) against the occurrence of a Yarborough in a single deal. But this is not the case. Take a simple case illustrating at once his difficulty and our position:—Suppose there are four cards marked respectively A, B, C, D, to be dealt, one to each of four persons. Then the chance that any particular card, as A, will be dealt to any given person of the party of four, is obviously one-fourth, or the odds 3 to 1 against that event, so that with that person any one might at once safely and honestly wager £3 to £1 against his getting that card. Now our position is that the same odds might be offered with each one of the four, although it is certain, in this case, that some one of the four must have card A. (In the Yarborough case it is not certain but more probable that one of the four will have a Yarborough than that any particular one will have such a hand.) Well, "H. P. H." might reason that this is not the case, for if the chance is $\frac{1}{4}$ that a particular person will have card A in any given deal it is $(\frac{1}{4})^4$ that he will have it in four successive deals, and on our principle the same is the chance that each one of the four persons will have the card A in a single deal, or, in other words, the odds are only 255 to 1 against the manifest impossibility that each member of the party of four shall have the same card dealt to him out of four. Yet it is perfectly clear that the just odds are 3 to 1 with each person of the four, and the proof is that if these odds are wagered with each the event can bring neither gain nor loss to the layer of the odds; he will have to pay £3 to one of the four, and receive £1 from each of the others. So, if a person wagered £1,827 to £1 with each of four persons, before an ordinary whist deal, that that person would not get a Yarborough, he would be laying the just odds. Now let us see what his wager really amounts to in this case. If he loses to one, he loses £1,827. One of the others might have a Yarborough, but the chance that this would happen is very small: it is really this, that out of thirty-nine cards dealt to three persons, one would only receive cards belonging to a particular group of nineteen—a chance very small indeed. Regarding it for the moment as zero, we may say that it is certain, or all but certain, that from each of the remaining three players the layer of the odds will receive £1. Therefore, the layer of the odds pays £1,827 and receives £3, or loses only £1,824. His case is, therefore, similar to that of one who had laid against a Yarborough occurring in each of four successive deals to one only of the four players, except that this one might have had to pay £1,827 for each of the four deals, whereas the other could only have to pay for two at the outside, and would most probably have had to pay for one only. The difference exactly makes up for the interdependence of the four hands in any given deal.

Take a simpler illustrative case to show what we mean. A person, P, wagers with another, X, one of four to whom four cards, A, B, C, D, are dealt four times running, that X will not receive a particular card A, offering £3 to £1 at each of four deals. Unquestionably each wager is fair. X may have A each time, in which case P will have to pay £12, or X may not draw A at all, in which case P will receive £4. There are other eventualities easily followed. But the wager is manifestly fair. Now take a single deal. P wagers with W, X, Y, Z severally £3 to £1 that they will not have card A. In this case, one of the four must have the card, and to him P must pay £3, receiving from each of the others £1, or neither losing nor gaining. Since each wager, or rather each set of wagers, is manifestly fair, we see that the possibility in such cases of having to pay the odds more than once when successive deals are considered, exactly counterbalances the certainty of winning in some cases (most probably in three when a Yarborough is in question, and certainly in three where four cards only are in question), in the case of wagers with the four parties to a single deal. We have, in fact, only to ask whether a certain wager with one party to the deal is fair or not. If it is fair, we may be well assured that there is no unfairness (either way) if the same wager is made with each of the four players.

However, although this, and this only, was what we were considering (as should be obvious from our remark beginning, "Supposing Lord Y. offered £1,000 to £1 to each member of a whist party for ten deals," &c.), "H. P. H." very naturally misunderstood us, seeing that we wrote carelessly, as if we were considering "the chance that a Yarborough will not occur in any given hand" (these are our very words, and naturally misled him). The chance of this is not to be inferred so simply, as our words might have suggested, from the true odds against the occurrence of a Yarborough in a single hand. To return to our simpler case. The odds against a card A being drawn by one of the four is $\frac{1}{4}$, and the true chance of its being dealt *once* to a given person in four successive deals is $(\frac{3}{4})^4$ or $\frac{81}{256}$; so that the odds in favour of its being dealt to him once in four deals are 175 to 81. Thus only £81 can be safely wagered against £175 that the card will not be dealt once, at least, in four trials to one of the four players; but the chance of the card being dealt to one of four persons in a single deal is, of course, certainty, or 1; so that no sum, however small, can be wagered against any sum, however large, that the card will not be so dealt.

Answers to Correspondents.

** * All communications for the Editor requiring early attention should reach the Office on or before the Saturday preceding the current issue of* Knowledge, *the increasing circulation of which compels us to go to press early in the week.*

Hints to Correspondents. *1. No questions asking for scientific information can be answered through the post. 2. Letters sent to the Editor for correspondents cannot be forwarded; nor can the names or addresses of correspondents be given in answer to private inquiries. 3. No queries or replies savouring of the nature of advertisements can be inserted. 4. Letters, queries, and replies are inserted, unless contrary to Rule 3, free of charge. 5. Correspondents should write on one side only of the paper, and put drawings on a separate leaf. 6. Each letter, query, or reply should have a title, and in replying to letters or queries, reference should be made to the number of letter or query, the page on which it appears, and its title.*

A "Paradox-Monger." Very likely you are right; with the kindest wishes and the best intentions men make enemies. It will be unfortunate if our evasion of any particular subject should be attributed to "prejudice"; but we *must* omit some things, and amongst those your letter.—S. H. Wright. You seem to me entirely to misunderstand Dr. Wilson's first letter—it was explanatory, not controversial. The spider's web is not a greater difficulty than the cell-making instinct in bees; the study of Darwin's reasoning on the latter subject would show you that there is no real objection in the arguments you consider valid against the Darwinian hypothesis. If you remember that of five letters received four *must* be omitted, you will see that you have no real reason to complain that your former letter was not inserted. It would seem that we are bound to offend four-fifths of our correspondents if we keep open a correspondence section at all.—Robert Kidd. The point has been corrected already.—Pascal. You are, of course, right; but is it worth while to reason back so carefully to the original equation?—G. B. Stubbs. Thanks. Our Chess Editor and "Mephisto" will analyse it.—J. Jeffares. In his reply to Archimedean, p. 61, "Mathematicus" took it for granted that the former would at once see what you have established, and what H. A. N. says the differential calculus is required to establish. As you say, the substance of the proof is to be found in books; we *must* not find space for it. F. S. Pilkington. The experiment proving that one part of the retina is insensible to visual impressions is well known. Must reserve space for newer matter.—Jas. H Symington. Letters relating to Vivisection will receive due attention, but the matter is hardly worth discussion; it seems so clear that those who hold extreme views on either side are mistaken. The editor of the paper you mention is well known to be an extreme opponent of vivisection, one who would not allow a tom-cat to have a hair plucked out if the lives of all his relatives depended on the experiment. On the other hand, there are some who would let a thousand dogs be tortured to death to establish a physiological law of no benefit to a living creature. Fanatics of the first sort consider all who approve of any experiments whatever on animals to be brutes, while those of the second sort consider all who oppose the wantonest cruelty, to be drivelling idiots. No amount of discussion will reconcile these, or either sort, with the moderate persons who hold that researches really directed to benefit the human race may be conducted at the expense of pain to animals, provided that every means be adopted to reduce such pain to a minimum. Your story hardly establishes the intelligence of that particular collie. What did he gain by taking the chop under the sofa, and returning it uneaten? Are we to assume that he was planning how to get the bone out without injuring the chop? If he had done this it would have shown intelligence; as it was he only showed the effect of craving.—R. S. Couch. You will find a reply to some of your questions in "Found Links," presently to appear in Knowledge.—H. D. General information on the subject of your letter will be given in numbers beginning December 30, or January 6, at latest, in a series of papers by a Fellow of the Royal Astronomical Society, on work with a 3-inch telescope. Most of the objects he will consider will be perfectly well suited to your telescope, though it is of only $2\frac{1}{4}$ inch aperture.—F. J. Brodie. We cannot at present make any arrangement: probably we may follow a plan which we pursued some eight years ago, when editing the natural science department of the *Mechanic* (not the *English Mechanic*, but the older paper, now merged in *Iron*).—John Bennett. Thanks. You will see that the "Witch of Agnesi" has been fully dealt with; the geometrical instructions being better suited to these pages than the analytical methods, though not so easily planned. The dotted curves in your figure, I see, correspond with the equation when $x-2a$ is written for $2a-x$.—A Constant Reader and Subscriber. It is not the case that the moon never sets in the polar regions and is never seen in the crescent form. The moon is only well seen as a full moon in the arctic winter months, and when so seen does not set. The crescent moon can be seen in the day time in the arctic summer months, not at all in the winter months.—J. A. R. I fancy I have seen your views respecting comets before. Was it not in the *English Mechanic?* They seemed to me unsound then, and I must confess I find no reason now to view them more favourably. They are inconsistent with many known facts.—Ron. Godfrey. Thanks. Have you heard the family anecdote about R. C.'s eleven reasons for not acknowledging the work?—Cogito. The trisection described by Mr. Warren may be extended to any angle by first bisecting, then trisecting the half angle. It is impossible by Euclidian geometry to draw from C a line CED such that DE shall be equal to OE.—J. Bacon. You are quite right. Sir D. Brewster wrote when the true nature of the spectrum was not understood.—J. M'Grigor Allan. Many thanks. Your interesting discussion of Section 3 shall presently appear.—C. T. B. Hardly room for Gems at present. G. M. The method of trisecting an angle described in Mr. Warren's letter will not do for angles above 120°, simply because when the angle 120° is reached, the point D comes up to the circumference of the semicircle.—A. J. You do not seem aware of the fact that the precession of the equinoxes is not explained empirically by the modern theory, but is shown to be an inevitable consequence, in measure and degree, of the action of gravity. It is one of the most beautiful of the proofs of the Newtonian theory. Your theory would be all right, if, first, it were certain that there would be no such effects as the law of gravity assures us there must be; and, secondly, if we knew that the sun were circling in a mighty orbit, such as you picture, in 25,900 years or thereabouts. As we are quite certain the sun is not so moving, and equally certain that the earth being spheroidal must reel precessionally precisely as she does, your theory cannot be satisfactorily maintained.—J. T. E. Mr. Foster has your illusion in his collection. It was published thirteen years ago in the *Intellectual Observer*. Mr. Foster complains that the increase of correspondence is destroying all his illusions.—K. Symons. Mere extravagance.—Edward Cox. You are quite right; but proving the earth not flat is killing the dead. The argument you use has been employed often—by myself among others,—but the Flat men cannot or will not see what to persons of average brain power is so obvious.—Thomas Bouttell. You misunderstand Dr. Wilson's reply to "Ignoramus," whose "frankness," you say, "brought down the wrath of his opponent." There is no trace of warmth, far less of wrath, in Dr. Wilson's letter. The tone of yours is, therefore (you will admit, I think, on consideration), unsuited to the occasion. Your letter is also not sufficiently condensed. If you had read Dr. Wilson's excellent essays on evolution, you would be aware that what you desire to "wisper in his tympanum," would be very far from being news to him. The evidence you adduce to "vanish the phantom touching those rudimentary members which puzzles so many of us," is precisely the evidence which the evolutionist quotes in support of his position. In any case the lecturer on Zoology and Comparative Anatomy in the Edinburgh Medical School, does not need to have such well-worn knowledge whispered in his ear.—T. Smithurst. We hope to give, before long, such articles on Time as you desire.—Cogito, C. E., Algol, &c. Thanks; questions already answered.—W. H. Jacob. The explanation is simple. Your right eye being very short-sighted, takes no part ordinarily in the vision of objects beyond its focal range. When you close it you see such objects (your finger and the distant light, for instance, in the experiment you describe) just as you did when it was open. But when you close the left eye the right eye is at once called into action. You see the distant object and your finger with this eye, though not well defined. Moreover, you see your finger to the left of the light, because the line from the right eye to the finger must obviously fall to the left of the line from the same eye to the more remote object (this object, the finger, and the left eye being in a straight line).—Thos. Hebden. Thanks. It shall appear early.—W. Ga. Rolfe. Your article on Seismology comes at an inconvenient time, when two papers on the subject are already in type.—H. Edmonds, B.Sc. We could hardly do so without apparently recommending that of which we have no personal knowledge.—Harris. Loomis's "Treatise on Practical Astronomy" is one of the best.—J. H. G. If there were any recognised connection between mild Novembers and the winters following we could ask your question, but there is not. J. Mitchell. Thanks; but no room. Clio's query already answered. Mariette, R. Lenormant, and Wilkinson quite out of date, Bunsen nearly so. Your question about first infant, and how it fed, reads like a joke.—Vega. Question 65 now sufficiently answered.—Excelsior. We were referring to another. Have no knowledge of the instrument you mention. Why not write to makers, asking what guarantees they can give as to performance, &c.—W. C. Blythe. Thanks; your interesting letter shall appear. Prof. Lunge. Thanks. Chas. Moulton. Your idea submitted to publishers; but it seem so easy for readers who prefer it to put the stitch through the paper for themselves.—D. K. Your idea that the sun-spots may be intra-mercurial planets was the first which Galileo, Fabricius, and Scheiner entertained (and disposed of) when the spots were discovered 271 years ago.—F. W. Dillon. The cat who put her paw on your knee when you were eating, so that she might receive a share of your attention and of your supper, showed intelligence, no doubt; but the case is not so unusual as you seem to think.—Ernest C. A. Axon. Thanks.—Thomas Newbitt. Thanks. Query now sufficiently answered.—T. R. Regret that we have no space at present for your sound discussion of the moon's rotation.—Cupidus Scientiæ. Puzzle marked for insertion early.—A. J. Martin. The eyepiece of a telescope is a microscope, by which the image formed at the focus of the object-glass is magnified.—Tentative. We cannot admit the ordinary stories of spiritualism, but anything tested by due experiments, properly conducted and vouched for, would, of course, be in our line.—Memb. Soc. Bibl. Arch. Many thanks for your interesting letter. We slightly alter one or two passages, lest they should lead to rejoinders of an undesirable kind.—Cæsar. The letters S. P. Q. R. were used on Roman banners, &c. They stood for Senatus Populusque Romanus (the Senate and People of Rome).—Wm. Young. Your suggestion is a good one, but at present something more than the name seems wanted.—A. A. Young. Questions sufficiently answered. D'Artagnan. "Engraver" wants more special instructions.—J. A. R. Letter on rainbow presently, if can make space, but the proof belongs to text-book matter.—J. F. Simpson. Many thanks. Correspondence occupies so much time and space that other matters, which I regard as more important, stand a chance of being neglected, unless a very strict hand is kept on our correspondence column.—Alpha. Certainly, "would" and "could," for "will" and "can," are suggestive of Mrs. Brown.—H. E. Parker. Chemical question answered sufficiently.—E. F. B. Harston. It is easier to explain than to name suitable book. You only get the force retardative by carrying the body outside the circle. So long as it moves in the circle, the force always act at right angles to the direction in which the body moves, and therefore neither hastens nor retards it.—A Haviland. Will see about it. —C. J. Shaw. Thanks. Queries already answered.—Sigma. The illusion not striking in the picture, which would cost a good deal to engrave.—Champion. The advertisement may or may not involve a reference to Knowledge. At any rate, we claim no rivalry in one of the subjects named. The last thing we should think of would be to assert any claim as to the other. We must let readers decide.—Orion. We cannot see that there is any difficulty, even now; later, the numbers will be widely different. Enough has been said about grammatical question, we cannot but think.—Sonnet. Our heart is hardening rapidly.—W. Arthur. How many have other knowledge than from books on the subject named; and if those who have are inarticulate, what good would they do our readers.—An Old Boy. We quite agree with you; Cassell's *Boy's Newspaper*, and S. Low & Co.'s *Union Jack* are capital.—F. C. S. Your proof shows how an angle of 135° may be trisected; but considering that such an angle is equal to the half of three right angles, or to three half right angles, this can hardly be regarded as the solution of the general problem, which is what is wanted. The other angle trisected in your proof is the complement of $22\frac{1}{2}$ degrees, or $67\frac{1}{2}$ degrees, the third of which is one-fourth of a right angle. Knowledge should reach Liverpool earlier than 1.30 p.m. on Saturday. —A. A. McMaster. You are certainly right. The length of the rope makes no difference if the action on the weight is along the rope. Great width between the axletrees would make a difference, because part of the strain on each side is parallel to the axle, and, of course, wasted.—F. J. D. Selbst. Will keep your remarks about water spheroids for further consideration.—M. D. P. Will try to find room for your question about worms. It seems clear your explanation is right. W. A. R. Should have put in note, but hoped to publish it ere this.—E. O. J. Thanks. Would insert your answer to Theta's paradox, had we space. A paradox is usually regarded as opposed to what is orthodox.—M. J. H. Impossible to answer your query without breaking through rules.—W. A. C. See reply above to J. H. Symington.—H. A. Bulley. Will repeat later on your lunar views. W. G. Rolfe. Alas! no room for reply. It is not a question of "may" and "might," but of "must not" and "cannot."—Philarist. You shall keep your "knowledge in a box," we keeping our five shillings "with no further trouble." Seriously, that is not the way we manage.—S. S. S. Do not know Cyples' book. We might reply to all queries if we knew how. But knowledge is very wide.—Freshman. The reverend doctor of medicine said much more than he could prove. A man who would say that "not a tittle of evidence could be found for evolution," would say anything.—J. P. F. You rather misunderstand the way in which, according to the theory of natural selection, leopards have become spotted. The case more nearly resembles modern breeding than the Jacobian system you refer to.

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CONTENTS OF No. 9.

CULTIVATED FIELDS.

BY E. W. PREVOST, PH.D.

IN a former number (No. V.) of KNOWLEDGE, the formation of the soil of a field was described as succinctly as possible, and it was then shown how a certain class of rock was caused to yield a loam fit for the growth of plants. It is now proposed to continue the subject, and to point out how, by the present methods of cultivation, soil can be brought into a higher condition of fertility than it would otherwise possess, if left only to the action of the weather.

It is generally found that there exists below the surface soil a layer of earth of a character somewhat different to that of the upper layer. This has been termed the *subsoil*, and we shall see later on that its composition may exert a considerable influence on the fertility of the surface soil. The methods of cultivation which we propose to consider (as also the reasons why they are employed), are draining, ploughing, and manuring. If water be poured upon some clay soil, placed in a suitable vessel, it will be noticed that some of it will pass through the soil, and that the rest will be retained; but, depending on the class of soil under examination, the amount of water retained will be greater or less, a clay soil retaining much, a sandy soil but little; this property of holding back some of the water which falls upon soil has been termed the retentive power, and it exerts a very great influence on the luxuriance of crops, for when a soil remains wet, its temperature cannot rise as high as if it were dry, and is consequently "cold." Such a "cold" land militates against the germination of seeds. On the other hand, if the retentive power is low, then the land dries quickly after a fall of rain, and the plants run the risk of being scorched by the heat of the sun. Now, it may occur that a soil is not naturally retentive of water to any great extent, yet an excess of water may be observed incapable of passing away from the surface; the cause of this stagnation may be found, on examination of the under-lying portion of the land, to arise from a bed of clay that will not admit of the downward passage of the water. This or another arrangement of the deeper portions of a field has given rise to the modern system of draining, which by causing the removal of the excess of water, and its more even distribution through the soil, permits of the entrance of air into its pores, whereby decomposition of the insoluble minerals is occasioned, and of a higher temperature being attained. Nor are these the sole advantages, but an excess of soluble saline matter, injurious to plants, such as stagnant water will deposit, is obviated, and the roots of plants are enabled to seek for their food at a greater depth than they otherwise could. Apart from these considerations affecting the well-being of the crops, there is also the great gain to the cultivator, in that the labour of tilling a soil sodden with water is far greater than when the land is fairly dry. The various methods in use for breaking up the surface of a field that pass under the name of tillage operations produce effects both physical and chemical: for by ploughing, the condition of the soil is improved, the soil being rendered less compact, dried from superfluous moisture, and exposed to the decomposing action of the air, whereby as before stated, solution of the minerals is facilitated. This decomposition has, however, been in progress but more slowly all the year round, and some of its products have been carried deep down by the rain; these the plough brings to the surface, thus adding to the stores requisite for the future crop. Sometimes, when the ploughing is carried too deep, the results are far from satisfactory, the land becoming for a short time less fertile than previously. When such an event as this happens, we may be nearly certain that the sub-oxide of iron has been brought to the surface, where it must remain until it becomes peroxide by exposure to the air, and after that the land may resume its original condition of fertility. Hence it is desirable on all accounts to allow a newly-ploughed field to remain untouched for some time, and the more so if the ploughing has been deep. It appears, then, that ploughing is in some measure supplementary to draining, and *vice versâ*, as without draining, a heavy wet soil can only be imperfectly ploughed; or, rather, the advantages following on ploughing are but in part gained.

By the removal of a heavy crop off a field, a considerable quantity of mineral matter is lost to the soil, which cannot be replaced under natural conditions in amount sufficient to produce a heavy crop in the succeeding season; to get rid of this difficulty it is customary to apply manure to the exhausted field, manures being substances which either of themselves supply food, or else by their action on the soil cause it to yield an increased quantity of available plant-food—at the same time improving its character. It would occupy far too much space to describe the properties, &c., of all the manures in use, so that we will confine our attention to two which are well known to the general public, namely, lime, and farm-yard manure. In the first of these two we have an example of a substance used not so much as a food—for, as a rule, all soils contain a sufficiency of lime for the plants—but as an agent whereby the stores in the soil are rendered accessible; but before being able to appreciate wholly the virtues of lime, we must call to mind what are the chief substances which are of value, and which are likely to be affected by the presence of such a manure; these substances are potash in combination with silica and alumina, and nitrogen contained in the organic matter or humus.

Lime may be applied as caustic or quick-lime, and as slaked lime (quick-lime to which water has been added), but as regards the action of either of these forms, there is no absolute difference, but only one of degree, in that quick lime is more energetic in its action than slaked lime; but to counterbalance the deficiency in energy of the latter, it is more capable of even distribution through the soil, by reason of its fine state of division. Rank and luxuriant herbage

is indicative of "sourness" of the soil. By this expression is to be understood the presence of an excess of acid humic matters; these acid matters on the application of lime are neutralised, and the nitrogen, held in combination in a state difficult of solution, is converted into ammonia, and finally into nitric acid which, combining with the lime, forms a most valuable manure, and one easily absorbed by the plants. The felspathic particles are likewise slowly acted on, the lime taking the place of the potash, which then becomes soluble in water, and fit for absorption by the plants. These, then, are the changes produced by lime; but there is considerable danger incurred if they are allowed to take place too often by repeated applications of the manure, for the organic matter will be reduced too much, as also the potash minerals, which in a poor soil may not be too abundantly present. But lime produces other as well as chemical changes, in that it lightens a heavy soil, and consolidates a light one, merely because it is a substance whose texture is opposed to a clay or to a sand. The lightening of heavy soil is, however, not to be ascribed wholly to the physical admixture of lime, but also to a somewhat complicated chemical change which occurs in the soil. To explain what actually does take place, it is necessary to know that when lime is exposed to the air, it gradually absorbs carbonic acid, and is transformed into carbonate of lime, or chalk; when, then, the acid humic matters of the soil come in contact with the chalk, carbonic acid is set free, but not being able easily to escape, it remains enclosed in the pores of the soil, causing the soil to swell and to become lighter, and, if the evolution of gas be not too great, much benefit is the result; but if, on the other hand, this porous condition is excessive, a loss of fertility ensues, in which case the land is said to be "over-limed." As a proof that this is one of the causes of "over-liming," it may be stated that rolling the land is found to be highly beneficial, as thereby the soil is rendered more compact, a portion of the imprisoned gas being forced out. To produce this porosity, it appears that we are not wholly dependent on lime, as the addition of carbonate of lime tends to produce the same result, and without such violence of action as is exerted by caustic lime.

Considering, next, farmyard manure, we see how different is its working, for by its application, actual food is given to the plant, though not in the form best suited for assimilation; hence the manure is slow in action, and produces its effects over a period of three years, as its constituents are but slowly brought into the soluble condition. Farmyard manure may be considered as consisting of nitrogenous matters, capable of conversion into ammonia, potash in a soluble state, and some phosphoric acid; and besides these compounds, there is a certain amount of organic or vegetable matter, such as straw, partly transformed into humus, and partly in its original condition. When this manure is applied to the fields, the soil retains all the valuable portions as they become soluble, handing them over to the plants seeking for food; but, at the same time, it is improved in character, for, if heavy, it is lightened. It is found that it is principally the straw which renders a heavy soil lighter; for, as it slowly decays, giving off carbonic acid gas, it leaves, as it were, its cast in the soil, thus forming passages which the air can readily traverse. In this decomposition of the straw, we have one of the sources of the carbonic acid which acts upon the lime, as referred to above, and which also aids in the destruction of the minerals, as previously stated in No. V.

According to an analysis of Professor Frankland, the water of the Holy Well of Zemzem, at Mecca, is sewage more than seven times as rich as the average sewage of London.

BABYLONIAN SUN-WORSHIP.

Nabupaliddina was the contemporary of the Assyrian kings, Assur-nazirpal and Shalmanesar, and the sculptured, inscribed memorial of his reign, which has been restored to us, is certainly one of the most important records that have rewarded the explorer in Babylonia. In the upper part of the tablet, the dimensions of which are 2 ft. long by 1 ft. broad, is a small sculptured panel representing the worship of the Sun-god by Nabupaliddina and attendant priests. The god is represented as seated on a throne beneath a baldacchino, or open canopy shrine. He has long beard and hair, like most conceptions of the Sun-god, and holds in his hand a ring, the emblem of revolving time, and a short stick; too small for a sceptre, we may, perhaps, see in this the fire-stick which was closely connected with the Sun-god. Before him, on a small table-altar, is a large disc, ornamented with four star-like limbs and four sets of wave-like rays. Above this group is cut the inscription: "The Disc of the Sun-god, and the rays (of his) eyes." The scene here depicted is clearly indicative of the fact that the priests of Sippara were worshippers of the solar disc and solar rays, and their creed seems to bear a close resemblance to that of the disc-worshippers of the eighteenth Egyptian dynasty, who, under Amenophis III. and his son Khunaten, for some time held their ground against the priests of Ammon. This heretical creed was introduced into Egypt by Thi, the Asiatic wife of Amenophis III., and its tenets have been made the subject of a special memoir by Sir Charles Nicholson. The discovery of this sculpture and inscription from the ruins of the temple of the Sun-god at Sippara tends very strongly to place the origin of the creed in Babylonia. The inscription on the back and front of this memorial tablet is a valuable record of the religious life and ceremonial of the Babylonian temples, and the list of the solar festivals in the fifth and sixth columns shows how far back into the remote past we must place the rise of Babylonian sun-worship. Astronomers will welcome this ancient list of festivals, as it proves very clearly the high character of the astronomical knowledge of the Babylonian priests. The six fixed festivals recorded in this inscription are:—

1. Nisan, seventh day, Festival of the Rays (vernal equinox).
2. Airu, tenth day, Festival of the Rays.
3. Ulul, third day, Festival of the Illumination of the Temple.
4. Tasrituv, seventh day, Festival of the Illumination of the Palace (autumnal equinox).
5. Marchesvan, fourteenth day, Festival of the Rays.
6. Adar, fifteenth day, Festival of Illumination of Palace.

The discovery of an important list of solar festivals such as we have here is an important addition to our knowledge of Babylonian astro-theology. The first month of the Babylonian calendar was the "month of sacrifice," or "the altar," and its position was fixed by the vernal equinox, which was in the time of Nabupaliddina, in the sign of Aries—the ram being the chief object of sacrifice. Students of Biblical archæology will find an abundant fund of matter in the carefully compiled rules as to the distribution of the sacrificial victims. "Sheep," "oxen," "rams," "fruits of the earth," were objects of sacrifice, and portions of each offering were set aside for the priests. These offerings were burned on the great altar of the temple discovered by Mr. Rassam in the chamber adjoining the record room. The great central court of this temple seems to have been styled "the court of the Sun-god," and there was also an outer court "called the court of Bel."—*Times.*

BRAIN TROUBLES.

Part III.—Distracted Attention.

THE next of these signs—one, indeed, which many mental physiologists set first—is an inability to fix the attention on any subject till the mind has done with it. We have taken the failure of memory first, simply because we believe that this symptom can ordinarily be recognised earlier than inability to fix the attention. The fact would seem to be that, since in ordinary processes of thought, we first recognise or ascertain particular facts, and then commit them to the keeping of the memory, the latter process is naturally the one which first fails us. That it should be taken first is indicated, too, by the circumstance that although many cases can be cited of persons who, although able to direct their attention to a subject, are unable to retain in their memory what has been suggested to their thoughts while thus directed, no case is on record in which this state of things has been reversed, and a person has been able to remember recent facts distinctly after he has lost the power of arriving at fresh ideas by efforts of attention. To mention only one case of the former kind, Dr. Winslow tells of one patient whose memory as to recent events was seriously damaged, while yet his perceptive faculties and reasoning powers were not at all affected. "He conversed with great sagacity, fluency, and acuteness on every subject, but if I permitted a second to elapse in the conversation, he entirely forgot what he had been previously talking of." From the time when his memory thus failed him, he retained his former power of reasoning. "He could discuss at short intervals the most subtle and abstruse political, professional, and literary matters with apparently unimpaired mental vigour; but his memory never recovered its healthy tenacity." It may hence be inferred that temporary loss of the power of fixing the attention (which must be carefully distinguished from mere forgetfulness, that is the habit of being inattentive), is more likely to be a sign of serious mental mischief, than failure of the power of memory. Yet the former, like the latter symptom, indicates in the great majority of cases no serious mischief, though it would be exceedingly unwise to overlook it.

The failure of the power of directing the attention to a subject may show itself in several ways. Thus the mind may be unable even to begin the study of a subject; or it may begin the study and presently wander off to other subjects, despite the most anxious efforts to restrain it from so doing; or suddenly it will seem to cease its action, remaining for a short time confused and, as it were, lost, and then resuming the consideration of the same subject at the point where it had left it, and apparently as acutely and attentively as before. These three forms of distraction are of different significance as symptoms of mental trouble. The first, though undoubtedly it would be very serious in this respect, if persistent, nearly always indicates only that the mind wants rest, and no one who is wise will neglect the warning. The second equally implies that the mind wants rest, though not in equal degree. But the third is usually a sign of serious mischief. We consider it here, not as belonging to those indications of mental disturbance which, without being alarming, should be attended to by all who wish to keep their brains in good working order, but because the nature of the cerebral mischief indicated by such symptoms has been ascertained, and we may hence infer the general nature of the mischief indicated when the less serious symptoms of distraction are recognised, and may so judge what is the appropriate remedy. For, unfortunately, several of the cases in which the mind has been observed suddenly to become confused or lost, resuming its activity and clearness after a short interval, have been followed by severe illness, which has proved eventually fatal.

Among the most remarkable and carefully-observed cases of this kind is that of King Oscar of Sweden, the circumstances of which were minutely detailed by Dr. P. O. Liljewalch, the king's first physician. King Oscar had enjoyed fairly good health during the greater part of his life; but before his last illness it had been noticed that occasionally the heart's action was irregular, oftener in spring than in other parts of the year. In 1851 the heart became very irregular in its movements, and the digestive functions were impaired. Although he soon after recovered to some degree, an attack of typhus fever, following the loss of a beloved son, severely tried his constitution, when, slowly recovering strength after this, he unwisely omitted his usual autumn rest and excursion, and devoted his mind to political matters requiring close and anxious attention. In 1857 his health again gave way, and it was at this time that the nervous mischief was first noticed which subsequently proved the characteristic feature of the king's illness, and, in Dr. Liljewalch's opinion, "brought him to his death." The first trace of this nervous disease, says Liljewalch, "manifested itself long since, although it was not until the last six or eight years of his life that, as we have seen, it occurred with more definite and at last with such threatening symptoms. No one who had an opportunity of observing him during a long period in his daily intercourse, could avoid being amazed at the very extraordinary power the king always exhibited of retaining in his memory the most varied details, or could cease admiring" (really one could imagine that some few could achieve this, however impossible it might seem to the courtly Liljewalch) "the rapid apprehension, the unerring judgment, and the singular clearness of statement which were exhibited whenever he spoke. But at the same time he could not fail to recollect how the king sometimes, in the middle of a conversation to which he was directing all his attention, would of a sudden appear to be abstracted, and would actually transfer his thoughts to some other subject on which unless he might be disturbed, he would allow them to rest, usually only for a few moments, but sometimes for many minutes, after which the conversation would be resumed as if it had not been interrupted. The peculiar expression of the king's features—particularly his look, assumed on such occasions, and the spasmodic state, or the involuntary movements which at the same time took place in one or other part of his muscular system—render it probable that this distraction, which at times was of frequent recurrence, was due to an incipient affection of the central organ of thought. This symptom, referable to the most important organ of the nervous system" (the care and ingenuity with which the court physician avoids any direct statement that the king's brain was affected is worthy of all praise) "was of late years accompanied, as has been already mentioned, with increasing weakness in the muscles of the lower extremities, and with uncertainty in the combination of movement, probably depending on a commencing organic change, either in the organ alone on which the power of motion depends or else in that by which the co-ordination of movements is effected." The king himself was not misled by the phraseology in which the court physicians endeavoured to cloak the fact that his brain was disordered. "Incapacity to discharge his royal functions now brought on a deep melancholy, and the king, even in the commencement of his illness, expressed his conviction of its incura-

bility." The strength of the body failed more and more as "the organ on which the power of motion depends" became more and more diseased. "The lower extremities, the muscles of which were always weak, began to totter under the weight of the body, and at the same time the power of combination for the motions of those parts was impaired, and the king was troubled with vertigo, particularly accompanying the movements of the head, and with vomiting, which symptoms, in combination with diminution of strength and the occurrence of involuntary muscular spasms, indicated the existence of a more deeply-seated affection, probably a softening in the central nervous system." (One could imagine that as, of old, Spanish courtiers adopted the conventional hypothesis that a Queen of Spain has no legs, Dr. Liljewalch held that the Kings of Sweden, and "royal personages" generally, have no brains). The means employed to combat the disease produced no good effects; "the paralysis, which commenced in the lower extremities, gradually increased, and after the king, feeling his inability any longer to fill the high position to which Providence had called him, transferred into the hands of the crown prince the government of the United Kingdoms, his deep melancholy gave way to a progressive indifference, even for those things which in his health he had regarded with the most lively interest." The rest of Dr. Liljewalch's account relates to the gradual failure of King Oscar's powers, mental and bodily, and is too technical to be quoted *verbatim*. It is noteworthy that the power of speech began to be affected early during the progress of the disorder, and later was lost altogether. From this we should be led to regard failure in the power of verbal expression as a later, and therefore a more alarming, symptom of cerebral mischief, than diminution of the power of fixing the attention. The *post-mortem* examination of King Oscar revealed extensive disorganisation of the brain.

A case somewhat similar to that of King Oscar is thus related by Dr. Forbes Winslow:—"A gentleman connected with the Stock Exchange was suspected to have disease of the brain. His symptoms were as follow: general muscular weakness, occasional paroxysms of severe headache, slight paralysis of the superior palpebræ and of the left eye, occasional sensation of numbness in the right foot. The mind was not apparently at all impaired. He continued, up to the period of my being consulted, fully competent to discharge all his commercial duties, attended to his accounts, and wrote letters of business with his usual ability and clearness. His brother informed me that at times he was greatly *ab*stracted and *dis*tracted; that whilst engaged in conversation, he would suddenly pause put his hand to his head, and appeared vexed with himself at having lost all consciousness of what he was saying. This symptom was observed *two* years before any question arose, or suspicion existed, as to the state of the brain! The family, judging from the subsequent progress of the case, were of opinion that the cerebral disorder was first exhibited by the sudden lapses of thought to which he was subject for many years previously to the manifestations of other and more unequivocal symptoms of brain disease. Such, also, was my opinion. . . . In about a year and a half he died, quite paralytic. Considerable organic disease of the brain was discovered after death."

In another case, which also ended fatally, an Irish barrister, three years before an attack of acute mania, was observed to stop occasionally whilst addressing the courts of law, as if for the moment lost. "So marked was this symptom, that a professional friend, often associated with him in the conduct of legal matters, considered it his duty to direct the attention of the gentleman's wife to the fact, considering that such attacks of mental distraction, on occasions when it was of essential importance for the mind to be in a state of continuous activity, looked suspicious, and, according to his judgment, were not consistent with a healthy state of the brain." About two years after this peculiarity had been noticed, this patient experienced a slight epileptiform seizure whilst at his chambers, during a very hot day in the month of July. "As this attack was considered to have been one of syncope, and to be caused by the then high state of the temperature, little or no notice was taken of it. Previously to travelling on the Continent, he had been working unusually hard, eating and drinking very sparingly, sitting up late at night, and rising early in the morning. In fact, he acted with great indiscretion and imprudence, and the result was an acute attack of brain disease, affecting the mind, a fortnight after his arrival in Paris." In this case, the *post-mortem* examination revealed the existence of chronic disease of the membranes of the brain—mischief which seemed to have lasted for a considerable time before death.

As we have already explained, it is not so easy to find illustrative cases of the less alarming forms of distraction. Even in cases where serious mischief has followed these slighter mind troubles, the symptoms immediately preceding such serious illness have commonly been of a more marked kind, and these alone have usually been regarded as really belonging to the case. Nevertheless, all who have given careful attention to mental maladies, can speak of instances in which the less serious forms of distraction have been noticed early in the progress of cerebral disorders; so that though they need not alarm those who note them in their own case, they should not be neglected. They are always signs that the mind wants rest, and they may be signs that some more specific remedy is required, which can be readily determined by noting whether rest brings relief. "I am anxious," says Dr. Forbes Winslow (and it could be wished that throughout his valuable work he had been similarly careful to avoid occasioning unnecessary alarm), "to attach no undue importance to this evidence of morbid intelligence, but I cannot close my eyes to the fact that a debilitated power of attention is a prominent symptom in the early stage of cerebral disorder. Cases of incipient brain disease have occurred in which patients have, previously to other symptoms, lost all ability to read continuously twenty lines of a book without a painful effort of thought." It will be noticed that Dr. Winslow here puts distraction as a phenomenon preceding, in cases of cerebral disorder, the loss of memory: albeit, we believe that had he had the means of ascertaining the precise progress of mental disorder, in cases where he supposed this to have been the case, he would have found that the memory had begun to go in the first instance. "If," he proceeds, "there be impairment of attention and debility of memory, it is illusory for the patient to imagine that he is able, until his *physical* condition of ill-health is attended to, by repeated and persevering efforts, to resuscitate these prostrated powers. In his attempt to do so he still further taxes the morbid state of these faculties" (meaning, apparently, that he overtaxes the faculties and makes their state still more morbid), "and, instead of invigorating, still further debilitates, and often entirely extinguishes his intelligence." This caution cannot be too carefully attended to. Returning to the analogy between bodily and mental powers, which we touched upon at the outset, we may compare the power of attention to actual muscular strength,—as the power of memory may be compared to skill in mastering such and such feats of muscular dexterity, and acquired mental knowledge to the various athletic exercises which a man has learned to

achieve. Now if an athlete finds that his bodily strength is unequal to a task which has hitherto been well within his powers, he would not think (if he were wise) of trying repeatedly to achieve the muscular effort which he has found too much for him. Or (extending the analogy to other ways in which the power of attention may fail) if an athlete finds that he is unable to continue some muscular effort so long as usual, he does not compel Nature to achieve the task which for the nonce has become too great for him. In either case he perceives that for the time being he is not himself, and, by rest or change of some kind (diet, mode of training, or the like), he seeks to restore his powers. At any rate, if he is so unwise, in either case, as to endeavour to master Nature, he increases the mischief, and may entirely lose the powers which had been weakened, and might otherwise have been soon restored, or might at least have been saved from further weakening. So, remembering how close in reality is the analogy between the mental and bodily powers, we can well believe Dr. Forbes Winslow, when he tells us that when the attempt to fix and concentrate the thoughts requires a continuous, painful, and vigorous effort of the will, "serious and irreparable injury may be done to the delicate organisation of the brain and mind by injudicious attempts to exercise, stimulate, and *force* into activity the morbidly flagging and sluggish mental faculties." These symptoms show that the brain is for the time being unfit for sustained action or for intense action, though not necessarily (or even probably) diseased, and that rest is essential to restore its enfeebled energies. Whether such rest should be long-continued or not, will depend on the question whether the symptoms of weakened powers of attention are marked or otherwise, and also in no slight degree on the length of time during which these symptoms, whatever they may be, have been neglected. If they are attended to so soon as they are noticed (in which case they will probably be slight), a very brief rest will generally restore to the mind its wonted energies. Many a man who, in the midst of prolonged and arduous mental exertion, has noticed signs of flagging in his power of attention, has found in even half-an-hour of sound sleep a remedy more effective than a three months' rest would be after such signs had been neglected during several successive weeks of mental labour.

Some physiologists assert that defective speech, the next symptom which we have to deal with, has been the first symptom noticed in cases of cerebral disorder. Dr. Forbes Winslow says :—"The first evidence of approaching apoplexy and paralysis is occasionally a sudden loss of speech." This may have been the first symptom noticed, but we question very much whether it has ever been the first symptom which has existed.

We ought to distinguish, perhaps, here, between defective speech and defective power of expression (by words indicated otherwise than by actual articulation). In fact, an important distinction exists even between the loss of the power of articulation and the affection of the vocal organs indicative of cerebral disease. Here, however, we consider generally the impairment of the power of linguistic expression which usually precedes serious mental trouble, and is often enough noticed where rest only or change of diet is necessary as a remedy. Usually, however, this symptom is serious. Indeed, one writer on the subject of cerebral disease remarks that it is a most unusual circumstance for the symptom to exist without being followed by acute cerebral mischief. Possibly the remark refers only to the absolute loss, whether for a short or long period, of the power of expressing ideas by language, spoken or written. That the power of expression may be affected, and even for a time affected seriously, while nevertheless there is no serious cerebral mischief, is within the experience of most persons who have occasion to exercise this power freely. The symptom, like others we have dealt with here, is one to be noticed, and its warning voice should be heeded early. This done, there is usually little occasion for alarm, startling though some of the stories now to be related may appear.

Dr. Winslow relates that "a literary gentleman, whose vocation in life was that of a public lecturer, noticed for nearly eight weeks before he was seized with paralysis, that occasionally whilst speaking he lost for a second or two all power of articulation. This occurred on five or six occasions previously to an attack of decided hemiplegia. This patient had taxed his powers of mind to their utmost by lecturing twice, and often thrice, a day*; but independently of this amount of literary labour, he had been exposed to much anxiety respecting family matters, and this had produced restless, and, in some instances, sleepless nights."

INTELLIGENCE IN ANIMALS.

LET us next examine a few cases in which animals have done things which they have seen done by the persons with whom they live, and more or less obviously with the object of obtaining the result which they had observed to follow from such actions. For this would seem, if the animal can be clearly shown to have had such a purpose, to be distinctly the result of reasoning. Monkeys may or may not reason when they imitate actions which, when performed by themselves, are of no advantage to them, or are even mischievous. Indeed, it is not improbable that they suppose their human fellow-creatures would not perform such actions except for a useful purpose, though what that purpose may be they may have no conception. But whatever opinion we may form on this point, we can have, it would seem, no room for rejecting the belief that an animal has reasoned who performs an act demonstrably for the purpose of producing a certain effect, such as he has observed to follow when human beings have so acted. Now in some of the cases which follow, this does seem to be most clearly made out.

A writer in *Nature* gives the following case :—"My sister, who lives just opposite to my own house, possesses a cat (now about thirteen years old), whose intelligence is very remarkable. He has a habit of making use of the knocker of a side door, which is just within his reach as he stands on his hind legs, whenever he desires admission. A single knock is tried in the first instance; but if this is not answered promptly, it is followed by what is known as the 'postman's knock;' if this is not successful, trial is then made of a scientific 'rat-tat' that would not disgrace a west-end footman. I should say that 'Minnie' holds the knocker in his paw as we should hold it in our fingers, and not by simply tipping it up. How far this practice involves 'abstract reasoning,' I will not say, but something like an approach to it is suggested, for he was never taught to knock at the door, and adopted the habit some three years ago, evidently to gain admittance, very often to the annoy-

* The writer of these lines has lectured twice a day for a week, filling up four or five hours of each day with literary work, without feeling any effects which seemed to suggest that he had taxed his powers of mind to their utmost. But, on the other hand, he has noticed that after lecturing only once a day, or even only four times a week, while travelling great distances each day, several of the symptoms of incipient cerebral mischief have appeared; and even less lecturing, accompanied by much mental anxiety, has caused such symptoms to appear.

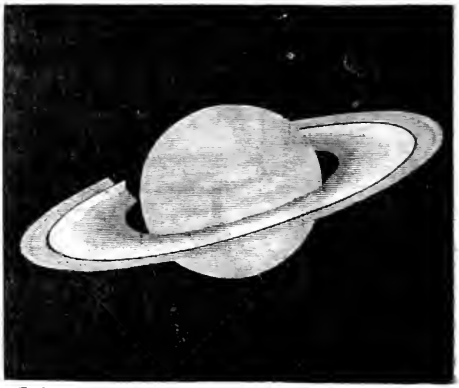
ance of my sister's family, who have occasionally been disturbed in this way at unseemly hours." The rest of the letter has no bearing on the subject we are upon, but it is too amusing to be omitted. "I should be sorry," says the writer, "in thus referring to the sagacity of poor pussy (who is now also somewhat feeble), to reflect upon him by noticing some other of his peculiarities, one of which is his fondness for a little brandy-and-water, and other alcoholic stimulants." It would be, perhaps, to inquire somewhat too curiously to ask whether this story shows that the fondness for stimulants is associated with an advance in reasoning power, or whether, perhaps, Minnie's brain was aroused to abnormal activity by the tippling in which alone (we may assume) he was indulged by his mistress. The point established by the story is that in some cases—at any rate, as in animals so low in cerebral development as cats—the consequences of a certain action are observed and remembered, the action being repeated by the animal when he wants those particular consequences to follow. This cannot be explained by any theory of mere instinct.

In the last story, the cat was an old one, and though this does not modify the conclusion to be deduced from the animal's behaviour, yet it in some degree diminishes our estimate of the activity of Minnie's reasoning power. In the following case, a young cat showed equal intelligence :—"I may mention a case," says the writer, "of a kitten about half-grown, having mental reflection of some sort. I was sitting in one of the rooms at a house where I was stopping in Somersetshire, and hearing a knock at the front door, was told not to heed it, as it was only this kitten asking admittance. Not believing it, I watched for myself, and very soon saw this kitten jump on to the door, hang on by one leg, and put the other fore paw right through the knocker and rap twice. The knocker was an ordinary shaped one, fixed in the centre of the door half way up; the top part of the door was glazed. I saw this performance dozens of times afterwards, and often used to put the kitten outside to see it done. It was never known to knock when anyone stood in the garden, but if one went indoors and shut it outside, in a few minutes came the usual knock. A sister kitten to this one was never known to knock, but sat on the doorstep and entered when the door was opened, and in nine cases out of ten the knock was successful. The kitten was never taught in any way: it would knock at both front and back door."

In the following case, the object of an animal's action in such cases was tested by an experiment, but the evidence is less satisfactory in one respect than that afforded by the two previous cases, the animal having been taught the action :—"A small English terrier belonging to a friend," says the narrator of the story, "has been taught to ring for the servant. To try if the dog knew *why* it rang the bell, he was told to do so while the girl was in the room. The little fellow looked up in the most intelligent manner at the person giving the order (his master or mistress, I forget which), then at the servant, and refused to obey, although the order was repeated more than once. The servant left the room, and a few minutes afterwards the dog rang the bell immediately on being told to do so." Here it is to be noticed that the dog did not ring the bell (as, in each of the preceding stories, the cat knocked at the door) to get some end of his own accomplished. He rang to save his master or mistress trouble. And the fact that he had been taught to ring for this purpose, although making the act itself less obviously a sign of reasoning power than the cat's action in knocking at the door, makes his refusal to ring when told to do so a more manifest evidence of reasoning than it would otherwise have been. If the dog rang for the servant because of some advantage he always gained from the servant's coming, it would have been natural enough that he should refrain from ringing when the servant was in the room. But his refusing to do what he had been taught to do, at the risk of offending his master or mistress by such refusal, makes it absolutely certain that he had clearly recognised the object which was to be attained by ringing the bell.

THE PLANET SATURN.

Although the ringed planet is passing away from the position where it shines most brightly, and being nearest to the earth, is, on the whole, most favourably situated for observation, yet, as he passes away from this position he shows certain features of interest which are either less favourably seen, or not seen at all when he is nearest to us. Just as the full moon shows no shadows, being seen from the earth when it is between the moon and the sun, so the shadow of the planet Saturn on his rings is less invisible when Saturn is in opposition (as it is called) to the sun, that is, when the earth lies on, or very near, a line drawn from the sun to Saturn. But when the earth has, by her more rapid motion, passed away from this position, the steadfast



shadow which, "as the planet whirls, sleeps on the luminous ring," is well seen. This shadow is an interesting object of telescopic study, because, instead of presenting at all times those uniform outlines which the laws of perspective teach us should be presented by the shadow of a spheroid on a plane, or nearly plane, surface, the outlines are often very cunningly distorted. Thus, in the accompanying picture, we have a view of the shadow as seen by Trouvelot, an excellent American observer, with the magnificent 26 in. telescope of the Washington Observatory, and this shadow is so distorted that one would say it was entirely "out of drawing" if one were not certain that Trouvelot depicted correctly what the perfectly trustworthy and most powerful telescope he employed, showed him. We would invite telescopists to examine the planet carefully during the next few weeks, and note any peculiarity of shape which the shadow on the ring may present. We shall be glad to indicate, for comparison with drawings which may be sent us, the true shape which, according to the laws of perspective, the shadow should have had at the epoch of the respective drawings.

THE MAGIC WHEEL.

WE had hoped to present our younger readers this week with a drawing of a trotting horse in the various positions successively assumed by the animal (as instantaneously photographed), for use with the ingenious instrument illustrated in the accompanying cut. But, on carefully examining the picture in the *Scientific American*, we found that there was an error which would have caused the picture to produce an imperfect illustration of the horse's action. Twelve positions had been taken from the photographer's series without its being noticed that the last two were almost exact repetitions of the first two (in other words, a complete double

step was illustrated in the first ten pictures). The delay caused by the corrections prevents us from giving the picture this week, but next week we shall have a set of ten positions of a trotting horse, arranged for use as in the accompanying figure, illustrating a highly ingenious method for avoiding the difficulties involved in the construction of a zoetrope. We shall later give a series of views of a galloping horse. In the meantime we leave our younger readers to puzzle out the meaning of the accompanying cut, and in particular to find out how it is that the various parts of a properly-constructed zoetrope are provided for here by so simple a construction.

PRIMARY COLOURS.

IT is impossible to construct a consistent theory based on three primary colours, whether the three be the older set—blue, yellow, and red, or violet, green, and red—the newer set of the theory considered by some to have been established by Clerk Maxwell in his paper to the Royal Society in 1860.

Clerk Maxwell's paper contains serious errors. He forms equations with different kinds of quantities, implicitly attributing to the units of those quantities values which, regarded relatively to each other, are purely accidental or arbitrary, and his results are, consequently, fallacious.

As is apparently recognised by Rood, in "Modern Chromatics,"—a correct and complete theory of colour vision ought to enable us to construct a circular diagram of colours, with complementary colours diametrically opposite to each other, and with the colours distributed with uniform gradation round the circle, and in accordance with their true relations to each other. Now, such a diagram cannot be constructed on the assumption that the primaries are three in number, without assuming certain colours, or combinations of colour, to be complementary, which are proved by actual experiment not to be so. The assumption also involves the absurdity that a colour can be (in a sense) complementary to itself; or in other words, that two diametrically opposite colours or combinations of colour which are complementary to each other may each contain the same colour as an ingredient.

When proceeding to arrange colours in a circular diagram, we have first to classify them. For this purpose I take so-called primaries and secondaries together, and for convenience call them simply distinct colours. To the blue-yellow-red theorists I say that to my eyes green and violet are as "distinct" as any of the three, but orange is not to the violet-green-red theorists. I say blue and yellow are as "distinct" as any of the three, but purple or crimson is not. How many "distinct" colours, then, are there to be assigned to equidistant points on the diagram? In my opinion the human mind cannot conceive of more than five colours which are as distinct from each other as red from yellow or from violet, or as blue from green or from violet, or as yellow from green. If, then, there are only five colours of the first class, or of the first and second classes together, it is impossible to construct a theory with three primaries, for such a theory implies three secondaries, or six colours of the first and second classes taken together.

With reference to seeing red in the violet of the solar spectrum, I may mention the case of a person who sees the violet of the spectrum as a dim grey only, and yet, when two spectra overlap, so that the red end of one combines with the blue part of the other, he apparently experiences the same sensations of violet and purple as normal-eyed persons.

It is held by Helmholtz and others, and I think there can be no doubt of the fact, that mentally we cannot really distinguish in any colour-sensation any components, but only a single resultant sensation. We may experience a sensation which may be called pure as regards its colouredness; for example, we may experience the sensation of a green, which inclines neither to yellow nor to blue, but it is quite certain that for a normal-eyed person it is impossible for light to act on the eye in a perfectly simple or pure manner. In this sense a perfectly pure colour is not obtainable even from the solar spectrum itself, however much it be dispersed, or however narrow a portion of it be taken. To explain my meaning, I will suppose separate nerve fibres of the retina are sensitive to the different primary colours, whatever they may be; then, what I assert, and can prove, is that no part of the spectrum, however small, acts on a single fibre (excepting, perhaps, at and near the extreme ends of the spectrum). Assuming this to be true, it follows that experiments, like those recently described by Lord Rayleigh, in which the green and red of the spectrum are combined and produce the sensation of yellow, do not in the least prove that the sensation of yellow is necessarily a compound sensation, or that yellow is not a primary colour. A great deal of the difficulty arising in the consideration of the effects of mixing colours disappears when it is understood that colours neutralise rather than combine with or add to each other, the resultant sensation being one which may be described as a mixture of more or less white or uncoloured light, with as much of the colouredness as is not neutralised. E. H.

Glasgow, *Dec.* 3, 1881.

Charles Brush, of Cleveland, Ohio, is declared to have perfected a new invention for storing electricity. The design consists of a battery in the same sense as in Planté's and Faure's, but the details are entirely different, and do not infringe upon the rights of either. Mr. Brush uses for his storage reservoir metal plates, so arranged that they are capable of receiving a very large charge of electricity and of holding it for an indefinite time. The storage reservoirs vary in size as desired, may be transported from place to place, and used as desired. Each citizen may then run his own electric light as he pleases; the plates can be put on street-cars, connected with the axles, and made to run the cars without horses, and steam-cars may be ultimately run in the same way. The practical character of the invention is said to be settled, and it is simply a matter of expense, but the details of the methods are not made public.—*Frank Leslie's Magazine.*

RIGHT-HANDEDNESS.

By James Shaw.

THERE is a difference both in structure and capacity between the left and the right lung. The former has two lobes, the latter has three. Suppose 240 inches of air have been inhaled, about 130 have been taken into the right lung, and 110 into the left. Watch at the same time the right side of the chest, and it will be seen to be more bulged out during inspiration. Meanwhile, the lower ribs, as they recede from an imaginary middle line, are firmly bound by a ligament, or short cord, to the liver. The liver, a heavy organ of about 4 lb. weight, inclines the centre of gravity to the side to which it swings, and the greater expansion of the right lung and the shifting of the liver to the right side tend to shift the centre of gravity to that side.

The heart, firmly attached to the spine and midriff, remains immoveable, but the stomach and spleen incline to follow the liver. They cannot follow the left ribs in a contrary direction, for they are not joined to them. The shifting of the line containing the centre of gravity towards the right has no countervailing-force opposed to it, for although the greater part of the heart and the spleen is on the left side, it is not enough to balance the part carried towards the right by the descent of the diaphragm, during nspiration.

It has been estimated that the viscera of the abdomen and the chest weigh heavier, by about a pound, on the right side than the left. Now it must be plain that if the right side be heavier naturally, and still heavier during the inspiration that precedes effort, the foot that supports it will be more leant upon; and if we lean more upon the right foot, it will afford a steadier basis of action for the right arm than for the left. According to this view, first clearly put forth by Professor Buchanan, there is a mechanical reason for right-handedness. The right leg is first preferred and utilised in action, and upon this preference the right hand and arm come more readily into use, and are then taken up consciously and educated as the skilled limb. No sooner is a beginning made of preference than the muscles oftenest or most strenuously used get stronger.

Violently exercise the right hand and arm, and you might expect that both lungs would inflate to their uttermost. But this is not the case. The right lung is better filled with air in proportion to its capacity than the left. If we exercise the left arm, the left lung is more inflated, according to capacity, than the right. We may even witness the right cheek of a man, violently engaged in lifting a load with the right hand and arm, inflating unconsciously. In some physical efforts, such as throwing a stone, in which the centre of gravity advances from the right, forward, and towards the left, the dilation of both cheek and lung has been observed, passing over from the right to the left. The temptation to prefer the right shoulder, the right arm and hand, in lifting a dead weight, because of the centre of gravity being more nearly over the right foot than over the left, is quite a natural one. So, when a carter puts his shoulder under the shaft of his cart, he prefers to hoist it up with the right shoulder, as having more power in raising it.

But the question may be urged, why are burdens generally carried on the left? Although portable loads are generally placed on the left shoulder, this, instead of being at variance with the mechanical theory, is really a proof of it. When a man has a heavy weight upon his left shoulder, the burden is, in reality, borne by the right rather than by the left lower limb. The body is inclined to the right, so that the mechanical axis passes from the left shoulder to the right foot, and the load is retained over it by the help of the right arm. In the case of burdens, such as fishwives' baskets, borne on the left side, it must be remembered that as the right side is the heavier, these burdens help to restore equilibrium with less of a bend towards the opposite side, and so leave the motion of the limbs less constrained. This is probably one of the causes why a nurse carries her child in her left arm, although we must recollect that, by doing so, she has another advantage, namely, the freedom of the right hand for work.

Professor Ferrier informs us that the brain has a cross action. The left hemisphere governs the right side, and the right hemisphere governs the left side. Therefore, when we see with our right eye, we see with the left side of our brain, and when we see with our left eye, we see with the right side. Now, not only has the right side a mechanical advantage; but it is strongly suspected that the left hemisphere of the brain, which governs the movement of the right side muscles, is, so to speak, a battery of greater power than the right hemisphere. Dr. Boyd made observations on the patients in St. Magdalen's Hospital, and he sets forward as a curious result, that, after weighing separately the hemispheres of 200 individuals, almost invariably the left hemisphere exceeded by an ounce the weight of the right hemisphere. To do so exactly, however, requires great nicety, as there is no definite division between the two hemispheres; and so we are not surprised that in Dr. Wagner's experience the proportion of cases having heavier left lobes was as five to two. In the Report of St. George's Hospital (1869), there is recorded a case of loss of speech and paralysis of the left arm of a left-handed lad, whose brain, after death, exhibited a softening of the right hemisphere. The question of how much the strength and dexterity of the right hand depends on the shape and nature of the brain substance is beset with difficulties; but the balance of the evidence is in favour of the constitution of the brain itself being a reason in favour of right-hand predominance. A reason founded on nature is much more satisfactory than the notion, somewhat prevalent, that right-handedness is a fashion. True, there is a percentage of our fellow-creatures left-handed; and there is a difficulty to account for this peculiarity. But it is not easy to account for many peculiarities equally striking; such as the want of beard in some men; the greater or less number of teeth, toes, and fingers; the heart being found occasionally on the right side; and the transposition of the viscera. Some men can shut one eye and keep the other open at the same time. Others can only do so with difficulty; and others, again, are quite unable to do so. From the evolutionist's point of view, it seems to me as if our destiny were to become more intensely and more generally right-handed than we are. If we go far enough back in infant biography, we arrive at a period when locomotion is chiefly performed by the aid of all four limbs. This is the case with adult apes and monkeys, who shamble along on a plain, or climb more gracefully in the woods, by the aid of all their limbs. With the infant, the difference of internal structure—throwing the centre of gravity to the right—is scarcely perceptible.

It is argued by some writers that it would be a great advantage were we ambidexter, using both hands with like skill. Now, no one doubts that the specialisation of hands for the purpose of grasping, and feet for locomotion, is of more advantage to man, than if he had four hands fitted for both functions. As the child grows older, the difference of hands appears; and this difference, in all civilised countries, is eagerly helped by precept and example. As in playing whist, it is better that partner should have many trump cards and self few, than that each should have an average number; so it is found that in a world where time is so valuable, where art is long and life is short, it is better one hand should be very well educated and the other comparatively neglected, than that each should have a moderate aptitude.

We can go back, in imagination, to the time when the grasping of a stick or stone was all the education received by a human hand. We can suppose the make of the body at that time more symmetrical, as an infant's is with us now. It is like going back to the time when the ancestors of our horses had more of a normal foot—three toes, instead of the one toe of our present steeds. Right-handedness would not be so regular nor so apparent then; just as it is said to be with Fijians at the present day, or, as it has been observed with the African elephant, which has a tusk called the "servant," with wh'ch it burrows more freely, but which is not so regularly the right tusk as the working hand is the right one with us. As soon, however, as man combined, either in labour or in war, the necessity for preferring one hand to the other would become apparent. Indeed, the evident advantage of shielding such a vital organ as the heart from wounds, and pushing forward the less vital right side, would incline men to place the shield in the left hand and the sword in the right. At all events, the thickening of complex circumstances would be unfavourable to a state of unstable equilibrium. When once the movement of preference began, everything would tend to strengthen it. Many of the implements by which man conquers nature would require to be made either to suit the supinating motion of the right hand or of the left.

The slightly stronger side would gain the day, and become the more apt and stronger after it had gained it. Now, since it has become an accomplished fact that screws, gimlets, scissors, scythes, &c., are all made for right-handed men, he who would educate us to the ambidexter must have two handles on every door, two methods of winding up every watch, Janus-shaped carpenters' benches and printcutters' gauges, duplicate sets of screw-nails, scissors and scythes.

Not only is the right hand the most dextrous, but, as far as I have extended my observations, in those cases in which there is a difference in the strength of vision between the right and the left eye, the advantage, more frequently, lies with the former. It may be thought that investigations of this kind are unpractical, but it will not appear so when it is stated that they have been the means of discovering serious differences in the organs of vision of the same individual, which drawbacks can be greatly modified by the spectacles of the optician. The relief, in reading, given to such a lop-eyed person when he has got a lens suited to either eye, is so great that, when once discovered, it is never forgotten. It is wonderful

how late in life some persons have been in detecting such inequality of vision. Indeed, there is a case mentioned by the younger Herschel, of an individual not knowing of complete blindness of one eye until advanced in years. When there is a weak one and a strong, it is generally the weak one that is most liable to disease, from its unconsciously straining to share the labour of reading, painting, or engraving with the other.

EARTH TREMORS.

(From the *Times*)

One day during the past summer, at the end of a long uphill beat after the partridges, I threw myself breathless on the ground, and on my back waited for the others to come up. As they drew near, five or six strong, tramping heavily through the turnips, I was struck by an apparent tremor of the earth beneath me. It was shivering like a jelly—or I was; for a moment I was in doubt which. Spreading out my hands upon the surface, and lying as close and flat as I could, I was soon made sure that the tremor was really in the earth and not in me. It grew more and more distinct, keeping time with the tramp of the walkers. When at last they reached me I told them of their Neptunian feat, and, making them jump altogether a few yards off was gratified to find that I could thus bring about a very respectable earthquake at will. The motion was very peculiar, and I can well believe that a queasier stomach than mine would soon be conscious of something very like a *mal de terre*. We examined the structure of this skipping hill, but found nothing that helped us much to an explanation. It was mainly made up of a thick cap of gravel on a base of red sandstone, and so was not likely to contain anything like a high-arched hollow or concealed morass within.

This vivid little experience made me readier, perhaps, than some to accept the striking statements about earth-shaking made by the brothers Darwin at the York meeting of the British Association. Especially was I prepared to give credit to what they quoted from the Astronomer Royal about Greenwich Hill and the Observatory. He wrote:—

"In the old times of Greenwich Fair, some twenty years ago, when crowds of people used to run down the hill, I find the observers could not take reflection observations for two or three hours after the crowd had been turned out. . . We do not have anything like such crowds now, even on Bank holidays, and I have not heard lately of any interference with the observations."

There is as little foundation for the calumnious hypothesis that the observers whose reflections were thus agitated had been visiting the fair themselves as for the suggestion that the above experience of my own took place after luncheon. No, the truth is, the solid earth is a very elastic solid after all, and Greenwich Hill and the Observatory and all that it contained were trembling like my Highland knoll. The *howdah* of the Atlas-elephant that stands on a tortoise is a rather ricketty structure, and quakes with every jog of the Titanic beast. But is it not being tugged at by every petty planetoid, pulled from its path by every planet, heaved all awry through its yielding bulk by sun and moon in their courses? It is; but over and above these longer, graver motions, there are incessant tremblings and quiverings in quick periods measured by seconds or less. This unlooked-for sensitiveness to small stresses, this incessant vibration when all obvious disturbing causes are eliminated, are the new facts that the Darwins have so strikingly brought out. How solid rock and massive piers of stone warp under heat and cold like unseasoned wood, how a wide stretch of ground may swell and rise for hours together after a little water has been poured on it, how the passage of a train miles away, or the pressing of a finger on the ground near at hand, may be enough to deflect the plumb-line to a visible degree—these and many other new phenomena are detailed in the full and most interesting preliminary report on the Lunar Disturbance of Gravity handed in to the Section of Physics by the ingenious brothers.

The title reminds us that, as so often in science, it was in looking for one thing that they found another. Every one knows that as the earth pulls the moon round in its monthly orbit, so, too, the moon pulls the earth and everything upon it. If a plummet be hung up right under the moon, so to speak, the earth is drawing the bob downwards, the moon very much more feebly pulls it upwards. The result is that the bob weighs a trifle lighter than if the moon were abolished. Thanks to the moon, the string is less severely strained. If the moon be not right overhead, but down a little towards its rising or its setting point, the bob will be a little drawn aside out of the straight and the plummet will no longer give a true plumb-line. As the moon rises, crosses the sky, and sets, then the direction of the plumb-line will change through a small angle. Of course, even when no moon is seen, its silent influence must be felt, and the plumb-line will return to its position by the time the moon is ready to rise again. How small the change really is we may gather from the fact that with a plummet 300 yards long the travel to and fro of the bob could scarcely in this country reach a thousandth of an inch. This is what is meant by the lunar disturbance of the direction of gravity; and there must, of course, be a solar disturbance also, the same in kind, but naturally very much smaller in amount. To investigate these disturbances experimentally clearly calls for refined skill and very delicate apparatus; but Sir William Thompson, to whom instrumental difficulties are always but child's play, in suggesting the investigation three years ago, had in view the detection of an influence still more recondite and refined.

I have said that the moon, in pulling aside the bob of the plummet, pulls also on the earth beneath it. If the earth were perfectly stiff and unyielding, this pull could have no effect on the deflection of the plumb-line. But if, as we have reason to believe, the earth yields like a great viscous mass to great stresses as well as to small ones, a hump of solid earth—a land-tide—will travel round the globe in obedience to the moon's attraction. This hump in its course will pass under the suspended plummet, and the actual deflections of the plumb-line as observed will no longer agree with those reckoned on the supposition that the earth is rigid. If we had an instrument, then, by which the minute aberrations of a carefully-suspended pendulum, isolated as far as possible from all local disturbance, could be magnified up to the point of visibility, we should have it in our power to settle some very pretty points in the physical theory of the world. Such an instrument, after various trials and failures, the Darwins have erected in the Cavendish Laboratory at Cambridge.

A massive stone, weighing three-quarters of a ton, is bedded in a pit upon the native gravel. It is surrounded by a trench, a foot wide, to isolate it completely from the floor and the building. The pendulum is a massive cylinder of pure copper, hung, by a brass wire about a yard long, inside a hollow cylindrical copper support, that rises from the stone. A tiny galvanometer mirror is hung by two fine threads, one of which is fastened to the bob, and the other to a projection of the fixed support. This suspension is so arranged that any movement of the bob displaces the mirror to a much greater degree. A ray of light is sent from a distant lamp on to the mirror, and thence reflected to a scale seven feet away. The magnification resulting from this double process is something like 50,000 times. To still and quench accidental tremors, the hollow copper cylinder is filled up with a mixture of spirits and water. It is a fact, made out by physicists that a boiled mixture of gin and water is much more viscous and clogs the motions of bodies immersed in it much more effectually than either the neat gin or the simple water. Further, to ward off the effects of external changes of temperature, the whole instrument is immersed in a tank of water resting on the stone; and lastly, after the precedent of the Tishbite, the surrounding trench is also filled up with water. Thus protected, the apparatus might seem sufficiently cut off from local influences, but as a fact its sensitiveness is now so great, that the observation has to be carried on in another room by means of a window and a telescope. Standing in the room itself 16 feet away, it is enough to shift your weight from one foot to the other to cause the speck of light to run along the scale. The same result follows if you press steadily with your fingers on the stone edge of the trench, but you may strike a good sharp blow even on the stone base without effect. It is the distortion of the soil by slight, steady pressure that is transmitted through solid gravel and stone, and shows itself as a microscopic deviation of the pendulum. Such being the case, the instrument should be delicate enough, in all conscience, to determine lunar and even solar disturbances in the direction of gravity; but, unfortunately, having got so far we seem almost to have done too much. When regular series of observations are made it is found that the pendulum is hardly ever steady. The image on the scale dances about incessantly. The ground is never really still. Some days it may be quieter than others and generally there is evidence of distinct diurnal periods, but the minor zigzags constantly interrupt, and at times reverse for an hour together, the slower march northwards or southwards. These tremors have been hitherto so persistent and so wildly irregular, that for the present, at least, the prospect of unravelling from them the perturbations due to the moon does not seem very near. Mr. George Darwin talks of the probable necessity of building a gravitation observatory at the bottom of a mine. There, it may be hoped, the railway train and the market cart will cease from troubling, and the plummet, save for the steady paces of the moon, will be at rest. The work of examining and observing these tremors of the surface is, however, still going on at Cambridge, and already several sharp seasons of microscopic earthquake unsuspected outside have been noted. Sometimes a very storm of tremor breaks out, for which no sufficient local cause can be traced.

Even so far the outcome of these experiments may prove of high value to practical astronomers. The piers on which their great telescopes turn are built of solid stone, hitherto regarded as the material most insusceptible to change or disturbance. The Darwins have shown that such piers are really most sensitive to inequalities of temperature and to small stresses. They yield and warp to a most unexpected degree. Their bad conducting power is responsible for this in part, and it is fruitfully suggested that it might be well to plate the piers with copper and to swathe them with flannel. Astronomers, who, to their vexation, have to redetermine the level of their instruments from hour to hour, and who have long suspected the occurrence of microscopic earthquakes, will take note of this practical hint. They will make ready use, too, of the observation here recorded as to the effect of the observer's own weight. They will think more of the drainage of the soil around their instruments after the observation on the irregular and long-continued swelling of the ground that results from the percolation of water. Meanwhile, the British Association and Cambridge may be congratulated on the new and valuable field of work thus opened out under their auspices, and especially on their having enlisted the services and energies of two workers who so worthily keep up the tradition of an honoured name.

BREATHING.

By Dr. J. Mortimer Granville.

(*Abstract.*)

Every act of life, every movement, every thought, involves the final use of some particles of the body. In the great majority of instances the material used cannot be used again, and must be disorganised and removed. This is effected through the agency of the blood, which brings the materials of food within reach of the living cells of which every tissue of the body is composed. These draw nourishment from the blood, just as the plant takes up material nourishment from the earth through its rootlets. Side by side with this process of feeding and growth, and an indispensable part of the process, is the interchange of elements—oxygen, hydrogen, carbon, nitrogen, and the like. In short, the vital process is in a large measure chemical, and the oxydising agent—oxygen derived from the atmosphere—is the most potent agent and factor in the production of the general result. If the supply of this agent is not sufficient for the vast purposes which it is required to effect, the animal must suffer a diminution of health, and in the end die. It is plain, therefore, that the phrase "breath of life" is full of the deepest significance. If the animal cannot breathe—using that term with its broad meaning—it must cease to live. It follows that the first concern of the living being, for himself and other living beings, should be to secure a full and fresh supply of pure air. In the case of children, this is especially necessary, for the obvious reason that the chemico-vital changes of structure in their organisms are more active and persistent than those which go on in the adult body. The child is growing in bulk, as well as constantly using up the materials of its body and requiring to replace them by new. The result is a large, continuous, and inexorable demand for copious supplies of fresh air. How is that demand complied with in the majority of cases? Growing children ought to live in the open air; but we mew them up in schoolrooms and confine them to the house on the smallest pretence of weather or indisposition. When a child is ill, its systemic demand for air is not diminished, but rather increased, as is plainly shown by the quickened pulse and breathing. Bad weather is no excuse for the confinement of children indoors. The danger of "cold" is increased by this treatment. Children are made delicate, and susceptible to the depressing effects of sudden or great changes of temperature, by the practice of calling or keeping them indoors for every shower of rain or cold wind. They are also rendered generally weakly by wrapping-up. Later on in years the requirement is very much in proportion to the activity. But, even in a state of rest, the need for oxygen is considerable. If health is to be maintained, it must be in excess of the actual chemical requirements. In truth, the more air of the purest description which can be taken into the lungs the better. Wind is, as a rule, an advantage, because there is less chance of the atmosphere we inhale having stood stagnant over bad soil, or around sources of poisonous or deleterious exhalations, and thus contracted pollution. Breathing bad air is disastrous. The "stifling feeling" and "headache" which are so commonly produced by sitting in a public meeting, are the immediate and more pronounced effects of breathing bad air; but, long before these inconveniences are consciously experienced, and even when they are entirely absent, harm is being done. The robust may not feel the effects, but they too are injured, while the weakly are enfeebled, and the seeds of disease are sown, and will probably spring up later on, and cause trouble of some kind. Nature's preventive remedy for disease, whether in the individual or in the multitude, is a bath of pure air. *The Estates Roll.*

MAN'S PROPER FOOD.

Long before reading Dr. Carpenter's articles in Knowledge, I had believed that, as a race, we are prone to eat more meat than is necessary. I must, however, protest against Miss Kingsford's argument, which, put baldly, is this:—

"Men and apes are closely akin; apes eat fruit and herbs only, therefore men ought to eat fruit and herbs only."

I cannot, however, see why it is desirable for men to go out of their way to assimilate themselves to apes, and I even think it possible that the divergence of the human from the ape stock began when men became omnivorous. I cannot assent either to the validity of the arguments derived from the animals mentioned by Miss Kingsford and in your article. I say that the wolf is incomparably superior to horse, mule, or camel in endurance, and I would myself gladly back either a lion or a tiger against a gorilla. There may be no flesh-fed animal equal in strength to the rhinoceros, and the other grass-fed animals mentioned are also grand specimens of bulk and strength (and often too of unwieldy inertness) but, *weight for weight*, they cannot compare in strength or activity with the carnivorous animals. Were the vegetarian Indians, who became intoxicated from eating meat, equal in stature, strength, or intellect to the omnivorous European? I guess not; it does not appear indeed that they were not South American Indians, some tribes of which are scarcely human. And that even a purely flesh diet may not be prejudicial is, I think, pretty well shown by the physique and strength of the Sioux and some of the other sufficiently fed tribes of North American Indians. I have lived with them in the buffalo-hunting season, when they and (after my biscuit was done) I too practically lived on buffalo meat only, and if I had not seen it in them, I could not have believed in man's having such wiry endurance; whilst for myself, I can say that, though blessed ordinarily with health and strength beyond the average, I have at no other time known either in anything like the perfection in which I enjoyed them then. Of course, the active life in the open air accounts for much of this, but the diet must, at least, have been wholesome. Personally, I believe in a mixed diet, but I also believe that man's capability of eating *anything* is one great element of his superiority to the beasts of the field. Certainly his adaptability to any climate is owing to that capability. Are the northern regions to be depopulated on the ground that, as there are no fruits and herbs there on which Miss Kingsford's apes can live, man also has no right to live there? Practical.

CALLAO "PAINTER."

On approaching Callao in a steamer, at a certain season in the year, the traveller suddenly becomes aware of an unbearable stench in the cabin and everywhere else on board; he naturally asks "what is the matter,"—he is informed "it is the 'Painter,'" the traveller, not yet knowing what really causes the vile smell, wishes "the painter would clear out with his smells." The next thing to be noticed is that the *white paint* on board becomes blackened. If the person who has observed the offensive smell for the first time, as well as the gradual blackening of the paint, has any knowledge of chemistry, he at once sees the cause of mischief, viz.: a great excess of Free Sulphuretted Hydrogen in the atmosphere—this is really the case, the air smelling abominably strong of "rotten eggs"—*i.e.*, H_2S.

If he now looks at the sea around him, he notices that the water has a yellowish milkish appearance, showing the presence of sulphur in the water—anyone who has seen Harrogate Sulphur Water will see what I mean, for the cases are similar.

His next thought is naturally what causes this singular phenomenon.

In answer to his question to those on board, "are you troubled with earthquakes at this time on land (Callao) and sea?" he is told that such is the case.

That earthquakes are felt at this time, the traveller, whether scientist or not, has rather a questionable gratification of finding out for himself before he has been long in the neighbourhood of the "Painter." That some of these earthquakes are serious occurrences our Geographies show us, though nothing is said about the "Painter."

The conclusion I have come to is one which anyone else would come to respecting this disagreeable local phenomenon, viz.:—that at a certain season of the year a submarine volcano breaks out impregnating the sea with its sulphurous vapours, then when the sea has absorbed its share, the air becomes filled with the gas.

It seems hardly necessary to explain why the white paint should become black. It is simply due to the action of the gas already alluded to above—on the white lead of the paint, sulphuretted, hydrogen having the property of changing the white lead or oxide into the sulphide of lead.

Bitumen is, I believe, thrown up onto the shores of Callao, just the same as it is on the shores of Mexico, where it soon becomes a hard mass. F. C. S.

COLOURS OF ANIMALS.

PERMIT me to reply to "B. Donbavand," [let. 130] that his remark concerning flounders being "concealed by *a covering of sand*" is entirely beside the question ventilated in my paper on colour in animals. If your correspondent has ever *seen* a sole or flounder, he must know how accurately the sandy hue is mimicked by the upper side of the animals.—If he had (as I doubt) ever seen a flounder lying motionless and uncovered in an aquarium tank, he would never have penned his sweeping statement above referred to. Permit me to say that I have repeatedly had the greatest possible difficulty, both in gazing into shallow water, and in aquaria, in distinguishing the outline of flounders of whose presence I was aware, from the sand on which they reposed. "B. Donbavand's" remarks on the "extreme tenuity" of scientific investigation (he might have used a plainer term than "tenuity") strike at the root of all scientific advance. Who, pray, is to decide what is important and what is insignificant in scientific research? Who can tell the bearing of even apparently the most trifling fact on future research? "B. Donbavand" arrogates to himself just a little too much authority, when he writes cynically of "extreme tenuity" in such a case as Mr. Darwin's observations on "Worms." The single sentence in "B. D.'s" letter, wherein he speaks of Darwin's work as a "huge paradox," is just a trifle too near silliness to warrant further remark.

In answer to "Ornithorhynchus" [100], who asks why the stings of bees and wasps do not affect a toad when it swallows the insects, I may simply refer him to the common-sense explanation of very plain differences between the constitution of a low vertebrate, such as a toad or frog, compared with the higher warmblooded vertebrates. Your correspondent is evidently thinking of the effects of the insects' sting on the human type when he puts his question concerning the immunity of the toad. But analogy reveals many examples of the fact that the powers of different quadrupeds to resist the evil effects of noxious foods, must be due to differences in the nervous sensibility, and to other features in the constitution of the animals in question. A donkey eats raw nettles, a dietary that would kill a man by producing severe throat inflammation. A secretary bird devours serpents, which may contain poisonous matter sufficient to kill a legion of birds; and man is in the same position as the bird, inasmuch as he can *swallow* safely poisons which only act when introduced directly into the blood-circulation. In a word, individual or race peculiarities serve to render innocuous to one animal what is a poison to another. The black races of men do not suffer from yellow fever, which kills off the white. A tropical sun burns and blisters a white skin, but leaves the black skin untouched.

The second Query [101] of "Ornithorhynchus," regarding "ants," will be best answered by referring him to Sir John Lubbock's "Scientific Lectures" for a full exposition of what is known about the habits of those insects.

ANDREW WILSON.

SCIENTIFIC GHOSTS.

THE following paragraphs are from the "Leaves from a Naturalist's Note Book,"* by our esteemed contributor, Professor Andrew Wilson, a work we can cordially recommend. It contains articles (some of which many of our readers may have seen in various magazines and journals) on many and very various subjects, Giants, Kangaroos, Food and Fasting, Jelly Fishes, Whales, Science and Crime, Leaves, &c., &c., all treated clearly and correctly, and all treated in a most attractive manner. The subject we select for extract is a good illustration of Professor Wilson's method :—

Modern science has made us aware that the old belief in apparitions rested on nothing more than illusive fancies caused by some kind of physical derangement of the person so affected. It is important that young persons should be made thoroughly aware of the fact that there never was and never will be any such fancy which is not capable of being explained upon natural grounds. A person in weak health, though in perfect possession of all his faculties, begins to be troubled by waking visions of persons with whom he may be familiar, or who may have been long dead, or who sometimes may appear as perfect strangers to him. The spectres who flit before him, "come like shadows" and "so depart." They represent, in the most perfect manner, the reproductions of things that are or were—utterly intangible creations. The subject of these visitations may hear the spectres converse, and they may even talk in turn to him. He is perfectly aware of their visionary nature, and is as convinced of their unreality as is the friend who sees them not, and to whom the phantoms are described. No suspicions of insane delusion as to these visitations can be entertained for a moment, and the question may therefore naturally be put to the man of science, "How can these illusions be accounted for?" The answer is to be found in one of the simplest studies in the physiology of nerves and of mind, and shows us that these illusions have a material basis, or that, in the words of the poet, the

"Shadow proves the substance true."

One of the most interesting cases of vision seeing by a person of culture and intelligence is that related in the *Athenæum* of January 10, 1880, by the Rev. Dr. Jessopp, who, in Lord Orford's library, when engaged in copying some literary notes, saw a large white hand, and then, as he tells us, perceived "the figure of a somewhat large man, with his back to the fire, bending slightly over the table, and apparently examining the pile of books I had been at work upon." The figure was dressed in some antique ecclesiastical garb. The figure vanished when Dr. Jessopp made a movement with his arm, but reappeared, and again vanished when the reverend narrator threw down a book with which he had been engaged. Dr. Jessopp's recital called forth considerable comment, and amongst others a letter from the present writer, detailing the familiar theory based on the principles of subjective sensations, treated of in the present paper. After noticing the fashion in which subjective sensations become projected forwards, the author says (*Athenæum*, January 17, 1880): "The only point concerning which any dubiety exists, concerns the exact *origin* of the specific images which appear as the result of subjective sensory action. My own idea is that almost invariably the projected image is that of a person we have seen and read about. . . . In Dr. Jessopp's case there is one fact which seems to weigh materially in favour of the idea that the vision which appeared to him in Lord Orford's library was an unconscious reproduction of some mental image or figure about which the Doctor may very likely have concerned himself in the way of antiquarian study." It is most interesting to observe that in the succeeding number of the *Athenæum*, Mr. Walter Rye writes: "Dr. A. Wilson's solution 'that the "spectre" . . . was an unconscious reproduction of some mental image or figure about which Dr. Jessopp may very likely have concerned himself in the way of antiquarian study,' seems the right one, and I think I can identify the 'ghost' The ecclesiastically dressed large man, with closely cut reddish-brown hair, and shaved cheek, appears to me the Doctor's remembrance of the portrait of Parsons, the Jesuit Father, whom he calls in his 'One Generation of a Norfolk House,' 'the manager and moving spirit' of the Jesuit mission in England. . . . Dr. Jessopp when he thought he saw the figure, was alone in an old library, belonging to a Walpole, and Father Parsons was the leader of Henry Walpole, the hero of his just-cited book. Small wonder, therefore, if the association of ideas made him think of Parsons."

All such illusive visions are thus readily explained as the creatures of an imagination which, through some brain-disturbance, is enabled to project its visions forward, on the seats of sense, as the "ringing" in our ears is produced by some irritation of hearing-centre of the brain. The known vision is a reproduction of a present memory, and the unknown vision is the reproduction of a forgotten figure which has nevertheless been stored away in some nook or cranny of the memory chamber.

We may thus dispel the illusion by its free explanation; and science has no higher function or nobler use than when, by its aid, a subject like the present is rescued from the domain of the mysterious, and brought within the sphere of ordinary knowledge.

* "Leaves from a Naturalist's Note Book." By Andrew Wilson, F.R.S.E., &c. (Chatto & Windus, London.) Price 2s. 6d.

A VERY IMPORTANT GIFT has been made to the New York Museum of Art by its President, John Taylor Johnston. It consists of a collection of 331 engraved gems made by the Rev. C. W. King, of Cambridge, England, a connoisseur and authority on glyptic art. For the most part these antique gems follow in chronological order the Di Cesnola collection, which Mr. King, in a treatise, called "a true revelation in the history of glyptic art." Speaking of this addition to the museum, General di Cesnola said that with it two more departments were now unsurpassed by any similar ones in the great European Museums. Each of the pieces is accompanied by a plaster cast. The catalogue is in Mr. King's handwriting, and a treatise on glyptic art, by Mr. King, accompanies the collection.—*Frank Leslie's Magazine.*

Letters to the Editor.

[The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with their writers. All communications should be as short as possible, consistently with full and clear statements of the writer's meaning.]

All Editorial communications should be addressed to the Editor of Knowledge*; all Business communications to the Publishers, at the Office, 74, Great Queen-street, W.C.*

All Remittances, Cheques, and Post-Office Orders should be made payable to Messrs. Wyman & Sons.

⁂ All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.

All Letters or Queries to the Editor which require attention in the current issue of Knowledge*, should reach the Publishing Office not later than the Saturday preceding the day of publication.*

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

Our Correspondence Columns.

THE PRIMARY COLOURS.—RED AT THE BLUE END OF THE SPECTRUM.

[149]—I venture to suggest the following explanation of the difficulty raised by M. W. Laing, letter 71, p. 96. The sensitive ends of the optic nerve take the form of cones; each cone is divided into three parts, and each part is able to vibrate independently of the others. As a violin string of a certain length, thickness, density, and tension is only able to vibrate a certain number of times in a second, and is set vibrating by those vibrations of air only which synchronise or keep time with the string's periods of vibration, vibration for vibration, or every second, third, fourth, &c., air vibration to each string vibration, so each part of the cone is set vibrating by those vibrations of ether which synchronise with its periods of vibrations, and by those only. The sensation of red colour is produced by the vibrating of the thick end of the cone, green, not yellow, by the middle, and blue by the thin end, these three being the primary colours; about 392 billions of ether vibrations in a second synchronise, vibration for vibration, with the periods of vibration of the thick end of the cone, and about 757 billions with the thin end of the cone. Now, it is evident that with 784 (twice 392) billions of ether vibrations in a second, every *alternate* ether vibration would synchronise with *each* vibration of the thick end of the cone, and the result would be a weak red, contiguous to and beyond the blue of the spectrum. Thus we should have *about*

392 billions of ether vibrations in a second, represented by					red.
575	,,	,,	,,	,,	green.
757	,,	,,	,,	,,	blue.
784	,,	,,	,,	,,	red.

Higher numbers of vibrations are probably absorbed or reflected by the refracting media of the eye (*i.e.*, the conjunctiva, cornea, aqueous humour, crystalline lens, and vitreous humour).

In giving yellow as a primary colour, the "commissioners" were probably guided by their knowledge of pigments and their compounds, which knowledge only misleads in the matter of coloured lights.

I trust that my explanation, be it right or wrong, is "plainly worded," and the assumed facts "exactly described"; but I cannot hope to rival "M. W. L." in these matters.

W. Rayment, Amateur of Sc.

THE MOON'S ROTATION.

[150]—C. O. K.- [39] Newcomb, in his "Popular Astronomy," says:—"The most remarkable feature of the motion of the moon is that she makes one revolution on her axis in the same time that she revolves round the earth. . . . The reason of this peculiarity is to be found in the ellipticity of her globe. That she should originally have been set in revolution on her axis with precisely the same velocity with which she revolved around the earth, so that not the slightest variation in the relation of the two motions should ever occur in the course of ages, is highly improbable. . . . The effect of the attraction of the earth upon the slightly elongated lunar globe is such that if the two motions are in the beginning very near together, not only will the axial rotation accommodate itself to the orbital revolution around the earth, but, as the latter varies, the former will vary with it, and thus the correspondence will be kept up."

Herschel, "Outlines of Astronomy" (S. 436ª), refers to a remark made by Professor Hansen, viz., "that the fact of the moon turning always the same face towards the earth is in all probability the result of an elongation of its figure in the direction of a line joining the centres of both the bodies acting conjointly with a non-coincidence of its centre of gravity with its centre of symmetry." He then gives a practical illustration.

"Suppose, then, its (the moon's) globe made up of materials not homogeneous, and so disposed in its interior that some considerable preponderance of weight should exist eccentrically situated, then it will be easily apprehended that the portion of its surface nearer to that heavier portion of its solid content under all the circumstances of a rotation so adjusted will permanently occupy the situation most remote from earth."—A. T. C.

FOUR FOURS, SINGULAR NUMERICAL RELATION.

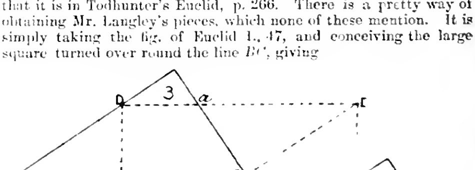
[151]—It may be as new to some of the readers of Knowledge as it was to myself when first shown the other day that all the numbers to twenty inclusive (and many upwards), with the single exception of nineteen, may be expressed by four fours, using any signs necessary except those of squaring and cubing, in which figures are required. Only four, but at the same time, the whole of the four figures are to be used. I do not say that it is impossible to obtain the number 19 in this way, but neither myself nor the gentleman who showed me the above has been able to do so. With the hope that this may prove interesting to at least some of the readers of your valuable paper.—Yours, &c., Cupidus Scientiæ.

[Our correspondent gives the solutions for all numbers from 1 to 20, except 19. These shall appear next week. In the meantime we leave the problem as an exercise to our readers.—Ed.]

THREE SQUARE PUZZLE.

[152]—A great number of letters relating to this puzzle have been received, nearly all of which we should like to print, but we have nearly twenty pages of correspondence already in type, besides the correspondence received since No. 8 appeared. We have, therefore, absolutely no choice but to omit matter which otherwise would suit our pages exceedingly well.

Mr. Langley's puzzle has been solved and explained fully and exactly by J. O. M., W. T. Y., Mathematicus, F. F., J. S., and others. T. Turner, Thomas Mactaggart, and J. T. E. point out that it is in Todhunter's Euclid, p. 266. There is a pretty way of obtaining Mr. Langley's pieces, which none of these mention. It is simply taking the fig. of Euclid I., 47, and conceiving the large square turned over round the line *BC*, giving



We thus have the five pieces of Mr. Langley's figure, and, at the same time, see how they are to be arranged to fill the square *BE*. We leave 2 where it is; put 1 where *BAC* is, 4 on *Eab*, and then 5 and 3 together cover the triangle *bEc*, divided as shown by the line *cd*. R. A. Proctor.

THE QUERIES IN "KNOWLEDGE" (*Abstract*).

[153]—Perhaps you will forgive my making a few observations on Knowledge. Like many, doubtless, I have profited very much by such essays as those in the first half of each number. But, Sir,

t is very hard that space should be taken up with questions, the answers to which are in well-known text-books. May I suggest that such questions be not inserted, but answered shortly in the small print "Answers to Correspondents."

I also thought it very hard that so much room was given for "that the sun is cold," to men whom you say cannot comprehend an argument derived from the solar dark lines.—Yours, &c.,

Anti-Paradox.

[Anti-Paradox's letter is allowed to appear as a rather remarkable specimen of the class to which it belongs. Among some twenty or thirty kindly letter, recognising what we are trying to do in Knowledge, and, indeed, giving us credit for a degree of success which we ourselves would hardly claim, will come a letter or two such as the above. Anti-Paradox's complaint reminds us of a remark of a theatrical agent we met in New York, who said there were some who would not go to the theatre "unless they had orders given for all their family, and *then* they complained unless first-class carriages were sent to take them along." Putting aside the question whether those who have queries to ask have not a right to expect some space, and noting that many primers and text-books are not simply worded, even when (which is not always the case) they are exactly described, we would invite "Anti-Paradox," and the small proportion of our readers who view matters like him, to consider things from the point of view of the proprietors of such a journal as this. "Anti-Paradox" pays twopence for each copy, of which sum more nearly the half than three-fourths reaches the proprietors of Knowledge. Let "Paradox" inquire how much twelve, fourteen, or sixteen double leaves (as the case may be) of good paper is likely to cost, even at wholesale rate, and the probable expense per copy of such matters as composing, printing, folding, advertising, and so forth, to say nothing of editing. When he has done this, and notes the nature of the margin between a penny-farthing and such costs per copy, let him ask himself if it is quite reasonable for him to expect us to crowd queries, correspondence, &c., into "the small print 'Answers to Correspondents,' in order that he may have as nearly as possible the entire contents of Knowledge devoted to original matter. Observing that a page of small print in itself involves a loss (costing more than twice as much as a page of large print), let him notice that, on the average, we give him, as it is, as much original matter as would make the sixth part of such a work as my "Light Science" or Professor Wilson's "Leisure Hour Studies"—so that, for a shilling, he gets as much of such matter (fresh and fresh) as in such volumes costs six or seven shillings. It is "a very hard thing," he considers, "that we do not fill our entire space with matter so costly that if we did, the greater the sale, the greater would be the proprietors' loss. It would be, we venture to tell him, a "very hard thing" if our reward for giving six or seven pages of extra space to correspondence should be a claim for so much more original matter. It is "a very hard thing" to find room for so much original matter as we insert, and also to give space for correspondence, queries, &c., without making the proprietors cry out lustily at our extravagance (considering the price of Knowledge). If we had many such cheerful correspondents as "Anti-Paradox," this "very hard thing" would simply become impossible. We beg, on the proprietors behalf, to remind "Anti-Paradox" that the "very hard thing" which affects him is an infliction of his own choosing. He is not obliged to bring this terrible hardship on himself by expending the sum of twopence weekly on Knowledge, and then groaning because we answer queries in other than our smallest type, or admit inquiries from readers who do not fully appreciate the significance of the solar dark lines. To our more just and generous readers we say that we do the best we can to oblige all; we give to each class, correspondents, querists, mathematical students, chess and whist players, and original writers, more space than we can fairly afford. We feel satisfied that so long as we do so, the proportion disposed to be as unreasonable as "Anti-Paradox" and a few others have hown themselves, will be very small indeed.—The Editor.]

THE FIFTEEN PUZZLE.

[154]—The following is a solution to the Fifteen Puzzle, starting from the lost position :—

1	2	3	4
5	6	7	8
9	10	11	12
13	15	14	

Let R = right L = left U = up and D = down.

12. D.—11.10.9. R.—13. U.—15.14.12. L.—11. D.—10.9.13. R.—15. U.—14.L.—13.D.—9, &c., to L until the figures of the last two rows read.

9	10	11	12
15	14	13	

Then move: 15.14.13. R.—9.5.1. D.—2.3.4. L.—8.12.13. U.—14.15.9. R.—5.1.2. D.—3.4.8. L.—12.13.14. U.—15.9.5. R.—1.2.3 D.—4.8.12 L.

Then turn the box so that

4	8	12	
3	6	7	13
2	10	11	14
1	5	9	15

will be

1	2	3	4
5	10	6	8
9	11	7	12
15	14	13	

Then move: 14.13. R.—11.10. D.—6. L.—7. U.—14. U.—15.11 R.—9. D.—10.14. L.—11. U.—15. R.—14. D.—11.12. L.—13. U.—15.14.9. R.—10. D.—11.12.13. L.—15. U.—14. R.—13.D.—12.11. R.—10. U.—9.13.14. L.—15. D.—12.11.10. R.—9. up.—13.14.15. L.

Wishing success to your valuable paper,—I remain, yours truly,

Yawnups.

[The position attained by "Yawnups," which, of course, he does not regard as an actual solution of the problem, for which a money prize was offered in America, can be more readily reached; in fact, we do not see the plan of "Yawnups'" solution, many of the moves in which seem to be wasted. The actual number of moves in his solution, counting such a move as 9.13.14. L as three moves, is 102. By the following method the position is attained in 57 moves. It may, perhaps, be done in fewer, but the solution is straightforward, and its stages illustrate the method of dealing with such difficulties as occur in all "fifteen" problems :—4.8.12. D.—12.3. R.—13.9.5. U.—15.14.12. L.—3.4.8. D.—5.1.2. R.—15.13.9. U.—14.12.8. L.—2.3.4. D.—1. R. Now turn the box so that right hand runs

1	2	3	4

after 28 moves. We next get 5, 6, 7 right by the following moves:—7.11. L.—10. U.—6. L.—11. D.—7. R.—5. U.—6.11. L.—10. D.—7. R.—6. U. The two upper rows are now right, after 12 moves more, or 40 moves in all thus far. The last two lines now run thus :—

11		10	12
9	13	15	14

To get these right proceed thus :—11. R.—9. U.—13.15. L.—10. D. 11. R.—15. U.—10.14. L.—12. D.—11. L.—15. R.—10. D.—14. L.—15. D.—11. L.12.— U. The last two rows are now right, after 17

1	2	3	4
5	6	7	8
9	10	11	12
13	14	15	

more moves, or 57 moves in all. The blocks are now in the order which we may regard as one of the only possible forms of solution from the "lost position."—Ed.]

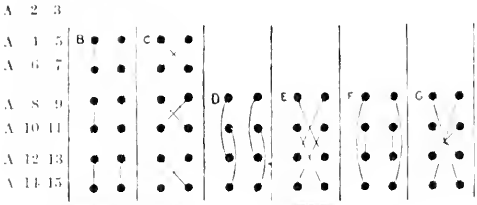
We have since received the following solution in 60 moves:—13.15.14. R.—1.5.9. D—2.3.4. L.—8.12.14. U.—9.13.15. R.—2.1.5 D.—3.4.8. L.—12.14.15. U.—5.9.13. R.—3.2.1. D.—4.8.12. R. One line is now right in 33 moves. Then: 14.15. U.—10.11 R.—6. D.—7. L. Two lines are now right in 39 moves. 15. R.—11. U.—13. U.—9. R.—10. D.—15. D.—11. L.—14. D.—12. R.—11. U.—15. U.—13. L.—14. D.—15. R.—11. D.—12. L.—15.14. U.—13. R.—10. U.—9. L.—or all the numbers right in 60 moves.

O. F. W.

THE FIFTEEN SCHOOLGIRLS.

[155]—In default of better, I enclose a sort of analysis and rule for doing the puzzle of fifteen, which is the simplest I have been able to find. Yours, &c. R. N.

The black dots stand for the numbers in the same line with them in the A column.



Otherwise—

A	b	c																		
A	d	e	B	d	f	C	d	g												
A	F	g	B	e	g	C	e	f												
A	H	i	B	H	J	C	H	K	D	h	l	E	h	m	F	h	n	G	h	o
A	J	K	B	i	K	C	i	J	D	i	m	E	i	L	F	i	o	G	i	n
A	L	m	B	L	n	C	L	o	D	J	n	E	J	o	F	J	L	G	J	m
A	n	o	B	m	o	C	m	n	D	K	o	E	K	n	F	K	m	G	K	L

The order of walking is then—

A B C	A D E	A F G	A H I	A J K	A L M	A N O
D I M	B L N	B M O	B E G	B D F	B H J	B I K
E J O	C H K	C I J	C L O	C M N	C E F	C D G
F H N	F I O	D H L	D J N	G H O	G I N	E H M
G K L	G J M	E K N	F K N	E I L	D K O	F J L

If the black dot formula is committed to memory, the puzzle can always be done nearly off-hand but, if not, a sure way of getting to the same result is to set down the 105 combinations of two letters from which the 35 combinations of three letters have to be formed side by side, and then amalgamate with *perfect regularity* from the top, cancelling all the used pairs.

A FOOD QUESTION.

[156]—If you think the following extracts have enough of general interest for your "Correspondence" columns, their insertion might lead to our getting some further useful information on the matter:—

In a recent number of *Chambers's Journal* (Nov. 12), under the head "Some Queer Dishes," it is said:—"In the West Indies, where meat becomes tainted very rapidly by the agencies of the damp motionless heat and abundance of insect life, it becomes necessary to cook it almost as soon as it is killed. To obviate the toughness which would result from this, it is wrapped in a large fleshy leaf, which has the curious property of softening the muscular fibre and rendering it tender. If left on too long, the juice permeates the meat and disintegrates it altogether, hastening its decay. I do not know the proper name of this leaf the black people call it "sangulo"—nor the nature of its action; there is a large tree in the market-place, near the King's Wharf, at St. Thomas."

This is probably the same plant or tree that is mentioned by Mr. H. Stonehewer Cooper, in his "Coral Lands," Vol. I., page 75. He says:—

"The one drawback to Fijian beef is that it is sometimes very tough, in consequence of being cooked the very day it is killed. The papau (*Carica papaya*) grows luxuriantly all over the Pacific, and according to Mr. Wittwach, a German naturalist, papau is the remedy. The juice of this is found to possess the property of rendering tough meat tender when boiled with it. If the unripe fruit be placed in the water in which the toughest meat is to be cooked, it is found to render it perfectly digestible, and the same results are observed if the meat be merely washed with the juice of the fruit. The thick, white, milky juice, when extracted from the unripe papau, in fact, contains properties similar to those of pepsine; and it is possible that it may be susceptible of chemical preservation, and become a valuable preparation. Tough meat is not unknown in England, and surely papau could be canned and sent over here, even if no mode of preservation is arrived at."

If this leaf or fruit has the virtues attributed to it, it is strange that we have not had it here in some form. "Coral Lands" was published last year.—Yours faithfully, F. C. M.

Nov. 28, 1881.

MAGIC SQUARES.

[157]—I am glad to see that your correspondent "H. S." has started the subject of magic squares in the last number of KNOWLEDGE; but though he gives examples of odd squares, he does not give the rules for making them. The subject is of great interest, and some squares are so very curious that I should like, if you will allow me, to bring it more fully before your readers.

Of magic squares there are two kinds: 1, those whose root is an odd number; 2, those whose root is an even number; the rules for the construction of each kind differing from each other.

The examples of odd squares given by "H. S." are made by Bachet's, or the Indian method; and the result is not capable of variation; but by the methods of M. Poignard and others, the resulting squares may be varied in many ways. I will now describe these methods for odd squares, and in a future paper will give the rules for squares with even roots and squares with borders, &c.

Bachet's Method.

Example: A square of 49 cells whose root is 7.

Fig. 1.

						1						
					8		2					
				15		9		3				
			22		16		10		4			
		29		23		17		11		5		
	36		30		24		18		12		6	
43		37		31		25		19		13		7
	44		38		32		26		20		14	
		45		39		33		27		21		
			46		40		34		28			
				47		41		35				
					48		42					
						49						

Fig. 2.

22	47	16	41	10	35	4
5	23	48	17	42	11	29
30	6	24	49	18	36	12
13	31	7	25	43	19	37
38	14	32	1	26	44	20
21	39	8	33	2	27	45
46	15	40	9	34	3	28

After having filled in Fig. 1 with the series, as shown above, proceed to transfer the figures in the cells outside the square into the

empty cells in the square directly opposite to them, as seen in Fig. 2—which will then be a magic square, whose root is 7—and the sum of the numbers in each vertical and horizontal band, and also of those on the two diagonals is 175.

Poignard's Method.

For a square whose root is 7.

In the square, Fig. 1, place in the top horizontal row the first seven numbers of the progression 1 to 7 in any order whatever, as 3, 7, 5, 1, 6, 4, 2. Then choose a number which is prime to the root 7, and which, when diminished by unity, does not measure it, say 3. Begin the second row with the third figure of the first. The third row with the third figure of the second, &c., and fill up the square.

Fig. 1.

3	7	5	1	6	4	2
5	1	6	4	2	3	7
6	4	2	3	7	5	1
2	3	7	5	1	6	4
7	5	1	6	4	2	3
1	6	4	2	3	7	5
4	2	3	7	5	1	6

In the top row of Fig. 2, place the multiples of the root 7, beginning with a cipher, viz., 0, 7, 14, 21, 28, 35, 42, in any order at pleasure; and fill up the square on the same principle as in Fig. 1, taking care not to assume the same number for varying the order of the figures. As 3 was taken for Fig. 1, we may take 4, or 5, or 6, for Fig. 2, say 4. Then the square will be filled up as in the margin. Now add the corresponding numbers in each cell of Figs. 1 and 2, and place the sum in Fig. 3, which will be a magic square.

Fig. 2.

28	7	42	0	14	35	21
0	14	35	21	28	7	42
21	28	7	42	0	14	35
42	0	14	35	21	28	7
35	21	28	7	42	0	14
7	42	0	14	35	21	28
14	35	21	28	7	42	0

It is easily seen that by this method the position of the numbers in the finished square may be varied greatly, and I leave it to the readers of Knowledge to say how many ways there are of varying the square of 7.

J. A. Miles.

Fig. 3.

31	14	47	1	20	39	23
5	15	41	25	30	10	49
27	32	9	45	7	19	36
44	3	21	40	22	34	11
42	26	29	13	46	2	17
8	48	4	16	38	28	33
18	37	24	35	12	43	6

INTELLIGENCE IN ANIMALS.

[158].—Referring to Mr. Henslow (p. 46, No. 3), is not his reasoning somewhat curious? He tells us that had the dog not been taught to ring the bell, his reasoning would have been abstract. As the fox was *not* taught to do what he did, why was not his reasoning abstract?

Another story is still more *apropos*. In this case the fox cut the line connecting the trigger with the bait, then went up deliberately (as seen by his track on the snow) and ate the latter. In this case the fox could not scrape a trench, so as to get safely at the bait, as there were only a few inches of snow on the ice where the bait lay.

In neither of these cases had the foxes been taught to do what they did, yet each in his own way used reasoning powers which enabled him to accomplish his object in the only way by which it could be safely attained by an individual fox (or man), ignorant of the mechanism of firearms. Was this abstract reasoning?

Mr. Henslow is rather hard upon brutes and boys. Every day we find what are supposed to be educated, reasonable, and, I presume, reasoning men and women, doing as silly things as the most stupid "boy or brute" could be guilty of."

Ask twenty persons what they would do if, whilst exposed to great cold, they found their faces freezing? Probably, nineteen of the twenty would reply, "rub with snow." This would be proper treatment if a person frozen was brought into a warm house, but is not right whilst he is exposed to the low temperature that is freezing him.

This snow application was improved upon by Major Burnaby, when, on his ride to Khiva, he thought his hands were freezing. He added brandy to the snow before rubbing it in, and thus made a sort of freezing mixture, as anyone can easily prove by trying it on some cold day, and exposing the part rubbed to the air. The brave lady who ascended Mont Blanc in winter adopted the same plan, and thought it successful. J. Rae.

[In a paragraph, which we have omitted, Mr. Rae appears to have misunderstood Mr. Henslow's remark about "pulling the string out of the line of fire." Mr. Henslow meant the fox being out of the line of fire while pulling, not the fox pulling the string away from the line of fire.—Ed.]

[159]—In favour of the lower animals possessing reasoning powers, I submit the following:

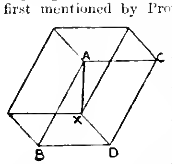
While living in Dublin, a few years ago, we had two female cats—one a "tabby," the other a "tortoiseshell." The tabby, my especial pet, was of rather fierce disposition, but an excellent mouser, and was in the habit of bringing her prey, generally alive, to show me before devouring it. The tortoiseshell—a feline beauty—passed the greater part of the day on the rug before the fire. One day "Tabby" brought no less than three mice, consecutively, into our sitting-room. The first two she ate; but the third, after playing with it for some time, she hid under a mat at the door, evidently meaning to reserve it till a hungrier moment. As soon as she left the room, the tortoiseshell, who had been sitting, as was her wont, with half-closed eyes before the fire, went to the mat, poked out the mouse, and gobbled it. This theft amused me so much that I resolved to watch the consequences. Tabby did not come in again till evening, when she proceeded instantly to fetch out her moose. She searched well under the mat, but not finding her *bonne bouche*, began growling ominously. At length her eyes fell upon the guilty tortoiseshell, upon whom she rushed, and beat and scratched her until in pity we rescued the culprit. Another time both cats were with kitten. The tortoiseshell disappeared for a few days, and returned looking very lean and miserable. Tabby, who had meanwhile brought forth her young, received her more amicably than usual, and after some rubbing of noses and purring, tho tortoiseshell took charge of the kittens, and suckled them. This was not the only time she was seen doing duty as wet-nurse, whilst the other was rambling somewhere round. A. F. R.

A BEAR'S MEMORY OF LANGUAGE.

[160]—The strategic shifts of Colonel Perkins' (Purtons'?) dog, Bully, at Bangalore, reminds me of another story connected with Bangalore. The late Rev. William Campbell, who was formerly a missionary there, on visiting Dublin, to advocate the claims of the London Missionary Society, went with some friends to see the Zoological Gardens in that city. While walking through the gardens, their attention was drawn to a particular den by the excitement of a small crowd before it. On approaching the place, they found it was the den of the Indian bear. Bruin was in a bad humour, and sat resolutely on his haunches, with his back to his visitors. Some tried to coax him with buns, others to startle him with shouts, and a few tried to stir him up with walking-sticks and umbrellas. All their efforts were in vain. At last it occurred to Mr. Campbell to address the bear in the language he was accustomed to in his youth, so he shouted "Cuddapah! Cuddapah!" (Get up! Get up!) To the surprise and amusement of the spectators, Bruin immediately turned round with a delighted grin.—Yours, &c. Cuddapah.

AN ILLUSION.—DURATION OF FLASH OF LIGHTNING.—RAINBOW.

[161]—Wheatstone calls attention to the following illusion first mentioned by Professor Necker, of Geneva. The rhomboid AX is drawn so that the solid angle A should be seen the nearest to the spectator, and the solid angle X the farthest from him. But in looking at the rhomboid, it will occasionally so change that the solid angle X will appear the nearest, and the solid angle A the farthest away. Necker attributed the alteration of appearance not to a mental operation, but to an involuntary change in the adjustment of the eye for obtaining distinct vision. Wheatstone, on the other hand, supposed the effect to depend entirely "on our mental contemplation of the figure intended to be represented, or of its converse."



I think Mr. Molloy (letter 116, p. 121) will find no account of any experiments of Wheatstone's on the duration of a flash of lightning in the collected volume of his papers issued by the Physical

Society of London. In the paper to which Mr. Molloy refers, "An Account of some Experiments to Measure the Velocity of Electricity and the Duration of Electric Light," Wheatstone gives an account of experiments made on the duration of the electric spark, as produced in the laboratory, and towards the conclusion of his paper he states that "the light of electricity in a state of high tension has a less duration than the millionth part of a second."

If "G. S. M." (query 67) could see a rainbow under perfect conditions, it would appear not semicircular, but circular. If, for example, he were up in a balloon, at a great height, with the sun overhead, and a rain-shower beneath, he would see a circular rainbow below, probably with the shadow of his car and its occupants fair in the centre. A rainbow being formed by those raindrops which are placed at a particular angle with regard to a line drawn from the sun through the head of the observer, it is apparent that as our balloon descended towards the earth the coloured ring would descend also, until finally it impinged on the earth's surface. Upon going lower still, the continuity of the ring would be broken, the earth having taken the place of some of the raindrops, and, probably by the time the observer had reached the ground, he would be only able to see a semicircular rainbow.

Wm. Ackroyd.

COMETS' TAILS.

[162] In reply to the query by F. C. S., p. 119, Mr. G. M. Seabrook, Temple Observatory, Rugby, writing to *Nature*, says:— "The comet (b. 1881) was examined spectroscopically here last night. The nucleus gave a bright, continuous spectrum, while the coma and brighter portions of the tail gave the three least refrangible hydrocarbon bands superposed on a faint continuous spectrum. On moving the slit of the spectroscope towards the fainter part of the tail the bands died out, leaving a faint continuous spectrum, which, again, gradually faded away as the end of the tail was approached. I have not measured the position of the bands, but they are sensibly the same as those from an alcohol flame."

Mr. Percy Smith, of the same Observatory, on July 1, writes:— "On the 27th (June), the bright jet extending from the nucleus was very plain, and gave the hydrocarbon spectrum very distinctly. No bands were seen in the tail, but only in the immediate proximity of the nucleus."

M. Wolf, in a paper read to the Paris Academy on July 11 (in regard to the same comet) says: ". . . "When the slit of the spectroscope is passed over the comet, starting from the head, one finds the three bands all round the nucleus at nearly the same distance from all the sides. They disappear in the tail properly so-called, the very pale spectrum of which seems to be continuous. Thus only the nebulosity surrounding the nucleus contains incandescent gases. The light of the tail comes to us from a pulverulent matter luminous or simply illuminated. Such are the data of spectroscopy."

A. T. C.

AN INSTANCE OF PRESSURE.

[163]—The readers of Knowledge have, doubtless, been interested in the scientific controversy on the subject of toads being enabled to live, enclosed by blocks of solid matter; probably the following fact will be unknown to many of them, and may afford additional proof of the frog's vital tenacity:—

A few years ago an experiment was made in the Arsenal at Woolwich, the particulars of which were published by the well-known Mr. Tegetmeier, of the *Field*. A frog was subjected to the enormous pressure of three tons to the square inch, for a period of thirty-six hours. At the end of the experiment the frog appeared prostrate, but recovered a perfectly normal condition in a few hours, and as a reward for his severe test, was taken back to his native marshes.

R. C. Fraser.

A geography of the almost unknown kingdom of Corea has been compiled by a member of the suite of the Japanese envoy to that country. Several valuable papers, containing accounts of travels in Corea, have been read before the Geographical Society of Tokio, and have appeared in its transactions. As they are written in Japanese, they are unfortunately all but inaccessible to European geographers. —*Frank Leslie's Magazine.*

The French Minister of Commerce has just issued a notice regarding the inspection of salted pork imported from abroad. Inspectors are to be appointed at the different ports to which the entry of this commodity will be confined. A course of lectures on the subject of micrography is to be instituted at the School of Pharmacy for the preparation of the experts who will be required for these posts. These measures foreshadow the eventual raising of the embargo on American pork.

Queries.

[130]—Known Languages.—I shall be glad if you, or any of the readers of Knowledge, can tell me the number of known languages spoken at the present day. J. A. L. R.

[131]—Creation.—Will any correspondent versed in this subject give his views as to what are the proofs of man having existed more than six thousand years?—R. I. P. [R. I. P. should read, amongst other works, "Quatrefages on Man," in the International Series, published by Kegan Paul & Co., London. Price 5s.—Ed.]

[132]—Fungi.—I shall be glad to have any information respecting the nature of fungi and truffles; how they are produced, and what their place is in the vegetable kingdom. Ignotus.

[133] Would you kindly inform me if you know of any works, articles in magazines, or anything whatever by the Rev. W. H. Dollinger, F.R.S.?—Querist.

[134]—The Stars in Northern and Southern Latitudes.—Would you oblige a boy by explaining the reason why we in northern latitudes do not see the whole of the southern constellations, as people in southern latitudes see all our northern constellations? Vide page 12 of "Easy Star Lessons." You state the fact, but do not give the reason. Quisquis.—[At the North Pole, the pole would be overhead, and the heavens turning around on upright or vertical axis, we should see only one half the star sphere, even if we watched through twenty-four hours of an arctic winter's day (really night). At the equator, the poles being on the horizon, we should see the whole star sphere, turning as it does round a horizontal axis, in the twelve hours of an equatorial night. In intermediate positions, more or less of the star-sphere would be seen the nearer the station to the equator. You must not confound latitudes south of ours with southern latitudes.—Ed.]

[135]—Cheap Telescope.—I am wishful to have a *cheap* telescope, such as would answer a beginner; say a $2\frac{1}{2}''$ or $3''$ object-lens. Would any reader give me a few hints as to construction, what would be probable cost, including tube, &c.?—R. I. P.

[136]—Sunlight on Fires.—Does bright sunlight interfere with ordinary combustion? If, as I believe, it does, I should be glad to have a scientific explanation of the fact. As evidence, let me quote the housemaid, who says she cannot get her fire to burn if the sun is shining upon it. And the smoker, who says that he cannot keep his cigar alight under the same circumstances.—N.

[137]—Humble Bees.—What success has attended the attempt to introduce humble bees (*Bombus terrestris*) into New Zealand, to fertilise the clover there? The first attempt was unsuccessful, but I have been unable to learn the fate of the second attempt.—Thomas Chandler.

[138]—The Gyroscope.—What particular laws of motion are illustrated by the gyroscope? Does it throw any light upon planetary motions? And what? Of what motions is the instrument capable, and can they be popularly explained? Is there any simple treatise on the subject? Can you give some easy papers upon it in Knowledge?—T. W. F. [Newton considered the movements of a rotating body under gravity among the most difficult problems which mathematics can deal with. We have tried elsewhere to make this difficult matter clear, and may try again soon in these pages. —Ed.]

[139]—Temperature of Interstellar Space.—What is the supposed temperature of interstellar space, and how has it been ascertained?—E. C. R.

[140]—Ice.—Does the volume of ice vary as other solids do with variation of temperature?—E. C. R.

[141]—Time of Glacial Epoch.—Is Dr. Croll's theory of the time at which the ice age existed in Britain probably correct? Are there other theories on the same subject?—E. C. R.

[142]—The Great Bear.—Will some reader of Knowledge inform me if it is possible to see the Great Bear $17\frac{1}{2}°$ south of the equator? I read in a book (and the author ought to have known) that it is so, but it seems hardly credible. Would some one state how far south it may be seen?—Marian. [Any star of the Great Bear can be seen just as many degrees south of the equator as that star is distant from the pole of the heavens.—Ed.]

The biggest thing yet in the way of plaster casts is the cast of a whale, taken at Provincetown by Mr. Palmer, modeller for the Smithsonian, at Washington. A papier-mâché fac-simile is to be moulded from the cast, the entire skeleton of the same whale is to be inserted, and the monster will be suspended in the museum. To show the arrangement of the skeleton, one side of the whale will be left open.

Replies to Queries.

[103]—HISTOLOGY.—Your correspondent may be glad to know of the following books on "Histology":—"1. Frey's, translated by Barker (expensive); 2. Rutherford's "Outlines of Practical Histology," is very good for students; 3. Klein's "Atlas" is a grand work on this subject, but too expensive.—B. TOM TINY.

[39]—MOON'S ROTATION.—I give a quotation from the lecture delivered by Dr. Ball, at the Midland Institute, Birmingham, Oct. 24, 1881:—"For many centuries it had been an enigma to astronomers why the moon should always turn the same face to the earth. It could be shown that there were many million chances to one in favour of this being due to some physical cause. The ordinary theory of gravitation failed to explain the cause. Every one had noticed this phenomenon, yet the explanation was never given till lately. It was Helmholtz who showed that this was a consequence of ancient tides, and this simple and most satisfactory explanation has been universally accepted."—A. T. C.

[88]—BRAIN TROUBLES.—The division of the brain into two parts will certainly not take in all the functions of the brain; at least one more must be added. The division into *a*, involuntary motion, *b*. centres of reasoning, higher emotions, &c., cuts out entirely the function of voluntary motion presided over by the brain. Also the function of the cerebellum, that of co-ordination of muscular actions, or muscular sense, is left out, and is certainly important; for a pigeon, having his cerebellum removed, cannot stand, but topples over, and although seeing a blow threatened, cannot avoid it. When lying down, it was not in a state of stupor.—P. H.

Notes on Art and Science.

AT Gnosso, in Crete, Professor Stilman has excavated the remains of what he believes to be the historical labyrinth famous from the story of Theseus and the Minotaur.

AT Honolulu, a signal station in communication with the United States Meteorological Bureau will be established on the volcano Kilanea, and a series of observations will be taken.

THE SUNDAY SOCIETY.—The twenty-sixth Sunday Art Exhibition of the society was opened on Sunday, Dec. 18, at the Hanover Gallery, from half-past one till four o'clock, when there was an attendance of 512, the admission being by ticket.

DR. SCHONDORFF has constructed a safety lamp which can only be opened with the help of a strong magnet. A lever presses against a toothed wheel, which allows or prevents the movement of the screw fastening the glass door-holder to the socket. The lever must be moved in order to open the lamp, and this is done by means of a strong magnet.

BRUGSCH PASHA has succeeded in deciphering the Ethiopic inscriptions of Meroe, the language of which resembles that of the Ethiopic *graffiti* on the walls of Philæ and other Nubian temples. He thinks that the language has some similarity to the pre-Semitic Sumerian dialect of Southern Babylonia, and quotes in support of this view words like *sher*, "King."

A MOST remarkable discovery has been made in the Sweetwater country, in Wyoming territory. It is a deposit of sulphuric acid in its natural state. The odour, chemical action, and general appearance of the stuff demonstrates it to be a pure quality of sulphuric acid. The ground is impregnated over a large area—100 acres or more—and parties have filed claims upon it.

A CONGRESS of experts has assembled at St. Petersburg to inquire into the evils caused by excessive drinking in Russia. By an overwhelming majority they have advised a diminution in the number of public-houses; while they also passed a resolution in favour of vesting in the communal authorities the right of opening liquor-shops under regulations to be determined by a sub-committee appointed for this purpose.—*Frank Leslie's Magazine.*

THE survey of Palestine east of the Jordan is proceeding rapidly under the superintendence of Lieut. Condor. When he last wrote, several hundreds of miles had been measured with accuracy, and a number of places having more or less modern names were identified as those mentioned under different titles in ancient history. He discovered a great many cromlechs, or flat stones, supported like a table by others set on end. Not less than fifty of these monuments were sketched in three days. Some of them had small chambers near them from 3 ft. to 5 ft. long, and 3 ft. high, excavated in detached cubes of rock 10 ft. to 15 ft. on each side. The interest in the work is increasing, and the result cannot fail to be of great archæological importance.—*Frank Leslie's Magazine.*

A SIMPLE ELECTRICAL MACHINE.—As a domestic electrical experiment, few are simpler or more demonstrative than that of first drying and warming a piece of paper, then smartly stroking it with india-rubber and placing it against a wall, to which it electrically adheres. Electric sparks may thus be obtained in the dark, and a variety of other experiments performed. When the wind is from the east, and dry, a small Leyden jar may be charged by using a long strip of paper, equal in width to the outer coating, and drawing this repeatedly, when excited, along the outside of the jar. An improvement on this simple electrical material has recently been made by Wiedemann. He takes Swedish filtering-paper (procurable wherever chemical apparatus is sold), steeps it in a mixture of equal volumes of nitric and sulphuric acid, then washes with abundance of water, and dries it—the same process as making gun-cotton, into which the fibres of the paper are thus converted. It is stated that with this gun-cotton paper, nearly all the stock experiments of the static electrical machine may be performed by laying a sheet of it on waxed paper for insulation, and rubbing it briskly. This was announced in the *Comptes rendus* of the French Academy about the beginning of the year, but I have heard no more of it since. As Christmas holidays are coming, I recommend it to my juvenile readers, who may possibly be able to improve upon the original suggestion by coating a fig-box, or other wooden cylinder, with a non-conducting surface of gutta-percha varnish, or shellac, or wax, then covering this with the prepared paper, and mounting it like an ordinary old-fashioned electrical machine; or by making an electrophorus of this material.—W. Mattieu Williams, in *Gentleman's Magazine.*

THE BURSTING OF WATER-PIPES.—In a country like England where the obstinate natives persist in the practice of burning their fuel in a hole made in the wall, with a shaft rising perpendicularly above it, in order that the greatest possible quantity of the heat of combustion shall be devoted to warming the clouds, and the smallest possible amount shall be radiated from only one side of the fire into the apartment, anything like a severe frost becomes a national calamity. Last winter, though far less severe than an average winter in Germany or the United States, is made miserably memorable by the domestic calamities connected with the bursting of water-pipes, and is recorded in the household accounts of expenditure for mending the same and repairing the damage done by the general house-and-furniture-soaking. If English houses were equally warmed *throughout*, as they are in other countries where domestic civilisation has made some progress, the freezing of any water-pipe inside would be impossible in any weather, and all outside water-conveyance can be made underground. But as the domestic fetish of the Englishman and Englishwoman, the hole-in-the-wall "cheerful" fireplace, must be worshipped: as the fire-worshippers must continue to scorch their noses while their backs are matriculating for lumbago; as the cheerfulness of the fetish must be maintained, and its devotees must demonstrate that cheerfulness by staring vacantly at the glowing coals which roast everything and everybody at one side of the room, while the rest of the house is at the mercy of the outside fluctuations of temperature; as all this must go on for a generation or so longer, in spite of Kyrle societies and smoke-abatement exhibitions—some adaptation of water-pipes to our existing domestic barbarism is very desirable. A very little geometry is required for understanding that if a pipe of circular section be flattened in any degree, its internal capacity must be proportionately lessened; and conversely, that a pipe thus flattened, or made of elliptical section, may have its internal capacity enlarged by simply squeezing it out towards the circular shape. Lead being flexible, a leaden pipe made of elliptical section and filled with freezing water will swell out towards circular shape, and thus allow room for the expanded ice without bursting. It is proposed that such pipes be made and used, and I think the idea an excellent one though plumbers are not likely to favour it, but their disapproval should be a strong recommendation to the householder who has to pay for mending ordinary pipes. I am told that a patent has been secured, but do not know by whom, and as I am going to suggest an infringement, he is entitled to any advertisement this note may afford. I recommend all householders to save their existing pipes by simply flattening them with a mallet, taking care to place behind the part which is struck a flat piece of wood, where the pipe rests upon rough brick-work. The freezing will simply reverse the work of the mallet, and lead of good quality will bear this double-bending. If freezing water were a rigid solid, the transverse expansion of the cylinder of ice within the tube would be proportionate to its diameter, and thus the elliptical form would be maintained; but freezing water is not a solid; it exerts an equal expansive pressure in all directions; and the walls of the pipe being equally pressed, give way in the direction of least resistance.—*Ibid.*

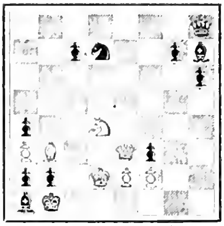
Our Chess Column.

In the *Illustrated London News* for Nov. 5 last, the following problem appeared:—

Problem, No. 6.

(By W. Grimshaw.)

Black.



White.

White to play and mate in three moves.

The solution, which appeared on Nov. 6, is—

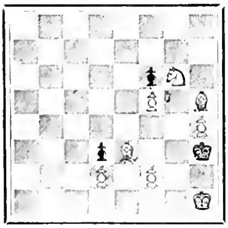
White.	Black.
1. Kt. to K.B 5.	2. B. takes Kt.
2. Q. to K.6.	3. Any move.
3. Q. or B. mates.	

The above problem, assuming this were the only solution, would be neat, but not particularly difficult. Its point, as thus solved, lies, not in the placing of the Knight on the Black Bishop's line of action, for that is an idea obvious enough; but in compelling the Black Bishop to occupy such a position that, when the White Queen goes to K.'s 6th, she will (though putting herself *en prise* by going there), threaten mate in two ways instead of one, as would be the case if she moved there at the outset.

There is, however, it so happens, a second solution, which involves a very pretty stratagem, one which hitherto we have not seen embodied in a problem. We leave our chess readers the next fortnight wherein to discover this second solution. They may suppose the other prevented by the addition of a Black Knight at Q.'s 8th, or simply that a second solution is required as a condition of the problem.

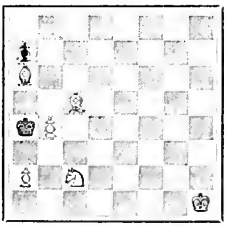
We mentioned last week a case in which a problem by the Editor (in chief) had been unmistakably anticipated, somewhat as a part of Mr. Baxter's idea in the problem of last week (No. 5, it should have been entitled) had been anticipated by the Editor. The case was on this wise. In the year 1858, the Editor sent several problems to the Chess Editor of the *Illustrated London News*, among which was the three-mover No. 7. The Chess Editor

Problem 7. By the Editor.



White to play and mate in three moves.

Problem 8. By D'Orville.



White to play and mate in three moves.

of the *Illustrated* remarked that he had seen something like this problem in the collection of D'Orville and Kling, and on the Editor writing that he had not seen that collection, the following pleasant remark appeared in the *Illustrated London News* for Oct. 9, 1858:—"R. A. P., St. John's College, Cambridge.—The following is the position by D'Orville, to which we referred [see Position 8 above]; we leave you to judge whether the resemblance is accidental." Mr. Staunton paid no attention to a disclaimer by the Editor of all knowledge of the position in question. We leave both problems as an exercise to our chess readers, leaving them to judge whether the resemblance is quite so close as to justify the remark above quoted, which followed (be it observed) a statement that the author of the later problem knew nothing of the collection by D'Orville and Kling. Strangely enough, the Editor's problem was published later (in August, 1859) in the chess column of the *Illustrated*. The Editor considers his problem so far superior to D'Orville's as to have justified him in claiming it as his own, even if he had known D'Orville's. But let others decide on that point. What the Editor would chiefly call attention to is that an experience such as his in this matter, makes him far readier than he might otherwise perhaps have been to see that even a close apparent resemblance in chess problems does not necessarily prove that there has been any borrowing.

The problem sent us by Mr. G. B. Stubbs is not quite up to publication standard. It is scarcely ever permissible for a three-mover to begin with a check, or capture (though a few instances are known of really strong problems so opening); but in the case of a two-mover like that by Mr. Stubbs, a check at the first move is quite inadmissible. There is a flaw still more serious in the existence of a dual solution, which, as Black has only one move, is a rather more serious matter than duals affecting only White's reply to moves by Black, which are not defensive. Thus there are duals in the Editor's Problem No. 4. If Black moves his Queen to Q.R.7, or 8, or to Q.B.8, or makes other purposeless moves, White can mate by moving his Rook from Q.5 to any free square; but that Black should have but one possible move, and White then be able to mate in more ways than one, is, of course, a fatal flaw in a two-mover.

Our Whist Column.

By "Five of Clubs."

When, having the original or first lead, we are obliged to lead from a short suit, as in the case considered in No. 4, p. 83, or when we have four trumps not very strong, and three of each of the other suits, we should, in general, select that suit which is least likely to injure our partner or to benefit the adversary. Of course, if your best short suit is very strong, as ace, king, queen, ace, queen, knave, ace, king, knave, or the like, you lead as from strength. Again, if you have to lead from ace, or king, or queen, and two small ones, you lead the smallest, so as not to throw away the command of the suit. You suggest, indeed, to your partner that you have led from numerical strength; but that is the misfortune of your position. It is better to do that than to give up the command in what may be a strong suit of one of the adversaries. When you have knave and two small ones, you should lead knave; because the card cannot help you against strength held by the adversary, and if your partner is strong it may help him.

But your best way of helping your partner, when you are obliged to lead from a short suit originally, is to play from a suit in which you have a strong sequence—such as queen, knave, ten; or queen, knave, and another; or knave, ten, and another. By leading the highest from such a sequence, you help your partner, if he is strong in the suit, without materially weakening yourself, if the enemy should be strong in it. Next to such hands come hands in which you have two honours and a small one. The proper leads from three-card suits, as well as from long suits, will be considered in detail later.

It can scarcely ever be advisable, no matter how your hand is constituted, to lead from ace, king, or queen, and one other. To lead from ace king, or king queen, or queen knave, alone may, in certain cases, be better than leading from a weak three-card suit. But in most cases of that kind it is better to lead from your four-card trump suit, even though it be weak.

In considering thus far the lead from a suit of three cards, we have dealt with the original lead. If you have not the original lead, then, even though only a single round has been played, you can generally form some idea of the suit you should select from among three weak non-trump suits. Thus, if your partner has led, and you have taken the trick, you should of course return his lead. Leading any other suit would imply that you had considerable strength in that suit.

If you are fourth in hand, you know at least one suit which you should not lead—viz., the one opened by your left-hand adversary. Unless, indeed, the fall of the cards in the first round showed that

he is not very strong in that suit, in which case, by leading through him, you put him at a disadvantage. Many players seem to think that the excellent general rule, lead *through* strength (that is, lead a suit in which your left-hand adversary has high cards), is a rule to be universally followed when you have no good suit of your own, and do not know which is your partner's best suit. But if your left-hand adversary leads from a suit both strong and long, and you, making first trick, lead through him in that suit, you are simply playing his game. Of the other two suits (outside trumps), you select that which you can lead with least chance of aiding the adversaries, and, as a rule, you play the best of the suit. It is an even chance that your partner is strong in it.

If you are second player, and take the first trick, you can hardly go wrong. Leading the suit your right-hand adversary had led would be doubly disadvantageous: you would be probably leading up to strength, and certainly helping to establish his suit. Of the other two suits, outside trumps, you select the best, and play the card most likely to help your partner. If you have thus led from a suit in which your left-hand adversary is strong, you at any rate lead through his strength. If your high card makes, and you then play a low one, your partner knows you have led from a short suit (or that you have made a forced lead), and infers that either you have four trumps, and no other four-card suit, or that your only four-card suit is very weak. His own hand will help to show which of the two explanations is the more probable.

With a five-card suit, however small the individual cards, it is scarcely ever wrong to lead from the long suit. Cavendish, in an amusing story in his "Card Essays," gives it as his opinion that the long suit should be led from, even with such a hand as this:—Ace, King, Queen of Spades; Eight of Clubs; Ace, King, Queen, and Three of Diamonds (trumps); Nine, Eight, Six, Four, Three of Hearts.

A YARBOROUGH HAND AT WHIST.

At the close of the letter to which we referred last week, "H. P. H." suggests a plan far calculating the odds that there will not be a Yarborough at a given deal. He says: "We must find the number of ways in which the pack must be dealt so as not to include a Yarborough. Suppose, for instance, one hand contains one of the high cards, another three of them, the third six, and the fourth ten. This arrangement may happen in

$$\frac{20 \;.\; 19 \;.\; 18 \;.\; 17 \;.\; 16 \;.\; 15 \;.\; 14 \;.\; 13 \;.\; 12 \;.\; 11}{2 \;.\; 3 \;.\; 6 \;.\; 5 \;.\; 4 \;.\; 3 \;.\; 2} \text{ ways.*}$$

By taking all such arrangements as these, and adding the number of ways, we get the number of different arrangements of the whole pack which will not include a Yarborough; the ratio of this number to the number of different arrangements of the pack, namely, $\frac{52}{(13)^4}$ is the chance against a Yarborough happening. This would give a long piece of work, but, perhaps, some of your readers may find a short method." H. P. H.

* The reasoning here is unsound. We leave the problem as an exercise for our readers (not proposing, however, to publish all solutions which may be sent to us).

Our Mathematical Column.

MATHEMATICAL QUERIES.

[8]—Are there any exact solutions of the equations:—

$$\frac{1+\sqrt{1-x}}{1+\sqrt{1+x}}=\sqrt{1-x^2} \text{ and } \frac{1+\sqrt{1+x}}{1+\sqrt{1-x}}=\sqrt{1-x^2}.$$

[The equations are really the same, so far as any difficulty in their solution is concerned, for they differ only in the sign of x, so that whatever root we find for one, the same quantity, taken negatively, is a root of the other. But it will be found on trial that 0 is the only value of x which satisfies either equation. The solution may run thus (taking first equation):—

$$\begin{array}{lll}
 & 1+\sqrt{1-x}=\sqrt{1-x^2}+(1+x)\sqrt{1-x} & \\
 & 1=\sqrt{1-x}(\sqrt{1+x}+x) & \text{(A)} \\
\text{But} & 1=(\sqrt{1+x}+x)(\sqrt{1+x}-x) & \text{(B)} \\
\text{Whence} & \sqrt{1-x}=\sqrt{1+x}-x & \text{(C)} \\
\text{and} & 2-2\sqrt{1-x^2}=x^2 & \\
\textit{i.e.} & 1-x^2-2\sqrt{1-x^2}+1=0 & \\
 & \sqrt{1-x^2}=1 & \\
\text{or} & x=0. &
\end{array}$$

It might seem that since, after obtaining (A) and (B), which give

$$(\sqrt{1+x}+x)(\sqrt{1+x}-x)=\sqrt{1-x}(\sqrt{1+x}+x)$$

we divide by $\sqrt{1+x}+x$ to get x, the equation should be satisfied by the roots of

$$\sqrt{1-x}+x=0$$

which are

$$\frac{1\pm\sqrt{5}}{2}$$

but this is not the case. Neither root will satisfy the original equation, whatever signs we give the quantities $\sqrt{1-x}$ and $\sqrt{1+x}$.—Ed.]

[9]—Arithmetical Problem.—Would any reader of Knowledge favour me with a solution of the following problem:—If twelve horses eat ten acres of grass in sixteen weeks, and eighteen horses eat ten acres in eight weeks, how many horses will eat forty acres in six weeks? The grass is supposed to grow uniformly.—G. H. Mapleton.

[10]—On base BC are triangles BAC, BDC, having equal perimeters, AB being equal to AC. If AC BD intersect in O, show that $AO>DO$. Mathematicus.—[Like most problems of the kind this is best dealt with indirectly. Thus, take at point G in OA (produced if necessary) such that $OG=OD$, and from OB the greater, cut off OF, equal to OC the less, and join FG. Then obviously

$$\begin{array}{ll}
 & FG=DC \text{ and } GC=FD \\
\text{Hence} & FG+GC=FD+DC \\
\text{But} & BF+FG+GC=BD+DC=BA+AC \\
 & \quad <BF+FA+AC \\
 & \therefore\ FG+GC<FA+AC \\
 & \text{or } FG+GO<FA+AO
\end{array}$$

whence it follows that G must lie between O and A. For if G were at A, $FG+GO$ would be the same as $FA+AO$; and if G were in OA produced $FG+GO$ would be greater than $FA+AO$. Therefore AO is greater than GO, that is, than DO.—Ed.]

[11]—Perimeter of Inscribed Triangles.—Show that the perimeter of an equilateral triangle inscribed in a circle is greater than the perimeter of any other isosceles triangle inscribed in the same circle.—Mathematicus.

[Prove as follows:—Let ABC be the greatest triangle with a given perimeter. Then, if it be not equilateral, there must be, at least, two sides not equal to each other. Let AB and BC be unequal. Through B draw KBL parallel to base AC, draw CM perpendicular to KL, and produce CM to N, making $MN=NC$. Join AN, cutting KL in D, and join DC, BN. Then $BN=BC$, and therefore $AB+BC=AB+BN>AN>AD+DC$, so that there must be same point P in DN, such that if PC be joined $AP+PC=AB+BC$, or there is a triangle greater than ABC, and having the same perimeter, contrary to our supposition. Hence, no two sides of the greatest triangle with the given perimeter can be unequal, or the triangle is equiangular.—Ed.]

[12]—I wish to prove that

$$\frac{2n\,(2n-1)\,(2n-2)\cdots(n+1)}{1\cdot 2\cdot 3\cdots n}.$$

The middle term in the expansion of 2^{2n} is equal to

$$1^2+n^2+\left(\frac{n\,(n-1)}{1\cdot 2}\right)^2+a\quad+n^2+1^2.$$

the sum of the squares of terms of the expansion of 2^n.

Proposing to do this by mathematical induction, I find, calling the first expression Σ, that when in it I write $\overline{n+1}$ for n, it becomes

$$\Sigma\left(1-\frac{n^2}{(n+1)^2}\right)$$

Can I, at this point, without more ado, conclude that since

$$\Sigma=1^2+n^2+a+n^2+1$$

(by hypothesis) when n becomes $\overline{n+1}$ that Σ will become

$$1^2+\overline{n+1}^2+\left(\frac{(n+1)n}{1\cdot 2}\right)^2+a+\overline{n+1}^2+1^2$$

which would prove what I want?—F. B.

[Your demonstration is beyond us. If you write $(n+1)$ in the first expression, calling it Σ before the change, it becomes—

$$\Sigma\left(\frac{2(2n+1)}{(n+1)}\right)$$

The best way to solve your problem is this:—

$$(1+x)^n=1+C_1x+C_2x^2+\&c.+C_2x^{n-2}+C_1x^{n-1}+x^n$$
$$(x+1)^n=x^n+C_1x^{n-1}+C_2x^{n-2}+\&c.+C_2x^2+C_1x+1$$

where C_1, C_2, C_3 are the well-known co-efficients, in the expansion of the triunial $(1+x)^n$. Multiplying—

$$\begin{array}{l}
(1+x)^{2n}=1+(C_1+1)x+(2C_2+C_1^2)x^2+\&c. \\
\quad+(1+C_1^2+C_2^2+C_3^2+\ldots\ldots\ldots+C_2^2+C_1^2+1)x^n \\
\quad+\&c. \qquad\qquad +(1+C_1)x^{2n-1}+x^{2n}
\end{array}$$

Hence, equating the coefficients of x^n, we have—

$$\frac{2n(2n-1)(2n-2)\ \ldots\ (n+1)}{1\cdot 2\cdot 3\ \ldots\ n} = 1^2 + \ldots$$

$$\left(\frac{n(n-1)}{1\cdot 2}\right)^2 + \ldots + n^2 + 1$$

[13]—Dividing an Ellipse. I wish to know how to divide an ellipse (1) that the radius vector may cut off equal parts of the periphery, and (2) that it may divide the ellipse into equal areas. J. A. L. R. [The latter problem is solved in my "Geometry of Cycloids," section vii. The construction for a given ellipse is sufficiently simple and can be supplied if "J. A. L. R." particularly requires it. The former problem cannot be solved by any geometrical method of construction.—Ed.

Answers to Correspondents.

⁎⁎ All communications for the Editor requiring early attention should reach the Office on or before the Saturday preceding the current issue of Knowledge, *the increasing circulation of which compels us to go to press early in the week.*

Hints to Correspondents.—1. *No questions asking for scientific information can be answered through the post.* 2. *Letters sent to the Editor for correspondents cannot be forwarded; nor can the names or addresses of correspondents be given in answer to private inquiries.* 3. *No queries or replies savouring of the nature of advertisements can be inserted.* 4. *Letters, queries, and replies are inserted, unless contrary to Rule 3, free of charge.* 5. *Correspondents should write on one side only of the paper, and put drawings on a separate leaf.* 6. *Each letter, query, or reply should have a title, and in replying to letters or queries, reference should be made to the number of letter or query, the page on which it appears, and its title.*

Newton Crosland. That would be your better course. It would please you, and everyone would understand why your letters could not appear here.—J. Belcher. Whewell's "Plurality of Worlds" is now very much out of date. It is full of suggestive matter, but even when Whewell wrote it the work was not regarded as of much scientific weight.—N. Railway noise next week; thanks.—M. E. Pendred. Having made no "certain statement against a certain body," nobody, certain or uncertain, has a right to space here in reply. You simply went out of your way to drag in the very subjects we had decided not to admit. The article you think we should not have published was practically a part of our programme. You go on to say that you would like to send us a letter anent the article on suicide, anent which, you ask us "this plain, straightforward, question: Is it or is it not our intention to publish correspondence anent any social question?" We answer, Not about any social questions, but about some such questions we should admit correspondence. Suicide is not an inviting subject by any means—though a review of a work recently published, on the statistics of suicide, suited our pages well enough. Whether any letter you might send on that subject appeared or not would depend on what you said in it.—J. M. Brown. Thanks for your kindly expressions about our chess. We were absolutely obliged to omit the column the week before last.—Young Stager. If phrenology has no absurdities, then "none have we offended," for we objected only to its absurdities. Thanks for the illusion, which has been submitted to Mr. Foster. We do not know who wrote the elegant lines you quote.—G. F. J. Thanks, your letter shall appear before long.—E. C. R. asks where an explanation has been offered of his query, 13, p. 60. He has searched in vain through Knowledge, he says, for any such explanation. If he will turn to query 13, p. 60, he will see, over the initials E. C. R., the explanation referred to.—Marian. "Oh, no; we never mention him" (in these columns).—A. B. Phrenology has been fairly tested, and the discrepancies between theory and observation are found to be far too numerous to be overlooked. There have been marked cases of agreement, of course, or the theory would never have had any adherents. Our readers would not care, we believe, for the article you suggest.—Ben Trovato. We understand: correspondents are to be wholly neglected, or they are to be left only a page or two of the smallest print, and we are to fill Knowledge with articles by "able individuals," preferably perhaps enlarging each number to twice the present size. We should displease correspondents, and each number would involve a heavy loss. But these are mere details. Ben Trovato "prefers such articles to the uncertain lucubrations of unknown individuals."—Jas. Green. Thanks. The illusion you illustrate had already been sent by Mr. Hodd, and before that Mr. Foster had had it in his collection. It shall appear soon, but *not*, as you suggest, at the back of a star map.—Chief Librarian. Thanks, but we are too modest to present Knowledge as published. We wait returningly till sought for.—J. E. Gore. We will try to find room for your note on the star in Cassiopeia. Thanks.—J. A. L. R. We know beforehand that no one can tell you the number of known languages; still we insert your query.—Jas. Green. Do not you think it would be better to study the theory of evolution a little before attacking it? If you had, you would know that the theory, as now understood, involves, as an absolute necessity, the "survival of the fittest" *only*. You ask why there are not, at the present moment, examples of every gradation from one extreme to the other (from monads to monkeys, and thence to men), this being precisely the question which you would refrain from asking if you had studied even superficially the views of Darwin, Wallace, Haeckel, Spencer, and the rest. They show that the fittest survive, and you ask, Where are the others? Understand, we are not saying that they of necessity are right, and believers in the doctrine of multiplied special creations of necessity wrong. We are not urging our own or any man's opinion against yours. We are simply saying that you do not as yet know what the doctrine is which you are attacking. If you did, you would not find the points you mention militating against it.—R. C. Thanks; but that particular solution of the magic square problem has been sufficiently discussed in letters received during the last four weeks.—Mrs. Dr. Kingford. The reviewer of the "Perfect Way in Diet" should have written Mrs. rather than Miss; but so long as our present manner of speaking English remains unchanged, "Dr. Anna Kingsford" will not be regarded as a legitimate expression. A gentleman may recognise to the full a lady's professional skill, and her title to a diploma, but he can no more address her as "Doctor" or "Professor," without smiling, than he could gravely address her as "sir." Even in America, where lady professors, doctors, &c., are more numerous than here, this is found to be the case. Miss Mitchell, for instance, at Vassar College, knows more astronomy than many professors of the science in her own country and this; she is also distinctly entitled to be called Professor, and she prefers to be so called. But no man has ever yet been able to address her in a perfectly natural manner, and the young ladies who attend her classes, though they try hard to do so, most certainly fail.—H. Fitz Hart. Ask yourself the usual question which science puts in such cases. Is there any possible relation of cause and effect in this case? And you will at once see that the hazel twig must have been held by someone who knew where water springs were. The trick is as old as the hills whence water-springs come.—W. A. We agree with you that there is a clear distinction between "I can but think" and "I cannot but think."—Bee. Some parts of the brain have been injured without any noticeable change, but certainly it is not "strictly true," as a general statement, that injury to the brain is not followed by any "noticeable injury."—Cumarrah. Thanks for interesting story, and for poetry, but we could not well offer a prize of £100 for a poem on knowledge.—F. Jukes. Propose to have some papers on the Arabic names of the stars shortly in Knowledge.—J. W. Chester. It is, we believe, out of print.—W. Kidd. Your solution is not correct. Will give the solution in the next number. The problem is not so simple as you seem to think. As an exercise try this. A body projected vertically upwards will not fall to the east, but to the west of the point whence it was projected; moreover, it will be travelling westwards not only when ascending, but when descending also.

This week being the Christmas holidays necessitates some slight changes of arrangement. We take the opportunity to publish matter which has been lying over for several weeks. More recent correspondents will understand the delay.

Later.—Christmas holidays, and in particular Bank Monday, cause sometimes rather inconvenient delays, increased by postal delays due to multitudinous Christmas greetings. We are prevented this week from giving our paper on the Pyramid, and a paper on the Telescope, by a Fellow of the Royal Astronomical Society, because the illustrations for both these papers were not in hand in good time. However, so far as the latter paper is concerned, it is fitting that it should appear in the first number of the New Year, in which also will be the first of our monthly Star Maps, in which some features have been introduced which, we believe, our readers will regard as decided improvements.

The Protective Effect of Vaccination. Dr. Henry Tomkins, medical superintendent of the fever hospital belonging to the Manchester Royal Infirmary at Monsall, in a paper which he read recently at Owens College, said: "The most striking of all evidence is, perhaps, that derived from the small-pox hospitals themselves. Here the protective influence of vaccination is seen and proved in a manner beyond all cavil. At Highgate, during an experience of forty years, no nurse or servant, having been re-vaccinated, has ever contracted the disease, and evidence of the same character I can myself bring forward, for during the whole time that I have had charge of the fever hospital, more than a thousand cases of small-pox have passed under my care, yet no servant, nurse, porter or other person engaged there has, after re-vaccination, ever taken it, though exposed daily to infection in its most concentrated form. One woman, a laundress, who escaped vaccination, took the disease and died; one nurse, who some years before had suffered from small-pox, and was then considered protected, had a very mild attack; and this summer a workman, who did not live on the premises, but came in to work as a painter, was not vaccinated, and had rather a severe attack; and still more recently a servant, who by an oversight was allowed to go about her work three days before being vaccinated, had, before the latter had run its course, a slight abortive attack. Again, among all the students who during the past two years have attended the hospital for clinical instruction, not one has suffered, all having been re-vaccinated before being permitted to enter the small-pox wards. And in their case the false argument which opponents of vaccination have brought forward to explain the immunity enjoyed by nurses and others in attendance on the sick, viz., that constant intercourse and exposure to infection renders them proof against it by the system becoming inured to the poison, cannot be applied, as these gentlemen attend the hospital only a few hours once a week. I defy the most enthusiastic or conscientious of anti-vaccinators to produce evidence like this on his side of the question, or to bring forward even half-a-dozen persons, choose them whence he may, who have not been protected against small-pox, and expose them as the students are exposed, without more or less of the number taking the disease. Facts such as these should convert the most ardent anti-vaccinator from his folly, and convince him that a weapon of defence so powerful as vaccination should not be left to the pleasure of the individual, but that the State has the right and duty to look after its most thorough performance." *Lancet.*

Contents of Knowledge *No. 8.*

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CONTENTS OF No. 10.

THE GREAT PYRAMID.

BY THE EDITOR.

IT seems to me a misfortune that the researches made by Professor Piazzi Smyth into the proportions, position, &c., of the Great Pyramid, should have their value and interest impaired by being associated with wild, visionary theories. As De Morgan said long since, Smyth's views are "paradox of a very high order, backed by a great quantity of useful labour, the results of which will be made available by those who do not receive the paradoxes."

But it has been said (the question has been asked me repeatedly, not only in this country, but in America and Australasia), how can the numerous coincidences which Professor Smyth has shown to exist between pyramid features and the most advanced astronomy of our day be explained without supposing that the builders of the Pyramid were acquainted with a number of astronomical facts, which yet could hardly have come to their knowledge unless they were divinely inspired? Or, if some higher civilisation existed before the building of the Pyramid, and the facts in question were discovered as they have been re-discovered by modern astronomers, how is it that we have no traces of such civilisation older than the Great Pyramid itself? To these questions another has been added, especially for my own benefit, viz., this:—How can the great number of the coincidences be regarded as an argument against their significance? How can they be said to prove too much?

It appears to me that if we carefully study what the features of the Great Pyramid may be regarded as certainly proving, we shall readily distinguish the difference between the wrong and the right way of using the argument from coincidence.

We find first, in all the Egyptian Pyramids, the evidence of an astronomical plan; and in the Great Pyramid we find evidence that such a plan was carried out with great skill, and with an attention to points of detail which shows that, for some reason or other, the edifice was required to be most carefully built in a special astronomical position. It matters little at this stage of the inquiry whether we suppose the Pyramid was erected for astronomical observation or not. It was certainly constructed in accordance with astronomical observations of great accuracy, and conducted with great skill. Moreover, it is obvious that to obtain such accuracy, the building was made to serve, while it was being built, the purpose of an astronomical observatory. Just as the astronomer in our own time uses the instrument he is setting up to adjust and make exact the position of the masonry on which it stands, so the builders of the Great Pyramid used the passages which they made within it to determine, with the greatest accuracy attainable by them, the proper position of each part of it, up to the so-called King's Chamber, at least, and probably higher.

So much is certain. Every feature thus far discovered in the Great Pyramid corresponds with this theory, and some features can be explained on no other.

I have shown at some length elsewhere—but the matter scarcely needs demonstration—that the only possible way in which the Pyramid could have been oriented so accurately as it has been, was by stellar observations. Of all observations for *that* purpose, those made on the Pole-star of the time would have been the most effective. If there is a star which the astronomer observes less than another when using his observatory for that chief of all purposes to which a great public observatory, at any rate, can be applied, it is the Pole-star, simply because that star moves so slowly round its small circle. But for determining the direction of the true north point (and also for determining latitude) the Pole-star is invaluable. No astronomer who thinks over the problem at all, can fail to see that the builder of the Great Pyramid would have been driven by the requirements of his case to make just such a slant descending passage as that which opens out (now that the casing-stones have been removed) on the northern side of the Pyramid, not far above its base. It is equally certain that such a descending passage would have been directed to the position of the Pole-star when it was due north and at its lowest. The position of the Pole-star when exactly above the pole would have been just as well suited for determining the direction of the true north, but the slant passage would have had to run deeper down into the solid rock to give the same degree of accuracy, and the extra labour would have been wasted.

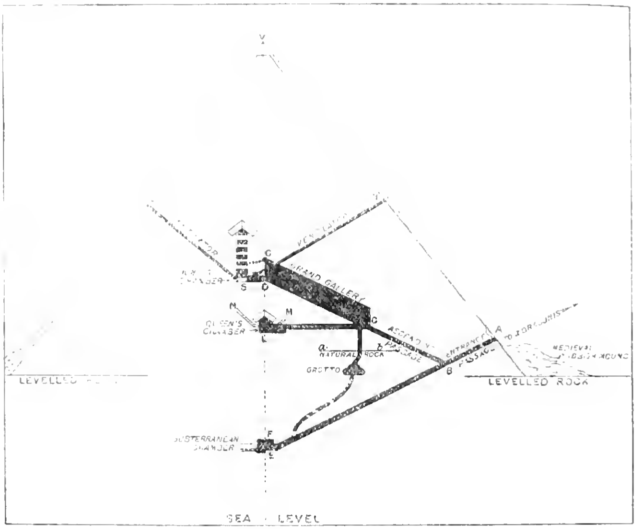
When, after marking the position of the base, the question of obtaining the true level came to be considered, only one method effective enough to give the required accuracy would have been available—viz., the use of water, flooding the squared space cut out in the solid rock. A difficult and costly task, doubtless, in itself, but a mere nothing considered with reference to the labour and cost to which the builders were prepared to go. For this purpose, the descending passage would have to be temporarily plugged; and as soon as the water-level had been marked at several stations on each side of the base, the plug could be removed, and the water run off into the pit which had been excavated underneath. A depth of a few inches of water all over the base would have sufficed for this purpose, but more probably a mere channel all round the base was prepared.

After thus orienting the base by aid of the Pole-star, and levelling it by using a property of liquids which was, of course, well known to them, the architects would place layer after layer, carrying towards the north the passage for observing the Pole-star, so that as each layer was placed, the work of orienting, and possibly of levelling, might be repeated, and an ever-increasing exactitude secured.

But they would know that ere long the direct pole-star observations would fail them; for the passage would pre-

sently reach the northern face of the Pyramid. By again using a well known property of liquids, however, combined with a well known property of light rays, they would continue the process of orienting to a much greater height. (When I say well-known, I mean well-known to them: they were manifestly skilful engineers and architects, and as surely as they were well acquainted with the properties of matter, so surely must they have been acquainted with the mathematical relations on which the simpler optical laws depend. Possibly they knew laws more recondite; but the simpler laws they certainly knew.) Now, the plan which would quickly suggest itself to anyone knowing these laws, would be to make use of the reflected rays from a star when the direct rays could no longer be employed. We know that when a ray from a luminous object is reflected at a plane surface, the reflected ray and the incident ray make equal angles with a line perpendicular to the surface at the point of incidence, and are also both in the same plane with that perpendicular. Now, what the Pyramid architects wanted was to have a constant means of determining the direction of north and south—in other words, a constant knowledge of the position of what modern astronomers call the plane of the meridian. They had this so long as they could observe the Pole-star when due north, through a passage opening out within the square layer they were adding to the Pyramid. When, as their work continued, this passage opened out in the part of the sloping side already completed, they could still detrmine the meridian plane if they carried up a passage through the masonry in such a direction as to contain the rays from the Pole-star after reflection at a horizontal surface, such as that of still water. For a perpendicular to the surface of still water is directed to the zenith, and the direct and reflected rays from the star (due north) lie, therefore, in the meridian plane which passes through the north and south points and through the zenith.

Now this is precisely what the Pyramid builders seem to have done, as the accompanying figure, the dimensions of which are taken from Smyth's book, "Our Inheritance in the Great Pyramid," shows. AE is the long, slant passage, which for convenience we may call the descending passage, BC is an ascending passage of exactly the same character, which, therefore, we might have presumed was intended for a similar purpose, even if the consideration of the natural course which intelligent builders would have pursued had not led us to expect to find precisely such an ascending passage here. But it may be asked how the reflected rays from the star were obtained? Nothing could have been simpler. The very same process which had been applied in levelling would be all that would be needed here. If the descending passage were for a time (a day, or even an hour would suffice) plugged at B, and water poured in so as to partially fill the angle thus formed at B, the surface of that water would reflect the rays of Alpha Draconis up the ascending passage BC. The direction for the south line thus indicated could be marked, and then the plug left to slide down to the subterranean chamber.



Once a year (supposing one layer of stones added each year, as Lepsius surmises) would have sufficed for this operation.

Not only do we thus find a natural and perfect explanation of the circumstance (hitherto unexplained) that the ascending passage is inclined at the same angle to the horizon as the descending passage, but precisely as we might expect from a true theory, we find that other points of difficulty have here their explanation. It is obvious that at B the casing stones of the descending passage would have to be very closely set and carefully cemented, so that the water used, year after year, in obtaining the reflected rays, might not percolate through and do mischief. Now, just here, we find the stones of the descending passage arranged with greater precision and made of better material. "Why," says Smyth—who notices everything, but seems always to insist on some forced explanation—"why did the builders change the rectangular joint at that point, and execute such unusual angle as they chose in place of it,

in a better material of stone than elsewhere, and yet with so little desire to call general attention to it, that they made the joints fine and close to that degree that they escaped the attention of all men until 1865 A.D. !" "The answer came from the diagonal joints themselves, in discovering that the stone between them was opposite to the butt end of the portcullis of the first ascending passage, or to the hole whence the prismatic stone of concealment through 3,000 years, had dropped out almost before Al Manoun's eyes. Here, therefore, was a secret sign in the pavement of the entrance passage, appreciable only to a careful eye and a measurement by angle, but made in such hard material that it was evidently intended to last to the end of human time with the Great Pyramid, and has done so thus far." In other words, the stones were thus carefully fitted that they might be a sign to Prof. Piazzi Smyth and the pyramidalists in 1865, just as the descending and ascending passages were all to be signs. It may show great want of taste to say that all these features indicate the builder's plan, and were in no sort intended for the benefit of remote generations of men belonging to an alien race; but it seems a long way more natural.

At any rate, it is certain that men having no knowledge of the telescope, and no means of securing accuracy of direction as our astronomers do by *magnifying*, would have adopted precisely such plans as thus far seem most clearly indicated in the Pyramid structure, making long passages in solid materials, and where necessary, changing the lines of sight by simple reflection. When we consider that this would be their natural course, and that even minute details of structure (some hitherto unexplained) correspond with the theory that they adopted this course, the conclusion seems fair that the theory is a sound one. Of course, it cannot be acceptable to pyramidalists, who prefer to believe that the labours of the Pyramid builders were directed by architects knowing all that is now known in science, and more; but we are, at least, saved from the incongruity of assuming that these wonderously-gifted architects were idiotic enough to adopt the blundering plan assigned to them—hiding away for preservation their sacred symbolisms and prophetic teachings, in a building so constructed that its interior could only be reached by being forcibly broken into, and would as a matter of fact be never properly measured until it had lost in great part the perfection of form on which its value for the supposed purpose depended.

This will appear still more clearly when we consider the great gallery, which to the astronomer is the most obviously astronomical part of the building, but to the pyramidalist is a sort of "Zadkiel's Almanac" in stone.

FOUND LINKS.

By Dr. Andrew Wilson, F.L.S., &c.

PART I.

As the question of "Missing Links" appears to be exciting a considerable amount of attention amongst the readers of Knowledge, I have thought that a paper or two on the general aspects of the beings that link together distinct groups of animals, may prove interesting and instructive to reflective minds. It is very necessary that in the first place we should remember the special form which the rational demand for such "links" should take. I have already shown, for example, that there exists no necessity or demand whatever for any theoretical link, either between man and any existing ape, or between man and any extinct ape. Such a demand is simply the outcome of an ignorance both of natural history at large, and of evolution also; and, as often as not, such ignorance is of the most prejudiced type. That which the evolutionist and naturalist desire to know, is the nature of the forms which, on the theory of "development," must have connected the human *root stock* with the pre-human *root*. The connection, or "link," cannot be sought in the existing world. It must be obtained, if ever it comes to light at all, from the world of fossil life, and from the stores of life-relics which the geologist is year by year adding to our stores of knowledge. It is true that Nature is not bound to furnish us with "links" because we see a logical necessity for their existence. But all analogy leads us to expect that such "links" once existed; and I will now to describe certain interesting examples of such *intermediate forms*, as they are called, culled from varied groups of the animal world. In a word, if I am able to show that we possess at present in the world around us certain animals which undoubtedly connect distinct groups, I may claim the strong support of such examples in favour of the idea that "links" that are now "missing" where we desire their presence, once did exist.

Lepidosiren annectens, or Mud-fish, showing the paired fins.

One of the most curious groups of fishes is that named by zoologists the *Dipnoi*. This name means "double breathers," and the significance of the name will become apparent later on. Of this order of fishes, there are two chief examples. The *Lepidosirens*, or "mud fishes," found in the rivers Amazon and Gambia, form the first of these examples; whilst a curious fish, occurring in Australian waters, and known as the *Ceratodus*, or "Barramunda," represents the second type. This latter fish is the "Jeevine" or "Teebine" of the Australian natives. Now, in looking at either of those fishes, the observer would never for a moment suppose that they presented any features out of the common. Yet a very slight acquaintance with natural history soon proves the singular nature of their position in the fish-class. Everyone knows that fishes breathe by gills; that they are cold-blooded; that their "limbs" are represented by certain of their fins (the "paired" fins); and that their bodies are covered with scales. If we add to these facts the declaration that fishes possess a heart consisting of only two chambers, we shall have nearly completed our definition of the fish type; and we might add, lastly, that the nostrils in fishes are typically closed pockets, and do not, as in higher animals, open backwards into the mouth. It is necessary for our present purpose to point out that most fishes possess a singular sac or bag, lying just beneath the spine, and called the *swimming-bladder*, *air-bladder*, or *sound*. From the "sound" of the sturgeon we obtain isinglass; and in a herring, for example, the "sound" may be seen as a silvery, glistening bag, which is removable along with the other organs of the fish when it is "gutted." This bag contains gas, and its use is that of serving to alter the specific gravity of the fish—that is, to render (by compression or expansion of the gas) the body of the fish heavier or lighter than the surrounding water. It thus enables its possessor to readily rise or sink in the medium in which it lives. The air-bladders of all fishes, as Dr. Günther tells us, at first open

into their digestive systems by means of a tube or duct. But in many fishes this tube disappears, leaving the air-bladder a closed sac (as in the cod); or the duct may persist, and place the sound in communication with the digestive tract, as in the sturgeon or herring. Again, the air-bladder may be a simple and single sac; or it may be variously divided, and its interior may be smooth or may be divided into cells. We shall presently see that in the mud-fishes and the "Jeevine," this structure assumes a form and function for which its variations in common fishes in some measure prepare us.

INTELLIGENCE IN ANIMALS.

IT may be objected that in cases such as those we considered last, the animal has merely imitated an action which it has seen performed by others, and has subsequently learned to associate the action with its ordinary consequence. Apart from the consideration, however, that although in any single case such an interpretation might possibly be correct, it would be most improbable that it should explain all cases in which cats or dogs have used knockers or rung bells in the usual way, cases may be cited in which animals have devised a way of their own for producing such signals. Thus Mr. E. L. Layard, of the British Consulate, Noumea, relates the following case in which a cat acted in a way which can hardly be explained, save by assuming that she reasoned :—"Many years ago," he says, "we lived in Cambridge, in Emmanuel House, at the back of Emmanuel College. The premises were partly cut off from the road by a high wall; the body of the house stood back some little distance. A high trellis, dividing off the garden, ran from the entrance door to the wall, in which was another door, or grate. A portion of the house, a gable, faced the trellis. . . . . We were, after some time of residence, extremely troubled by runaway rings, generally most prevalent at night, and in rainy, bad, or cold weather, which was a great annoyance to the servant girls, who had to cross the space between the house and the wall to open the outer door in the latter, and were thus exposed to wet and cold." The annoyance became so great, that Mr. Layard and a cousin watched behind the trees on 'Jesus' piece,' armed with stout ash saplings, wherewith to administer a sound thrashing to the ringer, whomsoever he might be. But though the rings continued, no one pulled the handle. Hence the theory of ghosts was naturally suggested, but Mr. Layard, having brains, rejected that interpretation. At length chance cleared up the mystery. "Being ill," he says, "I was confined to the wing facing the trellis, and one miserable, blowing, wet day, gazing disconsolately out of the window, espied my favourite cat—a singularly intelligent animal, much petted—coming along the path, wet, draggle-tailed, and miserable. Pussy marched up to the house-door, sniffed at it, pushed it, mewed, but, finding it fairly shut, clambered up to the top of the trellis, some eight or ten feet from the ground, reached a paw over the edge, scratched till she found the bell-wire which ran along the upper rail from the wall to the house, caught hold of it, gave it a hearty pull, then jumped down, and waited demurely at the door. Out came the maid; in rushed puss. The former, after gazing vaguely up and down the street, returned, muttering 'blessings,' no doubt, on the ghost, to be confronted by me in the hall. 'Well, Lydia, I have at last found out who rings the bell.' 'Lard, master, ye haven't, surely'—she was broad Zumersetsheer. 'I have; come and see. Look out of the breakfast-room window, but don't show yourself.' Meanwhile I went into the drawing-room, where Mrs. Puss was busy drying herself before the fire. Catching her up, I popped her outside of the door and ran round to my post of observation. Puss tried the door, and mewed, thinking, probably, someone must be near, and, after waiting two or three minutes in vain, again sprang up the trellis and renewed her attack on the bell-wire, of course, to be immediately admitted by the delighted maid, who this time did *not* cross the yard, nor ever again, I fear sometimes to the inconvenience of visitors, if puss was waiting for admission."

In this case it is possible that the cat may have only discovered by accident that the bell-wire could be reached in the way described. This is Mr. Layard's explanation. He considers that puss, in clambering up the trellis to the house-top, accidentally moved the wire and caused the bell to ring. It seems at least as likely that she noticed the wire moving when the bell was rung, and afterwards deliberately moved it to produce the desired effect. But in either case, it is clear that neither instinct nor mere imitative faculty can explain the cat's action in this case. In passing, I may remark that the imitative faculty, which some regard as a merely automatic quality, seems to me far better explained as the result of reasoning, though, of course, the reasoning is not of a very high order; an animal seeing a man perform some action, infers that some advantage is to be gained by the action, and repeats it in the expectation that some good result will follow, though without knowing what this may be. However, in the present case, there was no imitation, nor certainly could any instinct have been in question. Mr. Layard mentions other cases, of which the same may be said. "I have known dogs shake a door violently," he says, "to attract attention and be let in. A dear old spaniel of ours at the Cape used to rattle the empty bucket if he was thirsty, and then come and look in our faces. My horse will come up from his pasture to the pump in the yard, and whinny till someone gives him water. . . . . Surely all this is abstract reasoning." he proceeds. "These things are not taught them, and they do not do all of them even by imitation. I don't go to the pump and whinny if I want drink! nor rattle a bucket! No! they come by a process of mental reasoning, and I am convinced all animals have it to a certain degree, more or less."

There have been cases which have afforded opportunity of noting the behaviour of an animal when first some new experience has occurred to it, and (as it would seem) new ideas have been suggested. Such cases are of extreme importance in determining whether animals really reason or not; because it must be admitted that in some instances where animals have appeared to reason, the action noted may possibly have originated, in the first instance, by accident, and have been continued subsequently as a mere habit. It is rather unfortunate that the only animals which we can observe under favourable conditions—domestic animals, and those which, though not domestic, affect the neighbourhood of houses—are not those whose cerebral development is of the highest order among animals. If monkeys were commonly domesticated (which would, for other reasons, be by no means desirable), we should probably have a number of far more striking and convincing instances of animal reasoning than we at present possess, for nearly all monkeys are far higher in cerebral development than the most sagacious dogs, while horses, cats, rats, &c., are lower than dogs in this respect. Still, if we remember that whatever evidence we obtain from the behaviour of dogs and cats must be regarded as suggesting, for this very reason, a powerful argument *à fortiori* as to the reasoning faculties of monkeys, and especially of the higher orders of simians, we may be well satisfied with

such instances as have been adduced above. The following case, showing how a cat reasoned out the meaning of a phenomenon brought for the first time under its notice, seems to afford decisive evidence of the capacity of animals to deal with cases when neither instinct, habit, nor imitative faculty can afford them any assistance :—A household cat was observed to enter a bedroom which was being cleaned at spring-time : a looking-glass stood on the floor, and Tom, on entering, found himself confronted by an image which he naturally supposed to be another cat, an intruder on his domains. He made hostile demonstrations, which were presently followed up by a rush at his opponent, who, nothing loth, seemed to rush also at him. Finding an apparent obstacle to his vengeance, Tom ran round behind the glass, where he found no enemy ; so he came again to the front. Here he again found his foe, on whom he again made an onslaught, only to be similarly foiled. He repeated this two or three times, applying manifestly the inductive method to the problem before him. The result of these experiments was to suggest the theory that the cat in the looking-glass, if actually existent, was unlike those specimens of the feline race with whom Tom's experience had hitherto made him acquainted. These repeated failures must have a meaning, Tom seems to have reasoned. Either he was the victim of some illusion, or the cat behind the glass was of altogether exceptional activity. But, however active that cat may be, Tom proceeded to reason, he cannot be on the further side and yet not on the further side at the same moment of time. If, then, I look at him and see him to all appearance on the further side, while at the same time I feel for him there with my paws and find him not there, then the cat in the glass must be a mere fraud. No sooner was this *experimentum crucis* devised by the clever cat than it was put into execution. Tom deliberately walked up to the looking-glass, keeping his eyes fixed on the image ; then, when near enough to the edge, he reached out carefully with his paw behind the glass for the supposed intruder, whilst with his head twisted round to the front he assured himself of the persistence of the reflection. He also must have recognised, what the narrator of the story seems to have overlooked—that the looking-glass was not, as it seemed, transparent, for the paw with which he was feeling about for the other cat was not visible, though the supposed intruder remained in view all the time. The apparent presence of the feline foe, though the feeling paw could not be seen, satisfied Tom fully. "The result of his experiment," says the narrator, "satisfied the cat that he had been the victim of delusion, and never afterwards would he condescend to notice mere reflections, though the trap was more than once laid for him." It would, by the way, have been worth while to try whether a looking-glass without a frame deceived him after he had discovered the meaning of an ordinary mirror, or whether a cat placed on the other side of a transparent framed glass would be at first mistaken for a mere reflection—his conduct in either case being carefully watched. A cat which had shown such excellent capacity for reasoning was worth experimenting on.

Whether we suppose that the cat of the preceding narrative judged of the position of his supposed foe solely by sight, or may partly have been influenced by the sense of sound (very slightly, in any case), it must be admitted that he showed a fitness for original research which some amongst ourselves might be found wanting in, if we may judge from their actions in certain cases. But it is an interesting question how far an animal may really be deceived by the image of another animal, or of some object in which the animal observer takes interest. There are stories of birds pecking at painted fruit, and the like, of which some are unquestionably apocryphal. When we remember, too, that some savages fail utterly to understand the meaning of pictures,* even of the most familiar objects, we may well doubt whether animals can possibly mistake a painted figure for a real object. Yet there are some stories which seem to show that animals certainly recognise pictures of persons, animals, or objects familiar to them. It would almost seem as though such cases could only be explained as depending on the exercise of a certain amount of reasoning power, the animal inferring that, because a certain picture presents details of shape and colour corresponding to those belonging to a familiar object, the picture is in some way connected with that object, although other senses—as of sight, smell, hearing, &c.—must serve perfectly to prevent any possibility of actual deception. A letter in *Nature*, by one who remarks that "his own observations lead him to suppose that dogs very rarely take notice of a painting or any representation on the flat," seems to me especially interesting, as illustrating how the sense of sight may for a moment deceive an animal which usually trusts chiefly to other senses. "I only know of one instance," he says. "A bull-terrier of mine was lying asleep upon a chair in the house of a friend, and was suddenly aroused by some noise. On opening his eyes, the dog caught sight of a portrait of a gentleman on the wall not far from him, upon which the light was shining strongly. He growled, and for some little time kept his eyes fixed upon the portrait, but shortly satisfying himself that there was no danger to be apprehended, he resumed his nap. I have often," proceeds the narrator, "endeavoured since to induce him to pay some attention to portraits and pictures, but without success, though sometimes he will bark at his own reflection in a looking-glass. He knows it to be his own image that he sees, for he very soon tires of barking and looking."

The Health of Navvies.—In view of extensive canalisation soon to be done in France, the Minister of Commerce lately consulted the Academy as to measures that should be taken to preserve the health of workmen engaged. A report by M. Colin is the result. In it he notes the persistence of a depressed vital state in certain parts of the country, especially the coast departments, which are chiefly concerned in the works projected. Marshes, with their miasma, are not the sole cause of this "impaludism" (as he calls it) ; but virgin soil, newly turned, emits morbid germs, whence arise intermittent fevers, &c. With regard to precautionary measures, the report specifies the following :—1. Fragmentation of work, not attacking too many points at once, not entering on a piece of work before the piece next it, begun previously, is finished. 2. Choosing strong, vigorous navvies, by preference inhabitants of the region. 3. Diminishing the time of contact with the soil. The work should be stopped at times when it is known to be dangerous—viz., July, August, and September, and in the extreme morning and evening hours. Evening vapour on plains and low-lying parts is very dangerous. Large fires should be lit in the works morning and evening. 4. Lodging of the navvies in the neighbouring centres of habitation, as much as possible in the heart of towns and villages, and on high ground, or, where near the sea, in pontoons moored at some little distance from the shore. Fevers do much less injury among workmen who reach their homes at night, than among those who remain on the works. 5. A special diet—before his work the navvy should have a substantial hot meal ; preventive medications (such as arsenic, extract of nux vomica, and sulphate of quinine) have not yielded such results as recommend their use. 6. Immediate conveyance to the hospital of a navvy attacked by fever, and caution as to premature re-engagement of men discharged from hospital. 7. Early filling of the excavations, admitting water, in urgent cases, to parts which prove peculiarly infectious, and stimulating vegetation on ground newly upturned.—*Times*.

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* There are some illustrations of this in the Editor's little book called "The Flowers of the Sky," in the article relating to "Fancied Figures among the Stars."

THE ELECTRIC TELEGRAPH.

BY W. LYND.

God has been bountiful to the human race in this age. He has given us to see Titans enslaved by man; steam harnessed to our carriages and ships; galvanism tamed into an alphabet—a gamut and its metal harp-strings stretched across the earth, *malgré* mountains and the sea, and so men's minds defying the twin monsters Time and Space." —CHARLES READE.

HALF A CENTURY ago railways were in their infancy, George Stephenson was bravely fighting against ignorance and prejudice, and founding his gigantic scheme which has revolutionised the world and proved one of the greatest blessings ever bestowed upon mankind.

Half a century ago there was not a single mile of telegraph wire in the universe.

Stephenson's name and fame have been trumpeted far and wide. Biographies without number of the great engineer have been written; only recently Dr. Smiles has given to the world his admirable "Lives" of the pioneers of railways, George and Robert Stephenson.

But, who has heard of that other Titan of the nineteenth century, William Fothergill Cooke?

The name of the man who introduced the first practical electric telegraph to this country is almost unknown outside scientific circles; yet, what a debt of gratitude we owe to the inventor of the instruments, by means of which that potent but mysterious agent we call electricity was first made to record intelligible signs which could be translated into human language! The first electro-magnetic telegraph was produced between 1820 and 1832 by Baron Schilling, of Lanstadt. Mr. Cooke was, in 1836, occupied in the Anatomical Museum at Heidelberg, preparing wax models for his father, who had been recently appointed Professor of Anatomy in the University of Durham.

It is a remarkable fact that he had no knowledge, or very little knowledge, of either physics or electricity. When Baron Schilling exhibited and explained his primitive telegraph, Mr. Cooke was so struck with the vast importance of an electric telegraph to the railways then progressing with such marvellous rapidity in the United Kingdom, that he immediately gave up modelling, and devoted all his time and energies to the realisation of his hopes. To be the means of establishing instantaneous communication by electricity between towns, particularly the great centres of commerce, was to become a benefactor of the human race. And Mr. Cooke was so sanguine of success, that he left Germany and came to England in April, 1836.

On Feb. 27, 1837, while engaged in perfecting a set of instruments to be tried on the Liverpool and Manchester Railway, he was introduced by Dr. Rogel to Professor Wheatstone, who had been devoting much of his time to the subject of electrical communication. The meeting was a happy one. The two electricians entered into partnership, and in a marvellously brief space of time the first really practicable electric telegraph was ready for trial.

There were, however, other claimants for the honour of having invented the electric telegraph as a practical reality, but there can be no question of Cooke and Wheatstone's priority in date over the other inventors, Alexander and Morse.

The first line of electric telegraph was constructed upon the Blackwall Railway in 1838. A sight of Cooke and Wheatstone's instrument, with its five needles, would surprise the telegraphists of the present day, who are used to duplex and quadruplex working!

The five needle instrument required five wires; now four messages can be shot along one wire!

The double needle instrument, which will be described later on, was the next improvement necessitating the employment of two wires only, and that form of apparatus can still be seen at work upon one or two of the railways in the United Kingdom.

Although Professor Wheatstone assisted Mr. Cooke, and, no doubt, contributed in no small degree to the perfection of the instruments, we are in duty bound to regard the latter as the father of practical telegraphy.

THE MAGIC WHEEL.

WE are able this week to give the series of views of a trotting-horse to which we referred last week; and to explain how the picture is to be arranged to produce a life-like effect, we repeat Fig. 1, as the two have to be considered together.

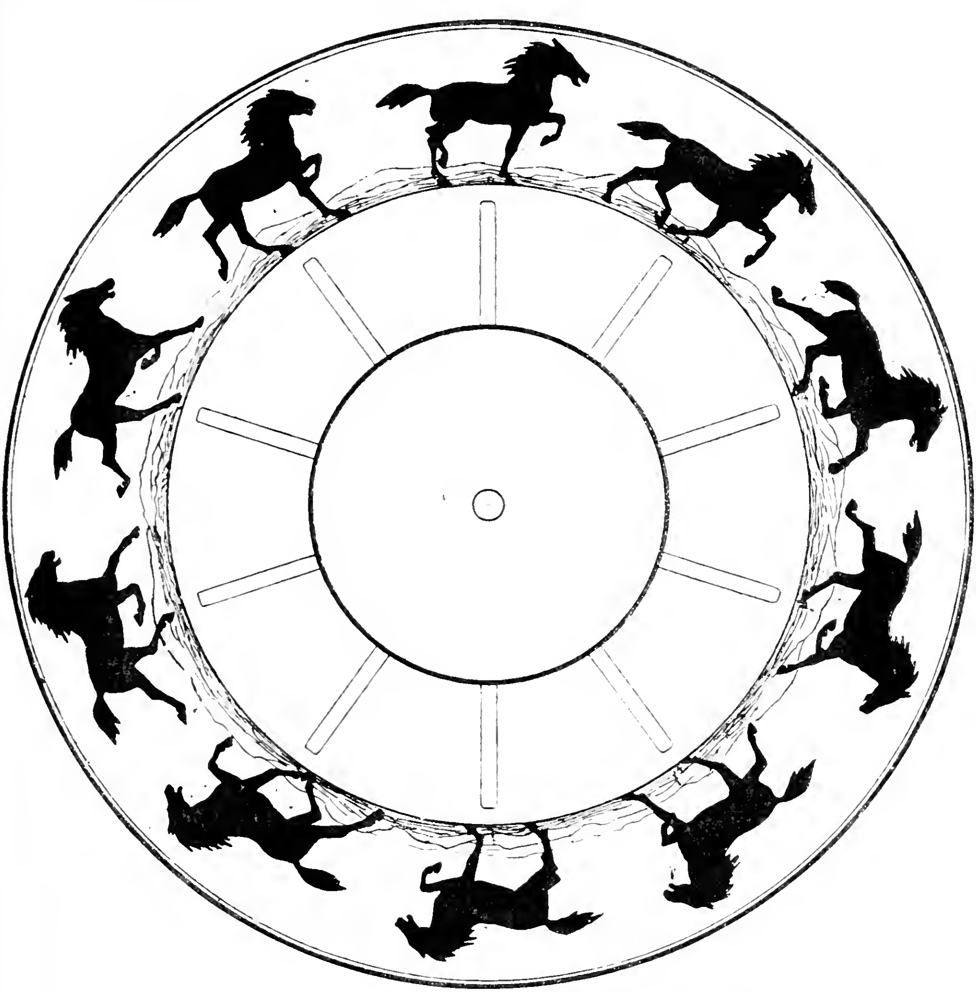
Fig. 1.

Cut out the series of views (following the outer circle) with the scissors, or carefully copy it on a separate piece of paper: and paste the circular disc thus obtained on a circular piece of cardboard. Cut out the oblong space under each figure, so as to make a series of oblong slits through the cardboard. Then fasten the wheel to a stick or handle, by means of a pin through the centre, on which it can freely turn. If now you stand opposite a mirror in the way shown in Fig. 1, and twirl round the disc before the eyes, looking through the upper slots, the horses will be seen to move as in life. The views have not been made by guess-work, as in most of the series used for zoetropes, but are from a series of actual photographs taken instantaneously at equal successive intervals of time during the trotting past of the celebrated racer, Abe Edgington. They were obtained by

Muybridge, of San Francisco. Next week, or the week after, we shall give a series showing a galloping horse.

The above views are from the *Scientific American;* but, as mentioned in our last, the series showing a trotting horse had to be modified before it could be used for the purpose of the magic wheel.

It is hardly necessary to say that the wheel can be readily made to turn uniformly by being put on a small axle, round which a string may be twined. But twirling with the hand will suffice to show how well worth while it is to provide for a more satisfactory method.



THE PLANETS AND SOLAR SPOTS.

M. DUPONCHEL considers that the maximum of solar spot frequency will not occur "as all the world, and M. Fagein particular," predict, in 1882, but not earlier than 1890. It may possibly occur as early as 1888, but far more probably will be as late as 1892. He bases this on the supposed relation between the sun spots and planetary movements, a relation which has not been established, but on the contrary, seems more and more unlikely the more the evidence is examined. Those who fondly imagine that the world is to come to an end in 1882 (the prediction of Mother Shipton—of fully equal value in our opinion—having failed for 1881) because of planetary perihelion passages, and resulting sun disturbances (also because the pyramid grand gallery is 1881·59 inches long, or ought to be), may breathe freely again, that is if they are disposed to prefer M. Duponchel as an authority to Professor Grimmer. For our own part we believe the world is quite as likely to come to an end in 1888, or 1890, or 1892, as in 1882. It has been coming to an end, at intervals of two or three years, for the last century, and probably, though we have no evidence as to details, ever since it seemed so certain to every one that the year 1,000 was to see the end of all things mundane. And so far as can be seen, one prediction in the past and for future dates has been as good as another—in other words, not one has been worth a straw.

NIGHTS WITH A THREE-INCH TELESCOPE.

UNDER the above heading we propose to furnish the beginner in astronomy with such directions as shall enable him to employ, to the greatest possible advantage, the kind of instrument with which he will, in all probability, at first provide himself. But, be it noted at the outset, that this series of papers is not intended for the possessors of telescopes of considerable aperture, equatorially mounted * or furnished with elaborate rackwork movements in altitude and azimuth.* For the owners of such, an abundant literature is already in existence; and they, at present, have such admirable works as Webb's "Celestial Objects for Common Telescopes," Crossley, Gledhill & Wilson's "Handbook of Double Stars," Chambers' one volume edition of Smyth's "Celestial Cycle," &c. We shall presuppose nothing on the part of our readers, then, beyond an ardent desire to become familiar with the beauties and glories of the celestial vault, and trust, if we can secure their attention, to put them fairly in the way of gratifying such a high and laudable aspiration. To this end, we shall use as our text the maps of the face of the sky which appear monthly in KNOWLEDGE, although we should strongly recommend the student to possess himself of the smaller "Star Atlas" by the editor of this journal, as well.

As it is of the first importance that the workman should be familiar with the tools he has to use, we shall devote this introductory essay to a description of the telescope itself, which we will imagine to be a 3-inch achromatic one, of about 42 in. focal length, mounted upon an ordinary "pillar and claw" stand. Such an instrument, as ordinarily sold, is shown in Fig. 1, which, however, represents it as furnished with a valuable little subsidiary contrivance (to be immediately described), which the observer will have to make, or get made, himself.

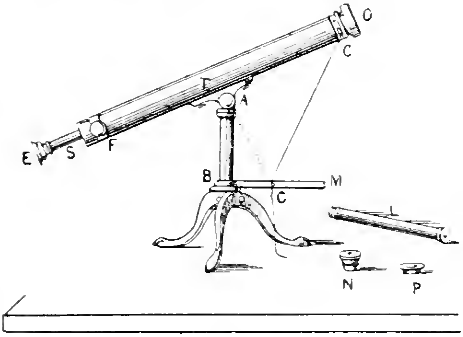


Fig. 1.

And here, albeit we are earnestly anxious to eliminate the commercial element altogether from our considerations, we are compelled to caution the student against supposing that a first-class 3-inch telescope for astronomical purposes can be made for £5, or, in fact, for any sum approaching it. The object-glass alone must cost the maker himself something like this amount. Hence, as we propose to deal with and describe celestial objects, as seen in an instrument of the highest class, we give this preliminary warning, lest the young observer should spend his money in a cheap glass, and then wonder at the discrepancy between our delineations of stars and planets and his own views of them. There is a vast amount of rubbish vended in the form of (so-called) cheap telescopes, and no tyro should ever purchase such a one without its previous examination and testing by a skilled expert. Makers like Cooke, Dallmeyer, and Wray will not imperil their great and deserved reputation by selling an inferior object glass, even to a total stranger; but instruments of unknown opticians require the most rigid trial before they can be safely bought. We shall give further on, a few tests by which the student himself may judge somewhat of the quality of his purchase. It is time, however, to turn to our figure. Here we see the brass tube T, into one end of which screws the cell containing the object glass O. Through a tube projecting from the brass disc which covers the other end of T, the smaller tube S is worked in and out by the milled head F, acting in a rack and pinion. This is for the purpose of focussing the telescope, and making the image of the object observed sharp and distinct. Into the tube S screw the eye-piece E, consisting of two lenses mounted in a short piece of tubing. Shortly, the action of the instrument is this. The object-glass forms in its focus an image of the object to which it is directed, and the eye-piece—which is really a microscope—magnifies this image before it enters the observer's eye. So much for the telescope itself. It is bolted, as will be seen, by two screws and nuts to a brass plate, which has a vertical motion, by means of the knuckle-joint at A, at the top of the stout brass pillar AB; and a horizontal one, furnished by the rotation of the whole of this top-fitting, inside the pillar. Three massive feet form its support. The arm BM shown in our drawing forms no part of the ordinary fitting of the instrument; it constitutes the subsidiary contrivance of which we spoke above, and we shall explain its use presently. L in the figure represents a terrestrial or four-lens eye-piece, which shows objects erect, and hence is used for land-purposes. It screws in at the extremity of S, just as E does. The ordinary astronomical, or so-called "Huyghenian" eye-piece contains, as we have previously said, only two lenses, and inverts, or turns objects upside-down. This, however, is obviously immaterial in a star, and this construction of the eye-piece enables us to obtain high power with comparatively small loss of light. N is another astronomical eye-piece, and P a dark cap or shade, screwing on to every eye-piece, for the purpose of observing the sun. The student is earnestly warned never to look at the sun through a telescope without first covering the eye-piece with one of these shades. When, however, we come, in a future paper, to speak of the sun, we shall describe how the solar details may be telescopically shown without looking through the instrument at all. The powers usually supplied with a telescope of the size we are describing are one terrestrial one, magnifying, perhaps, 45, and three astronomical ones, giving powers of something like 50, 100, and 180. If, however, the observer intends to devote his instrument wholly to the sky, we should advise him to replace the terrestrial eye-piece by two Huyghenian ones, magnifying 25 (for comets, nebulæ, and clusters), and 250 (for close double stars) respectively. For night use, too, a "Dew-cap" will be found indispensable. This may be made of a tin tube, bright outside and blackened within, about 8 inches long, and fitting over the object end of the telescope at O. This prevents direct radiation from the object-glass, and the consequent deposition of dew upon it. *Never wipe your object-glass if you can possibly help it.* Expose it to the heat of a fire (not too near) or of the sun, should it become heavily dewed.

A word may now be said as to the use of the bar BM shown in our sketch. It is a fact familiar to nearly everyone who has ever opened an astronomical primer (and, at any rate, to be established by a single winter night's observation of the sky from dusk to dawn), that the stars all seem to describe circles round a centre in the Northern sky, called the Pole, very close to which is situated the star we call the Pole-star. The farther we go from this centre, the larger these circles become, up to a distance of 90°, beyond which they begin to diminish again. Moreover, the point round which they turn is something over 50° above the Northern horizon (depending on the observer's latitude), so that they are all described obliquely to the horizon. Obviously, were the apparent axis of the concave celestial vault vertical, the Pole would be overhead, and the stars, seeming to describe circles parallel to the horizon, would neither rise nor set. In this imaginary condition of things (imaginary in England, for it really exists at the Poles), the mounting of the telescope shown in our figure above would enable the observer to follow a star by merely turning the telescope round the vertical axis, AB, when once that star was in the field; but a moment's thought will show that a simple movement round a vertical axis will by no means accomplish this when the star's path is described round an inclined one. The vertical movement of the telescope, we may here say, is spoken of as its motion in altitude; its horizontal motion as that in azimuth. It may require a little more attention to see that if we so tilted the axis AB that it became parallel to (or practically coincided) with the apparent axis of the sky; that then the simple motion round it would follow any star to which it was directed, from its rising to its setting. A telescope thus placed is said to be equatorially mounted. Now, the little device in our cut, for which, in its existing form, we are indebted to the Earl of Crawford and Balcarres, is intended to communicate an approximately equatorial motion to the ordinary altazimuth mounting of the instrument. It takes the form of a bar BM, extending from the base of the pillar AB. In it, at such a distance from the point B vertically under A that the angle ACB shall be = the latitude of the place, a hole is bored, and a thumbscrew (shown at C) inserted through the bar, so as to nip a light chain or thin wire tight when it is passed through the hole. The other end of this chain is fastened anywhere towards the end of the telescope at C', and sufficient weight is put on to the eye end of the telescope to keep

* These terms will be explained as we proceed.

the chain C C tight. Perhaps we may say that if the height from A to B is (as is very common) 11 in., the hole at C may be $8\frac{1}{4}$ in. from B. This will give a quasi-equatorial movement to the telescope for London and places not very far north and south of the same latitude. The use of this contrivance is very simple. The bar BM is placed due north and south (M, of course, towards the south). A star is got into the field, and the chain C,C′ stretched tight and made fast. Then the observer will find that on rotating the telescope horizontally round A, the end O will be so shackled as to constrain it to follow the given object. A few miscellaneous hints may conclude what we have to say on the telescope itself. First, the reader may wish to test it for its freedom from colour and aberration. For the first, let him turn the instrument on to the round edge (or "limb") of the moon, and first move the eye-piece within the focus by means of the milled head F, then a purple fringe should appear on the lunar limb. On moving the eye-piece outside the focus, this should give place to a green ring. A telescope that exhibits this sequence of phenomena is achromatic. For spherical aberration, focus the telescope on a tolerably bright star, with the whole aperture, and then put a diaphram of, say, $1\frac{1}{2}$ in. aperture over the object glass,

Fig. 2.

Fig. 3.

and see if the star remains accurately in focus. If it does, spherical aberration is cured too. A bright star in focus with a power of 150 should present the appearance of Fig. 2, by no means that of Fig. 3, which latter indicates a practically worthless object-glass; nor should any light *haze* appear about bright stars or planets. Next week we hope to set the young observer fairly to work.

THE SO-CALLED TUNNEL-WORM.

IN *La Nature*, for December 10, M. Maxime Hélène remarked that exaggerated ideas are prevalent respecting the defective ventilation of the great tunnel of St. Gothard, and in particular that the supposed development of a special parasite, the tunnel-worm, must be regarded as problematic. To this Professor F. A. Forel replies that the malady is perfectly authentic, and is due to a parasitic worm, the Duodenal Anchylostoma (the stiff-jaws of the first intestine, one might say), which attacks the mucus membrane of the jejunum and duodenum (the first parts of the small intestines), and sucks the blood like a leech. He says that Dr. Ed. Bugnion, a professor at Lausanne, has recently published a remarkable investigation, in which he has discussed the question thoroughly. He has examined forty-one well authenticated cases among the workmen in the Gothard tunnel, attacked by chloron and anæmia (ghastly paleness and bloodlessness). The presence of the tunnel-worm was established in these cases either by post-mortem examination or from the recognition of the eggs in voided matter. As for the origin of the disease, it is not, as has been thought, a new one, or produced by either the high temperature and bad ventilation of the tunnels. The worm is very common in Egypt, where it causes Egyptian chlorosis, and in Italy. Its appearance in the St. Gothard tunnel is easily explained, when we consider, on the one hand, the habits of the thousands of workmen employed there (all Italians, crowded in the villages of Airolo and de Goeshenen), and the migrations of the intestinal worm. It passes, says M. Bugnion, the first part of its existence in the mud and slime of water pools*, and it is from drinking dirty water containing young larvæ of the parasite that the infection is received. Since the nature of the disease has been known, worm medicines have been used, and the doctors of the St. Gothard tunnel have lost no more of the patients attacked by the ailment, which had been called provisionally the St. Gothard anæmia, and which should hereafter be called, Professor Forel thinks, by the pleasing name "Anchylostomasia."

* *Le limon et la vase des flaques d'eau*, amusingly mistranslated by a daily contemporary: "the lemon and vase, part of water flasks."

SCIENTIFIC PARADOX.

TIME was when itinerant lecturers made a great point of what was then called the "hydrostatic paradox," by which the weight of a pint of water might be made to burst asunder the strongest cask; but although that age has passed, yet we are not without some paradoxes which yet remain in the ordinary treatment of physics.

That steam at 100°C. should heat a bath of Calcium Chloride up to 115°C., is paradoxical at first sight; but it is an established fact, and well understood by those who are familiar with the laws of specific heat.

That getting nearer to the sun as a source of heat should result in perpetual ice, is paradoxical at first sight; but it is a fact, and well understood by those who are familiar with the complex conditions.

The following case may or may not enter this category; but after much trial, I have failed to understand the conditions.

(a.) *One Gas does not behave as a Vacuum to another.*—If I understand the books rightly, there is no difference between the pressure of two separate pound weights in a pair of scales, and the pressure of two separate units of gas (be they similar or different) in an exhausted vessel. In both cases, each presses with its own individual pressure; and in every case, the *joint-pressure* upon the sides of a vessel will be exactly as the sum of the individual tensions.

Volatile substances have different maximum points of tension, beyond which they will not volatilise. A cubic inch of water at 100 C. will evaporate into an exhausted vessel of 1,700 cubic inches capacity, and produce a pressure on the sides of 760 millimètres of mercury (or one atmosphere); and if more water be added, it will not be evaporated; while, if the pressure be increased, water in proportion would be condensed.

Such points of maximum tension for water are at 100° = 760 mm.

,,	,,	,,	alcohol	,,	1,697 ,,
,,	,,	,,	ether	,,	1,953 ,,

(b) *One Gas does behave as a Vacuum to another.*—If I understand the books rightly, one cubic inch of water would evaporate into the aforesaid vessel, equally whether it be a vacuum, or filled with air, or any other gas; so that if the vessel were previously full of both alcohol and ether vapours, their joint pressures would be 6,650 millimètres; against which the cubic inch of water would evaporate; but more slowly as the pressure increased.

In the books these two aspects are confusedly mixed up together, and perhaps a novice has not been quite clear in separately and strongly stating the paradox.

It is rather curious that Clerk Maxwell, in his treatise on "Heat," while admitting that one gas may be a vacuum to another, yet says nothing in justification of the second aspect.

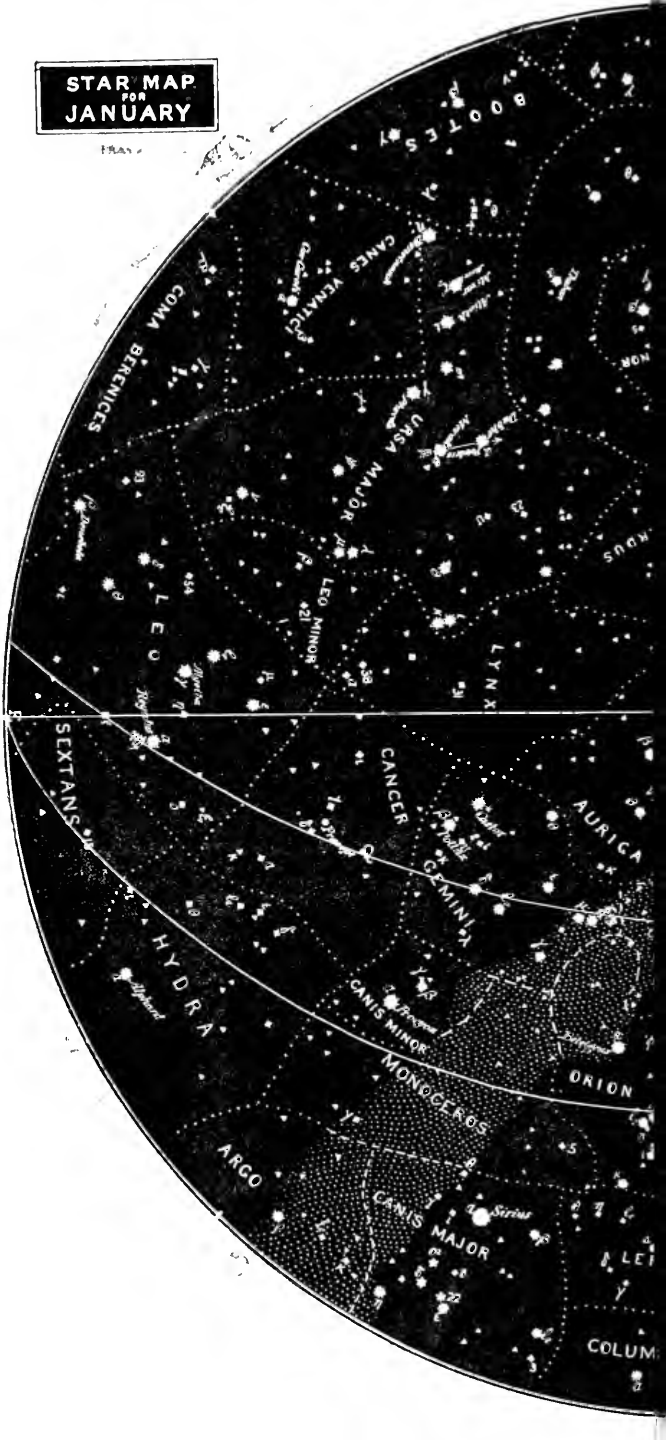
ECLECTICUS.

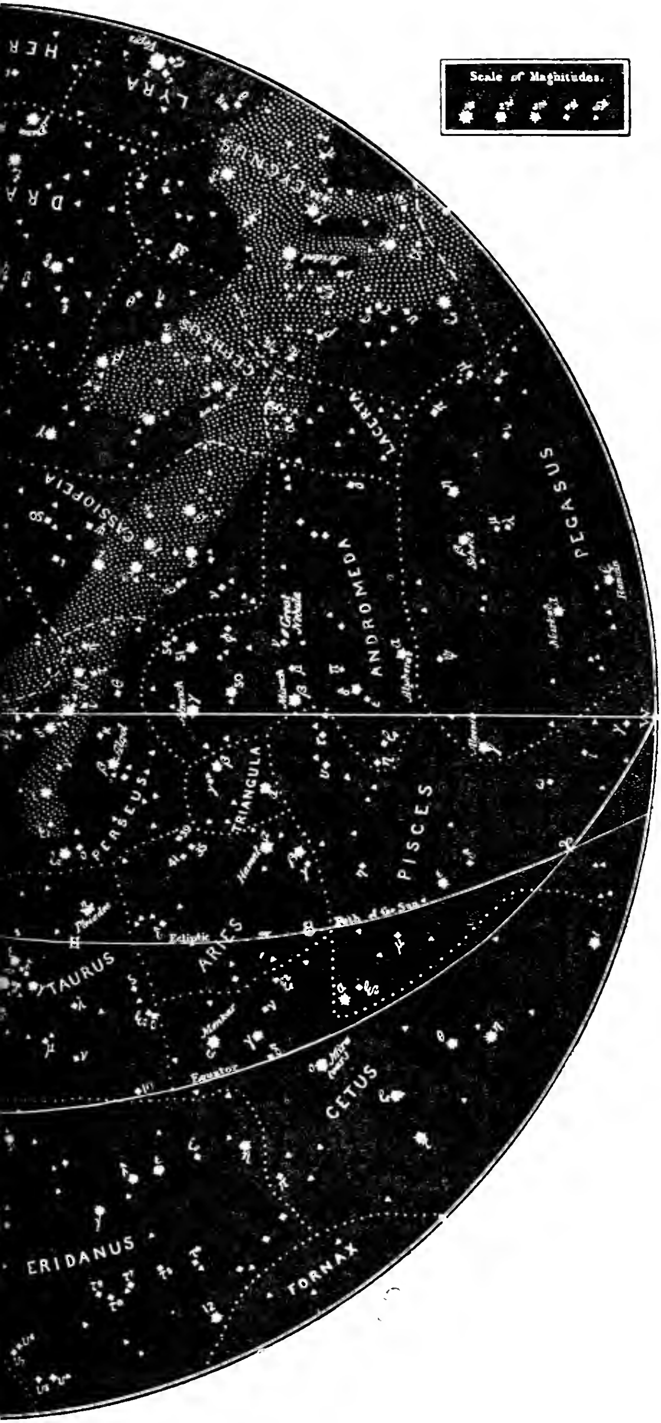
VITALITY OF TOADS.

THE following passage from an old number of the "Cosmos," may interest those who have taken part in the discussion about the vitality of toads. It is an account of various experiments on this subject, by M. Victor Legrips, of Chambon. M. Legrips put some toads in chambers hollowed in the earth to the depth of sixteen inches, placing them on a tile, and covering them with a pot. Others were immured between two discs of glass in plaster cells, without access of air; and some were imprisoned in masses of mixed plaster, which touched every part of them. The first were examined monthly, and exhibited no mark of decay till towards the twelfth month, their excretions being taken as nutriment. They lived thus, on an average, for twenty-three months. The second lot, examined through the glass, presented the following phenomena: Abundant excretions of the debris of insects and larvæ; torpor while in darkness; sparkling of the eye at approach of light; powerful efforts to escape; progressive emaciation, till death, which took place generally after fifty-six days. Among the third class some toads were living after twenty-eight months of absolute sepulture. These facts prove that the toad can live a long time without aliment or aëration; he lived longer in the chambers where his movements were unimpeded than in the cells where he could hardly change his position; but his life was very much longer when he was completely embedded in the plaster. Not being able to move, he lost nothing; and thus it will be easily comprehended that alimentation was much less necessary, and that his life, as if suspended, might continue for an indefinite period. M. Legrips states that toads are not only inoffensive, but exceedingly useful in gardens, and that he is not at all surprised at the increasing commerce in these animals, who live exclusively on worms, caterpillars, and insects, great and small, and are thus protectors of a host of useful and ornamental plants. Market gardeners will find them invaluable.

STAR MAP FOR JANUARY

On December 31, at 10.30 p.m.
On January 3, at 10.15 p.m.
On January 7, at 10 p.m.
On January 10, at 9.45 p.m.
On January 14, at 9.30 p.m.
On January 18, at 9.15 p.m.
On January 22, at 9 p.m.
On January 25, at 8.45 p.m.
On January 29, at 8.30 p.m.
On February 2, at 8.15 p.m.





OUR STAR MAP. — In response to the wishes of many correspondents, we show this week the star maps which would otherwise have been divided into four weekly maps, in a single star map. It will be understood that the circular boundry of this map represents the horizon. The map shows also the position of the equator, and of the ecliptic, with its signs. We propose next week to give the position of the Zodiac, now most favourably situated for observation, with the motions of the planets thereon.

Letters to the Editor.

[The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with their writers. All communications should be as short as possible, consistently with full and clear statements of the writer's meaning.]

All Editorial communications should be addressed to the Editor of KNOWLEDGE; *all Business communications to the Publishers, at the Office, 74, Great Queen-street, W.C.*

All Remittances, Cheques, and Post-Office Orders should be made payable to Messrs. Wyman & Sons.

⁂ All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.

All Letters or Queries to the Editor which require attention in the current issue of KNOWLEDGE, *should reach the Publishing Office not later than the Saturday preceding the day of publication.*

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

Our Correspondence Columns.

THE WEATHER FORECASTS.—PSYCHO.—TOADS STUNG BY INSECTS.—BOOKS ON HISTOLOGY AND THE UNDULATORY THEORY OF LIGHT.—TABLE OF LOGARITHMS.—DIAGRAMS WITHOUT DESCRIPTION.—PRONUNCIATION OF "BETELGEUX."—THE COAL AGE AND THE EARTH'S INTERNAL HEAT.—ANTIQUITY OF MAN.

[164]—The letter [121] of Mr. Spiller, on p. 140, is a very instructive one, and may well stimulate inquiry into the procedure of that rather remarkable department, the Meteorological Office. I, like Mr. Spiller, took considerable pains some time since to compare the daily vaticinations issued from Victoria-street, with the actual weather obtaining at the periods for which they were made, and with the following noteworthy result. At first I found that the predictions were very much more frequently wrong than right, and this suggested the idea that the officials were on the track of *some* law, but were merely misinterpreting it. Later on, however, I came to the conclusion, in common with your correspondent, "that they are about as often wrong as right." It is absolutely needless to point out to any one with the least smattering of mathematics, what this indicates. Put shortly, it is that equally valuable weather prophecies might be made by the simple process of tossing up a florin and calling "heads" fine and "tails" stormy. Now this suggests to me at once, as a scientific man and as a taxpayer, that the British public has a distinct right to know why it should be called upon to pay £14,500 annually for such results as these? Moreover, it is rumoured that the office is supervised by a certain "Meteorological Committee of the Royal Society," who divide £1,000 a year among themselves. One would be pleased to ascertain definitely what is actually done by these gentry for this modest little sum. The Meteorological Office exists for the benefit of the nation—not the nation for that of the Meteorological Office, and the sooner this is realized the better for all those concerned. It seems ridiculous that we should receive such accurate forecasts of gales gratis from a New York newspaper proprietor, and that we should be reading about the "calm," prophesied by our own paid servants, while those gales are howling around us. As a very humble effort towards the solution of this concluding part of Mr. Spiller's letter, I would venture to suggest *Payment by Results.*

May I say, in connection with the question put by W. H. B. (letter 126, p. 143), that three or four visits to Psycho sufficed to convince me that a small boy is concealed within the figure. Something I once observed through a powerful field-glass satisfied me of this.

Ornithorynchus (query 100, p. 145), is wrong in supposing that toads are not affected by the stings of the Hymenoptera. I have myself seen a toad stung by a wasp which it attempted to swallow, its tongue subsequently protruding from its mouth in the form of an inflated bladder, obviously causing it serious, if not very protracted, inconvenience.

If D'Artagnan (query 103, p. 145), does not mind reading a book thirty years old, he may derive an immense amount of information from Quekett's "Lectures on Histology." It was originally published by Baillière; but one of the second-hand scientific booksellers would be the likeliest place to procure it now. Dr. Lloyd's "Elementary Treatise on the Wave Theory of Light," published by Longman's, is excellent too, for the purpose for which he requires it.

To the list of books of Logarithms, given on p. 102, I should like to add Cape's "Mathematical Tables," published by Longman in 1860 (3rd edition), as the most convenient 6-figure set with which I am acquainted. The arrangement is excellent. In the trigonometrical tables the successive differences are given for 100″ instead of 60″, thus saving a quantity of calculation. I use these in my observatory, and Chambers' in my library.

May I venture to suggest that Mr. Thorp's drawings on p. 100, by themselves, are scarcely sufficiently explanatory of the instruments they represent? Notably Fig. 2, in the absence of verbal explanation, fails wholly to show how an ellipse can be described by the piece of apparatus delineated. It looks as though it must draw a circle—and nothing else.

In answer to query 107 (p. 167), astronomers, in practice, always pronounce the proper name of (a) Orionis "Bételjews."

"Ignoramus" (query 110, p. 167) should obtain and carefully read through that part of Page's "Advanced Text-book of Geology," which treats of the Coal Measures. It is not now supposed that the internal heat of the earth had anything to do with the production of the carboniferous flora; in fact, it is a grave question whether such flora was, in any legitimate sense, tropical at all. A humid and equable, rather than a tropical climate, would seem to favour the type of vegetation of which our coal measures are composed. The earth's superficial temperature is quite unappreciably afforded by her internal heat (Feurier says $\frac{1}{17}$th of a degree), such surface temperature being practically wholly derived from the sun. This influence extends to a depth varying from 60 to 90 feet. Downwards from this stratum temperature rises 1 degree for every 60 to 65 feet; so that at a relatively short distance from the earth's surface, it would seem that the most infusible rocks must be molten. Possibly, the sun may one day cool down, and the earth with it, but it will not be in "Ignoramus's" time, or mine either.

Any one reading the isolated words quoted in inverted commas from Sir Charles Lyell in reply 84 (p. 168), will imagine that he did not believe in the great antiquity of the objects discovered by Mr. Horner in Egypt. Those who have been misled into this belief had better read Chapter III. of Sir Charles's own "Antiquity of Man," as recommended by "Kemp" in the preceding answer.

A FELLOW OF THE ROYAL ASTRONOMICAL SOCIETY.

A CORRECTION: DATE OF MENES.

[165]—I perceive that "A Member of the Society of Biblical Archæology" quotes Mr. Stuart Poole as an authority for the date B.C. 2717 for the period of Menes. Permit me to say that Mr. Stuart Poole has long since abandoned that position in favour of a much more remote epoch, as may be seen by reference to his articles in *The Contemporary Review* for 1879.

ANOTHER MEMBER OF THE SOCIETY OF BIBLICAL ARCHÆOLOGY.

PERSONAL IDENTITY *versus* TATTOO MARKS.

[166]—Am I right in understanding that, owing to the Physiological waste and decay, our bodies virtually change in every part, so that our bodies to-day are not the same bodies seven years hence? If so, may I ask the following:—When a boy, I tattooed my arm with Indian ink; my body must have changed four times during that time; the design is as distinct and permanent to-day as ever. Will some of your correspondents kindly enlighten me upon this subject?—Yours, &c. WALTER H. MAGUIRE.

Dec. 5, 1881.

P.S.—I wish your valuable journal every success.

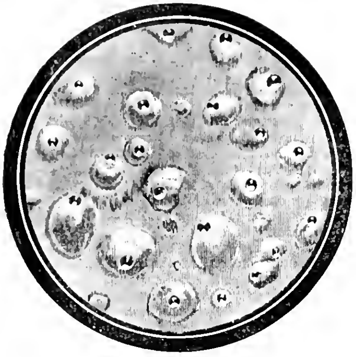
THAWING ICE.

[167]—In a book which I have—a translation from the French of Professor A. Cazin—the following experiment is described:—

"Take a piece of clear ice and place it so that a pencil of sunlight, condensed by a lens, shall enter it parallel to the planes of congelation; then with a microscope, placed at right angles to plane of congelation, examine the illuminated part, in which many luminous specks will appear, gradually increase in size, and send out little branches, generally six in number and of varying forms, &c."

I have tried this interesting experiment with several pieces of ice, but failed to see any traces of these beautiful little branchlets. What I saw was the specks of light rapidly increasing in size, and

of a more or less nearly-circular form. Each of these cavities contained water and a bubble (of attenuated steam, I suppose); some of these bubbles remained in the centre of their cells, but others soon (especially if the sun was hot) began to move round their cells, sometimes in jerks, at others smoothly, occasionally oscillating. I should be glad to know why I was unable to see the branchlets, and should be greatly obliged to any reader who would explain the cause of the little bubble's curious movements.



Enclosed is a drawing which will give a good idea of what I observed. The larger discs represent the cavities containing liquid, while the small circular black and white spots are the bubbles. E. C. R.

DIFFICULTY OF OBTAINING "KNOWLEDGE."

[168]—Having experienced the same difficulty as mentioned by your correspondent in No. 7, I would recommend your would-be readers to order KNOWLEDGE of their newsagent, instead of the bookseller, who, for some reason best known to himself, professes ignorance of the publication.—I am, Sir, yours faithfully,

A. GAUBERT.

[We have received several letters to the above effect.—ED.]

[169]—In reference to Letter No. 134, page 144, the fact that I have generally obtained KNOWLEDGE from my bookseller in North Shields before 9 a.m. on Friday mornings may convince Mr. Armstrong that either his booksellers or their London agents are alone to blame.—Yours, &c., GEORDIE.

"OUR UNBIDDEN GUESTS."

[170]—Surely "the great lesson to be learned" from Dr. Andrew Wilson's frightful disclosures in the last number of KNOWLEDGE, respecting tapeworm and trichinæ, is one quite different from that proposed by him, and not "thorough cooking," but total avoidance of the substances liable to be infested, is the true and sensible remedy. From the "Perfect Way in Diet," recently favourably noticed by you, it would appear that man is, by his constitution and structure, not carnivorous at all, but becomes so only through degeneration of habit. From this it ought to follow that man can attain to the perfection of his nature only by subsisting on the substances indicated by his structure. My own experience favours this conclusion. Having abstained from flesh-food for some seven years, I find the results—physical, intellectual, and other—such as to make me regret that I was not a follower of the "Perfect Way" from the first, and my experience is by no means a singular one within my own circle of acquaintance.—Yours, &c., E. M.

THE POLE STAR AND PRECESSION.

[171]—In your promised paper on the Precession of the Equinoxes, can you give us a star-map, showing the circle described by the North Pole, and therefore all the stars which have been successively Pole-stars? One meets with the statement that about 4,000 years ago *α Draconis* was the Pole-star; but no full information is given in popular books. With such a star-map as I suggest, we could find for ourselves the Pole-star for any period of the past. —Yours, &c., GEORGE ST. CLAIR.

[Will supply such a map the week after next.—ED.]

A NEW COMPARISON OF POISONS.

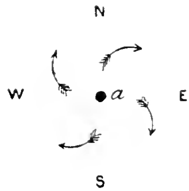
[172]—Notwithstanding your approval of "Technical Chemist" (Letter 102, p. 116), I venture to think he is not quite right. The allegation is that li. ce. is three times as poisonous as ba. ce.; and, while admitting that ba. ce. is poisonous, it is urged that li. ce. is often prescribed in comparatively large quantities for gout. Now, why mystify M. Richet's plain proposition? Li. ce. has 20 per cent. of metal, where the citrate has probably much less than 7 per cent.; besides, iron and mercury may be very poisonous in one form of combination, and very innocuous in another. S. E. P.

SOUND.

[173]—"C. T. B." (query 60, p. 125) may be right in saying "the lighter the weight, the better does a body transmit sound"; but only in conjunction with a mysterious function of elasticity, which figures so largely in scholastic formulæ and ideas. We know that air transmits sound in round numbers, about 1,000 ft. per second; water 4,000 ft.; and some dense woods and metals, 10,000 to 15,000 ft. per second. A great chemist on the Continent is holding up to ridicule a kind of weekly anthology of modern chemical utterances; with some such feeling I would try to fairly paraphrase the sound contentions of "T. J. H." (query 14, p. 123). The condensation augments the elasticity by the heat generated in the condensation of particles. The rarefaction augments the elasticity by the cold generated, and also by the separation of particles. The heat generated remains there to increase the velocity. It is because the heat generated in the condensation augments the rapidity of the condensation, and the cold developed, augments the rapidity of the rarefaction, that the heat and cold both help to augment the velocity of the sound wave. ECLECTICUS.

THE MAGNETIC NEEDLE.

[174]—There is no attraction of the kind "W. H. P." (query 90, p. 123) seems to suppose. The affection is one rather of polarity or deflection. Let "W. H. P." imagine a globe with convolutions of wire conveying electricity round the equator, representing *actual* earth currents, then he will find that particles of air or iron will have a binary polarity induced in them, at right angles to the direction of current. There will be a field of magnetism where the lines of force point N. and S., or to the two poles; but there is no tendency to movement or change of position of the polarized medium.



In order to remove any ambiguity about these poles, it may be well to explain that this same field of force subsists all round any wire conveying a current. Here, then, we have a circular field of force with no poles. Let (*a*) be a section of the wire carrying a current downwards or from the point of view; then small unmagnetized needles (with no directivity) would have polarity induced in them such as to point east, west, north, or south, as per diagram. If the current flow upwards, these polarities would be reversed.

In very many cases, arrows indicate motion or a tendency to motion. In this case, they only convey the idea of polarity in the media, where iron is immensely more sensitive or conductive than air. ECLECTICUS.

BUTTERCUPS—BEES AS FLOWER FERTILISERS.

[175]—In answer to "West Riding," I should like to remark that where grasses, daisies, and docks predominate, I have never observed that the vegetation was "thick and matted;" I was alluding rather to tangled hedgerows and overgrown spaces. "West Riding" can hardly expect me to explain the whole philosophy of leaves, among other things, in a single short article. Oddly enough, my intention, before seeing his letter, was to write a paper for KNOWLEDGE on the foliage of daisies and plantains, and, with the Editor's kind permission—[Gladly accorded.—ED.]—I will now do so shortly. The subject is one which I have long been observing.

Mr. Donbavand is mistaken in supposing that bees never visit

buttercups. Whether the hive-bee in particular does so, I cannot say with certainty; but I am sure that the common bumble-bee is often found among the flowers. Of course, other insects also aid in fertilising them.

GRANT ALLEN.

NUMERICAL COINCIDENCES.

[176]—This numerical disease is not confined to the Pyramids and astronomy.

M. Berger, referring to the failure of Prout's theory, that all the atoms might be multiples of the unit hydrogen, finds out that by dividing the chief elements into five groups, then all are multiples, or submultiples of five specified values, which are notated H, D^1, D^2, D^3, and D^4. He attaches much value to this classification, but admits that "these five divisors have no value or relation among themselves."

A professor of Harvard College, America, professes to see farther into a brick wall than M. Berger, under the attractive title, of "Atomic Phyllotaxy," to wit :—

Berger.				Phyllotactic.		
H	=	·9997	=	1-16th of O	=	·998
D^1	=	·769	=	5-13th of D^2	=	·768
D^2	=	1·995	=	1-8th of O	=	1·996
D^3	=	1·559	=	$(\frac{1}{2} \times \frac{5}{8})^2$ of O	=	1·559
D^4	=	1·215	=	5-8ths of D^2	=	1·247

ECLECTICUS.

THE SUN'S CONSTITUTION, AND ORIGIN OF SUN-SPOTS.

[177]—I have noticed that in many modern text-books of astronomy (especially those published since 1870), doubts are expressed as to the truth of Herschel's theory of the sun's constitution (to which for so many years astronomers had inclined) viz., the theory of a *comparatively* cool nucleus, enclosed in two concentric shells of matter, analagous to our clouds, of which the outer forms the visible surface of the sun. In your "Poetry of Astronomy" you say, "We know the sun to be infinitely more complex in structure . . . . than it was formerly supposed to be. . . . . We have learned that . . . . the glowing veil of air hides by day . . . . the largest (though not the most massive) part of that sun. . . . ." Would you give us a paper in KNOWLEDGE on this most interesting of astronomical subjects, as in your books you do not explain what you intend to convey by the above? I presume you speak of the sun's chrom[at]osphere, corona, and the atmosphere discovered by Professor Young. I should be much obliged if you would insert my letter, as I should much like to see correspondence on this most interesting subject. Discussions on it would be far more interesting than those on Anti-Guèbre's absurd theory. Hoping for the favour of insertion,—Believe me, yours, &c.,

VIGNOLES.

[We shall be only too glad. Our fear is lest readers should think we give too much space to our own subjects.—ED.]

SINGULAR PROPERTY OF NUMBER 4.

[178]—The following is a solution of this interesting property of numbers for the number 19.

$$\underline{|4} - \left(4 + \frac{4}{4}\right) = 19$$

H. SNELL.

[Functional 4, or the products of the numbers 1, 2, 3, 4, can hardly be regarded as a fraction of 4 in the sense required for the solution of this little problem. Neither can ·4, or 4 divided by 10.—ED.]

SMALL TELESCOPES.

[179]—As I am about to purchase a telescope for astronomical purposes, I am at a loss whether to invest in a refracting or a reflecting telescope. The work I want it for is the course specified in Webb's "Celestial Objects," at least as far as an ordinary $3\frac{1}{4}$ in. refractor, with powers of 70 to 200, would suffice; but I do not know whether to purchase a refractor of this aperture, or a reflector with corresponding powers. I am not prepared to invest beyond £15 for the instrument, and should feel extremely obliged if you could advise me as to what to buy. By so doing you would infinitely oblige, yours truly,

W. RIDD.

VERMIN.

[180]—I should be glad if you would insert an article in your valuable paper upon the uses of those apparently useless insects, bugs, fleas, flies, roaches, &c., or if you could name a good book upon the subject.

Hoping that your paper will have the utmost success.—Yours,

AN AMATEUR.

[We hope our readers will not say, with Mr. Pecksniff :—"Oh, vermin! Oh, bloodsuckers! Begone! abscond!" —ED.]

MALE AND FEMALE HEADS.

[181]—It appears to me that in the correspondence on this subject, too little prominence is given to the effect of long-continued training in the development of the male head and its faculties, while, in estimating female powers, few writers or speakers care to remember that, till within the last few years, a liberal education was beyond the reach of average women. Among the uneducated classes, is it not frequently the woman who is the manager, the shrewd head of the household? It would be fairer to measure relative power where both sexes are comparatively untaught, rather than in the classes where the men have for generations had the benefit of schools and colleges, while their sisters were indulging in "vapours" and "sensibility." Of course, there are plenty of fashionable fools still (whose waists are as small as their sense), but it would be rude to suggest that they might easily find their parallels among the loungers in St. James's-street, especially during the Derby week. I heard lately of a singular case of growth of the head of a student who has begun his work somewhat late; and, *pace* the artists, I hope that in a few generations our women's brains may show the result of broad education. If it is true that sons take after their mothers, the future Englishmen should not be the worse for increased intelligence and culture among those who make their homes.

M. McC.

PHRENOLOGY AND THE BRAIN.

[182]—I was very much pleased with your remarks on phrenology in "Answers to Correspondents" the other week, and must certainly concur with them. I think that the whole drift of modern physiological experiments tends to a disproval of the conclusions of phrenology. Dr. Ferrier's experiments, for instance, have shown that the individual convolutions of the brain are separate and distinct centres, and that "in certain definite groups of convolutions, and in corresponding regions of non-convoluted brains, are localised the centres for the various movements of the eyelids, the face, the mouth and tongue, the ear, the neck, the hand, foot, and tail." We have thus certain definite areas of the brain superintending various motions of the body, but we can find no trace of the numerous "esses" with which the phrenological vocabulary abounds. Certain lesions, moreover, prevent the voluntary movement of special groups of muscles.

J. H. H.

DARWINISM.—THE DESCENT OF MAN.

[183]—The Darwinian theory of the origin of man, of all forms of vertebrate life—that is, of all animals having a backbone and internal skeleton—is not, apparently from communications to KNOWLEDGE, so generally accepted as I supposed. There can be, then, no singularity in the apparition of another doubter.

"Vestiges of Creation" initiated the great public in the doctrines of the evolutionists. The work is a reputed hash of matter and ideas from German and French sources. The work had a great run, lived its day, died, and left no permanent impression. Mr. Darwin's unique works have influenced the entire civilised world. His profound investigations and remarkable method of developing his labours to the public gave Mr. Darwin, almost at a bound, a scientific and literary position such as few other men hold. But, with all this, I cannot reconcile myself to his theories, nor to the theories on which they are based.

I am neither biologist, nor ethnologist, nor anthropologist; I am simply one of the common herd of readers, with the difference, perhaps, that occasionally I venture on the more difficult labour of thinking. I may not have, but I presume I have, discernment enough to follow the lines of Darwin, and his co-labourers, so far as the steps of what, to me, is doubtfully the propylæum of the impenetralium that conceals the secrets of the Almighty Intellect we name Nature, and adore as Jehovah, Lord, and God.

"Vestiges" excited theologians, but the "Decent of Man" provoked them to rancour. The forcibly disagreeable, which the "Descent" is to very many, bites into the mind; the feeble slides over it. But the idiocrasy of the caste, the fenced lines of thought they habitually move in, blinded them to the actuality. They beat, and continue to beat themselves against a gross misapprehension of their own. Mr. Darwin presented his thesis as an argument to a probable conclusion; not as demonstrative of the absolutely certain. He has not arrived at a perfect induction, and makes no pretension to it, that I perceive.

Darwinism is based on the labours of the German transcendental anatomists, and on later theories by Étienne Geoffroy. Mr. Darwin elaborated on their principles by an *organon* of his own.

Geoffroy's epigrammatic enunciation, "There is but one animal, not many," involves the whole case of the evolutionists. The plain meaning of this dictum is, that all animals of the vertebrate class are formed on one plan; that all animals are a primogenial animal, repeated through time, in modified plan.

Goethe's theory is apparently identical with Geoffroy's; but the revelation of the German comes to us a great thought, in chaste grandeur of expression." "Comparative anatomy," he writes, "has united all organic nature under one idea; it leads us from form to forms, and while we contemplate near or far-removed natures, we rise above them all, to see their individualities in one ideal type." The entire literature of biology can show no equal to this unfolding of the transcendental idea of evolution.

This is Darwinism, in my conception of it. The "ism," then, rests on the assumption that all animals are descendants from one common ancestor.

Linked with this fundamental, there are four propositions we may expect without endangering any agreement to negative the fundamental. Indeed, save to present the case of the evolutionists in completeness, they need not have been brought in here. They are.—1. That no two animals are completely identical. 2. That offspring tend to inherit peculiarities of parents. 3. That of animals brought into existence, but a small number attain maturity. 4. That those which are best adapted to the circumstances in which they are placed, are most likely to leave descendants.

Wallace, who worked on lines conterminous with Darwin's, without being conscious of the fact, put this matter in a much better manner. He postulates: 1. Peculiarities of every kind are more or less hereditary. 2. The offspring of every animal vary more or less in all parts of their organisation. 3. The universe in which these animals live is not absolutely invariable. 4. The animals in any country (those, at least, which are not dying out) must at each successive period be brought into harmony with the surrounding conditions. "These," he affirms, "are all the elements required for change of form and structure in animals, keeping exact pace with changes of whatever nature in the surrounding universe. Such changes must be slow, for the changes in the universe are very slow."

I have, I believe, now put before you, briefly but accurately, the basis and principles of the evolutionists—of Darwinism, so called.

The first, I might say the only, difficulty of the theory is the foundation. That all animals are the issue of one common parent, and are but varied plans of that parent, brought about by time and change of telluric and climatic conditions, is a postulate that has no support in tangible fact, or appearance of fact, may be as safely affirmed as we may affirm that it is the expression of a phantasy of brains driven to overreach their powers.

What was the first-born animal, the common parent of all animals? How did it come to be, and in what form did it appear?

There is no answer to these two questions: to neither of them.

If we could rid ourselves of Pasteur's disproof of spontaneous generation, we might imagine a segregation of atoms that should acquire life, and develop into a caterpillar-like creature, which would pass through several transformations, embryonic, we may say, after the manner of butterfly development, ending in an animal of some kind, even man—the requisites, heat and food, being present. Very big things are produced from very small germs. Admit a beginning of this kind, we admit probability of innumerable various organs, and so account for every distinct animal in creation; for the endless kinds of insects, and the living myriads of the sea. In reality, the admission would put us in accord with the prodigious fecundity of Nature.

Then, how shall we account for the many kinds of vegetable forms? the ash, the oak, the beech, the elm, the pine, the birch? Are they evolutions from each other, or from imaginary trees of a long past epoch of earth-history?

But my main object is man. How came he? whence? and in what shape? Settle that, and all is settled. Darwinism tells us he is a lower animal, moulded by time and environments into his present form. The popular chant, "The long-tailed ape was the primal shape that led up to Adam and Eve," is not exactly an expression of Mr. Darwin's notion, though it is of Monboddo's. But Mr. Darwin might just as well have assigned us to an ape origin, as to a featureless thing of his own prolific imagination.

The origin of man has not been reasoned to. We cannot go back to it, fact by fact, for the needed facts are not. From first to last, we run through conjectures, and arrive at nothing but a final conjecture, worth no more than the conjectures that preceded it.

Whether the Caucasian man and the negro man have the same origin, is a problem standing from the general question for separate solution. Following the germ theory I have hinted at, would the germ from which the Caucasian would issue be one and the same, chemically, with that from which the negro would issue? But hinging on this is another question, Did the men appear contemporaneously?

Here, however, I must stop without completing my design. I have, I fear, already overrun the space I should have confined myself to.—Yours faithfully, B. Donbavand.

Picton, Chester.

Queries.

[113]—Gravity.—Does the orthodox definition of mass in terms of gravity $gM = W$ mean that it requires $\frac{1}{32}$ part of the weight of a body to overcome its inertia?—Zares. [It is a numerical statement, not a definition.—Ed.]

[114]—Mass.—If a weight, say one ton, be suspended by a perfectly flexible line of infinite length, what force in pounds would overcome the inertia of the mass in a direction at right angles to the direction of gravity?—Zares. [Any force, however small.—Ed.]

[115]—Inertia.—How is it that when I make 10,000 gallons of water pass over a perforated plate per minute, in a level position, there is a loss of 20 gallons per minute, but when the velocity of the water is reduced so that only 5,000 gallons per minute pass over, that then there is a loss of 250 gallons per minute through the perforations?—N.B.—50 per cent. of the plate has been punched.—Zares.

[116]—Chemical.—(1.) Why is the nitrogen contained in admixture with the oxygen in atmospheric air not absorbed by the lungs, as is the oxygen? (2.) If—as stated by Dr. Pavy and others—the value of hydrates of carbon as food is to be estimated *only* by the amount of unoxidised material they contain, what is the part played in the living body by the portion of the food already oxidised? Starch and sugar, *e.g.*, contain about 49 per cent. of carbon and hydrogen, and 51 per cent. of oxygen; so that but a very small amount of carbon remains unoxidised and available for combustion in the organism. What becomes of the bulk of such food?—E. M.

[147]—B. Sc. Examination, Edinburgh University.—I am desirous of obtaining information regarding the degree of B. Sc. at the Edinburgh University Examination. Would any of our correspondents kindly inform me through the medium of Knowledge what the subjects of examination are, and if it would be possible to pass without attending the University? If not, how long would I possibly have to attend, and what would be the probable costs for fees, books, &c.? I have attended evening classes in connection with the "Science and Art Department," and have passed first class in the elementary and advanced stages of chemistry (theoretical and practical), electricity, and acoustics, light and heat. Would the passing of these examinations be of any assistance? I have also an elementary knowledge of geology, botany, zoology, theoretical mechanics, and mathematics. An answer to the above will greatly oblige—A Philosophical Brushmaker.

[148]—Brewing.—(1). Having been in the habit of separating dextrose from cane-sugar by dissolving out the former in common alcohol, and having read in Prof. Graham's "Chemistry of Bread-making," that dextrose is *less* soluble in alcohol than cane-sugar, I am anxious to ascertain which sugar is the more soluble in the said reagent. (2.) Does basic acetate of lead precipitate dextrin? All text-books I have read state so, and yet I cannot obtain a precipitate on adding small or large quantities of the said salt of lead to either dilute or concentrated solutions of dextrin.—E. M. D.

[149]—Diameter of Sun.—As we cannot see the half of a globe whose diameter is greater than the width of our eyes how much larger is the real diameter of the sun than the diameter we see? As I have not seen this taken into account in any book I have read, an answer would oblige, yours truly. A. B. J. [The ratio of real diameter to apparent is that of tangent to sine of apparent angular radius, or, say, of 16 minutes. The logarithm of this ratio is 0·0000047, the number corresponding to which is about 1·000001; so that the sun's real exceeds his apparent diameter by about one-millionth part, or rather less than a mile.—Ed.]

[150]—Alpha Cassiopeia.—Can you tell me what is the magnitude of the companion of Alpha Cassiopeia?—Harris. [Estimated as about eleventh magnitude—bluish in colour.—Ed.]

[151]—Jordan Barometer.—What is the density of the glycerine in this, as used in the *Times* office? I make it 1·262 by calculation. Is this pure glycerine?—C. T. B.

[152]—Nickel-plating.—How is this done on iron without a battery?—C. T. B.

[153]—Cessation of the Sun's Heat.—It was stated by Sir J. Lubbock, at the last British Association meeting, that, after the lapse of seventeen million years, the sun would be cooled down to such an extent as to cease to emit light and heat. Will you kindly inform me on what grounds this is ascertained, and, if true, what must be the ultimate condition of the solar system, especially that of our own globe?—A. von Roulte.

[153]—Doubtful Organisms.—Kindly give me the names of those organisms, the nature of which, whether plant or animal, is doubtful.—H. J. C. W.—[Are any organisms doubtful, according to the modern definitions of plants and animals?—Ed.]

[154]—FROGS. Why are frogs excluded from the class Reptilia? —H. J. C. W.—[We always supposed that the class Reptilia included the Amphilia, and that frogs were classed as Batrachian Amphibia.—ED.]

[155]—TORTOISES.—What is the average duration of life of the tortoise, and how long can they remain under the earth?—H. J. C. W.

[156]—WORMS.—How is it that when a worm is severed in two, its parts still show signs of activity?—H. J. C. W.—[If you consider the nature of the nervous system in worms (and all articulated animals) that a double chain of ganglia, one may almost say of brains, runs along the body, you will see that the phenomenon is natural enough.—ED.]

[157]—ALUMINIUM.—The characteristic ingredient of common clay—what is the present process of obtaining this metal, or is there a treatise published on its extraction? Will any one of the metallurgists of KNOWLEDGE kindly furnish the information to one who believes that (by a new process) the price of aluminum might be reduced to that of copper?—LUNA.

[158]—A 1 lb. weight is carried to the top of a tower. Would this, if allowed to fail, raise another 1 lb. from the ground? They are connected by a thread passing over a pulley. Neglect, of course, the weight of thread, and assume it to be perfectly flexible and to move without friction.—H. ROLFE.—["H. Rolfe" should read a description of Atwood's machine. The weights would not move unless some impulse were communicated to one or other. Such an impulse downwards on the upper weight would cause uniform motion of both weights, until the one which had been uppermost reached the ground.—ED.]

[160]—BOTANY.—Can any one tell me who was the author of "The British Garden," a descriptive catalogue of hardy plants, indigenous or cultivated, in Great Britain, with Latin and English names? Two vols., published by S. Hazard, of Bath. 1799.—EUPTERIS.

[161]—HEAT FROM THE STARS.—Does the earth receive an appreciable, or any amount of heat from the stars?—STUDENT. [From measurements of the heat of a few bright stars and comparison with heat received trom sun, as also of light of star with light from sun, the inference seems sound that the total heat received from stars, bears to solar heat something like the same proportion that the light received from all the stars bears to the sun's light.—ED.]

Replies to Queries.

[79]—MENTAL PHYSIOLOGY.—As "S. S. S. S." seems anxious to test the accuracy of my information concerning Mr. Cyples's book, I can only tell him how it came under my notice, which was by press reviews, particularly one from Prof. Croom Robertson's organ, *Mind*, which carried conviction to my mind that a very unique and able work on the subject was referred to. I at once obtained it, and (as stated before) on the *mental* side, and as being exactly in keeping with its title, have never had any regret in so having purchased it. It would be out of place here to say further. If he went to Strahan's and asked to see the book, he might get a little idea thus of its character, &c.—S.

[125].—It *is* possible to prepare indigo on the large scale, but, unfortunately, *not* profitably. The well-known German chemical factory at Baden succeeded in producing artificial indigo, which made its appearance in the market as a commercial article, but it was announced in the newspapers a week or two ago that operations have been discontinued, as at present it cannot be made to pay.—H. GRIMSHAW.

[136]—SUNLIGHT ON FIRE.—The draught of a fire depends in great measure on difference of temperature between air in chimney and air in room. Sunlight, by heating the air round a fire, tends to create a draught in wrong direction. A smoker's draught, being created by the lungs, does not seem a parallel case.—C. T. B.

[71]—NAMES OF FLOWERS.—Hayward's "Botanist's Pocket-Book," 4s. 6d. (Bell & Sons), gives both scientific and common names, with concise characteristics. Dr. Hooker's "Student's Flora," 10s. 6d. (Macmillan), though giving fuller particulars than any other handbook, contains comparatively few common names. Bentham's "British Flora," 12s. (Reeve & Co.), contains many common names and a complete "English scientific nomenclature," also outlines of botany, &c.; but it includes fewer species and varieties than either Hayward's or Hooker's.—EUPTERIS.

[80]—THE CHIN.—"Ethnologist's" observations are not in accordance with the remarks of Lavater, who says that "flatness of chin bespeaks the cold and dry; smallness, fear; and roundness, with a dimple, benevolence." Elsewhere, he adds.—"A long, broad, thick chin—I speak of the bony chin—is only found in rude, harsh, proud, and violent persons.—EUPTERIS.

[90]—THE MAGNETIC NEEDLE.—I believe it is considered that a current of electricity is constantly passing round the earth from east to west, causing the magnetic needle to point north and south. Similarly, a wire from a battery, passing over or under the needle, will make it stand at right angles to the current.—EUPTERIS.

[105]—"J. S." can obtain all the information he requires from Mr. James English, naturalist, of Epping village, who is to be congratulated as the discoverer of a very ingenious, and, I believe, the only sure process of preserving fungi and lichens, and not only these, for in his hands even flowers of the most delicate structure and hue retain all the grace of form and richness of colour provided by nature. Having myself seen many remarkably perfect and beautiful examples of Mr. English's work on both fungi and flowers, I can recommend (as it gives me much pleasure to do) your correspondent and others interested in this really valuable discovery to communicate with him. In consequence of the great success of the method, and the large demand following upon it, I understand Mr. English is engaged upon publishing it in full—a generous act for the benefit of the world at large—and the subscription-list is open to any wishing to obtain a copy of his book. It only remains for me to add that, personally, I am entirely disinterested in the production, and offer this information as well for the profit of those botanists who are as yet unacquainted with this somewhat obscure but worthy worker, as for his own reward, and knowing that every nature-loving student will be glad to learn that the longed-for end has at last been secured, and that he can, without difficulty or expense, avail himself of its accomplishment. It may also be here recorded that it was the same industrious and deserving naturalist to whom we are indebted for introducing the largely-adopted practice of "sugaring" for moths.—W. W., Highbury.

[105]—FUNGI AND LICHENS.—In answer to "J. S.," in last Friday's KNOWLEDGE, Mr. D. Bogue, publisher, 3, St. Martin's-place, Trafalgar-square, publishes a book on British Fungi, by M. C. Cook, M.A., LL.D., which is, I believe, generally considered to be an excellent work.

[108]—THE EYE AS ONE OF THE SENSES.—In answer to "T. T." (query No. 128), the same publisher advertises a little book by Dr. Dudgeon, entitled, "The Human Eye: its Optical Construction popularly explained." Price 3s. 6d.—R. T.

[110]—THE COAL AGE AND THE EARTH'S INTERNAL HEAT.—The fact that in the lower levels of the silver mines on the "Cornstock" lode in Nevada (say 1,600 feet from the croppings), the men can only work fifteen-minute shifts, owing to the heat, may throw some light on this subject.—E. F. B. HARSTON.

[110]—THE COAL AGE AND THE EARTH'S INTERNAL HEAT.—The theory that the earth was once in a state of fusion, has many sound arguments in its favour, one being that the oblate-spheroidal shape of the earth was produced by the exertion of the centrifugal force on the molten mass of the earth, like a similar shape would be produced if a mop were whirled rapidly round in the hand. We have the evidence of geysers and hot springs, and, to some extent, of volcanos and earthquakes, that some of the original fire in the earth's internal regions still exists. The increase of temperature as we descend into the earth is 1° F., for every 50 or 60 ft. The gigantic flora of the coal period was perhaps as dependent on the internal as on the external heat which was exerted on it; that is, supposing, as most likely was the case, that the regions of fire in the earth were, in those remote ages of time, much greater than now,—each age witnessing a decreased space of internal heat compared with the preceding.—HERBERT R. WELLER.—[The answer is rather crude, but some of the relations indicated are worth studying.—ED.]

[131]—CREATION.—Read also Lyell's "Antiquity of Man" and Lubbock's "Pre-historic Times."—PAUGUL.

[133].—THE *Wesleyan Methodist Magazine* for science article every month, *Good Words*, of coming year, for articles on "Science and Religion"; also "Proceedings of the Royal Society," for a year or so ago, contain—or will contain—articles by the Rev. W. H. Dallinger. None of Mr. Dallinger's lectures have been published, I believe.—PAUGUL.

[134]—CHEAP TELESCOPE.—Complete instructions to make a telescope would, I fear, be too voluminous for this journal, requiring, as it would, a page or two; but if the querist will write to me I will give him the information he wishes for.—PAUGUL.

[136]—SUNLIGHT ON FIRES.—It is useless to quote housemaids about such things. It is absurd to suppose that sunlight prevents a fire or a cigar from burning. The fire will appear to be out when a bright light shines on it, and you *let* it go out when the sun shines, because it is warmer then than at other times. The fire in my study is in sunlight every day (if the sun appears), and sunlight has never yet put it out. I can also testify that in India the sun never puts out either fire or cigar.—PAUGUL.

[141] TIME OF GLACIAL EPOCH. This querist need not trouble himself about any other theories than Croll's. There are others, however, and if he is ambitious of wasting some valuable time he may read Col. Drayson's "Last Glacial Epoch." PATGUL.

NAMES OF STARS. In reference to letter 127, page 143, there is no work in English on the names of the stars. There is one in German, by a writer named Ideler, and is called "Ueber den Ursprung und die Bedeutung der Sternnamen" "On the Origin and Signification of Starnames." It is a pity that there is no translation of this work into English. Of the great interest scientific, archæological, and poetic, attaching to the names of stars, there surely can be no question.—ZETA.

WEATHER GUIDE. (Page 128). Allow me to mention that Messrs. Routledge publish just such a guide, entitled "A Manual of Weathercasts," 1s. EUPTERIS.

Answers to Correspondents.

**** All communications for the Editor requiring early attention should reach the Office on or before the Saturday preceding the current issue of* KNOWLEDGE, *the increasing circulation of which compels us to go to press early in the week.*

HINTS TO CORRESPONDENTS. 1. *No questions asking for scientific information can be answered through the post.* 2. *Letters sent to the Editor for correspondents cannot be forwarded; nor can the names or addresses of correspondents be given in answer to private inquiries.* 3. *No queries or replies savouring of the nature of advertisements can be inserted.* 4. *Letters, queries, and replies are inserted, unless contrary to Rule 3, free of charge.* 5. *Correspondents should write on one side only of the paper, and put drawings on a separate leaf.* 6. *Each letter, query, or reply should have a title, and in replying to letters or queries, reference should be made to the number of letter or query, the page on which it appears, and its title.*

KAY. Your suggestion, that ladies should meet for discussion of matters scientific, educational, phrenological, and physiological, instead of tea and small talk, is no doubt excellent; but our space is too crowded for the insertion of your remarks in full.—ZARES. We agree with you that all paradoxes should not be omitted from a paper like the present. We propose to act in accordance with that view.—T. PRESTON BATTERSBYE. From some of my published essays, you will see I take great interest in the phenomena of mesmerism: I would like much to see your papers.—W. A. C. What we said about vivisection was very moderate. We, of course, have nothing to say to the change of law, which you suggest as logical. If you had ever known the torture of one dear to you alleviated through knowledge acquired from such experiments as you denounce, you would possibly see that the question has two sides to it. ONE INTERESTED IN SCIENCE. Heat waves and light waves are of the same nature, and both travel through the ether. Many light waves are heat waves, and *vice versâ*. Your second question belongs to a region outside of knowledge. The gratings referred to by Prof. C. A. Young, are what are called refraction gratings, in reality, series of fine parallel lines cut on glass very close together. The formation of a spectrum by means of such a grating requires a fuller explanation than we can give here. A collimator is a portion of the spectroscopic apparatus by which the rays are made parallel before entering the spectroscope proper.—GUN. The dimensions of space may be described as length, breadth, and depth. In a plane we have only two dimensions, length and breadth. Some geometers think they can imagine the possibility of a fourth dimension. When they can show us that a point may be neither in a plane nor out of it, we may begin to think with them.—W. WILSON, M.A. Is it not purely a question of words? You would say, we see the light that comes from an object; others would say (and, I think, rather more correctly) that we see the object by means of the light.—HENRY WENTWORTH MONK. You suppose I "have not forgotten publishing" for you "in the *English Mechanic* about nine years ago;" I remember writing a short notice in the Paradox Column of your theory of Re-Creation, but "publishing" for you would have been a different matter. I have read your letters headed "The God of Israel" and "The Lord of Hosts" in the *Jewish World*. Thanks for sending the paper to me; but the letters are hardly suited for notice in these columns.—F. P. No; at least that was not the name given. Sleep preventatives are not safe. Drowsiness (unless following after too heavy a meal, the use of stimulants, opiates, and the like), means that rest is needed, and that, therefore, rest should be taken. Your other query would hardly be understood. Even Dr. Andrew Wilson could not, off-hand, tell you the name, origin, and habits of creatures about which you only say that they are minute, have developed themselves recently within your aquarium, are white, have numerous legs, and dart through the water with a jerky, spasmodic movement, some of them carrying what you suppose to be their young upon their backs. But if you were to describe briefly the original contents of your aquarium, give the exact number of legs, and state something definite as to the size and shape of the small white creatures, your query should appear.—HARRY WHATES. We should have to insert rejoinders if your article were published. We have definitely stated more than once that our subject is science, and that in these pages the religious doctrines of no sect whatever shall be either attacked or defended.—E. BURKE. Declined with thanks; no space.—R.T. Thanks; but we ought not to insert what amounts in fact to a definite advertisement of the books named. Cause of curved shape of rainbow has been explained in recent numbers. Along lines inclined at a certain angle to the line from sun to observer (they make an acute angle with this line produced) come the rays giving, after internal reflexion in rain drops, each particular colour. Therefore, the rainbow arc of that colour is a circle on the sky, having the point directly opposite the sun (with reference to the observer) as its centre. Loomis's "Treatise on Practical Astronomy" is, we believe, to be obtained of Trubner, if not published by them. Its price is 8s. or 8s. 6d. —W. B. RUSSELL. The promised paper on the subject of moon's former proximity to the earth shall presently appear. (The moon was not projected from the earth, according either to this theory or to any other regarded as admissible by science.) No. 7 can still be obtained from the publishers.—TOPER. Your question is vague. You can get a useful knowledge of the elements of astronomy with the time you mention as at your disposal, if you use it well. But the books you have are not very well suited for your purpose. Herschel is too difficult, and the "Elementary Lessons" (though written by one who has done excellent work in some departments of astronomy) has too many errors in it to be of much use, unless, indeed, you could get from Appleton's, New York, the American edition, in which the errors arising from the author's want of familiarity with mathematical and theoretical astronomy have been corrected. —EXCELSIOR. We thoroughly agree with you, though we have no room for your letter. Whipping means, in ninety-nine cases out of a hundred, laziness and bad temper on the teacher's part; where the teacher is also a parent, you may say a hundred instead of ninety-nine.—AMICUS SCIENTIÆ. We have already reprinted back numbers, but third editions are rather costly affairs.—M. J. H. As a rule, extra outlay for larger size telescope will repay. Few ever buy a telescope who do not before long wish they had bought a larger one. I would advise you to get the largest and best you can afford. Such an instrument as you describe would do a great deal of interesting work for you, if the object-glass is by a good maker. The question about nebulæ and double stars is vague. Every telescope will show some double stars, and give interesting views of some nebulæ. See answer to "Amicus Scientiæ" as to back numbers.—J. C. LLOYD. The constellations revolve around the pole, the pole itself remaining unchanged in position. The pole-star revolves in a small circle around the pole.—M. M., *alias* J. H. Have we not requested that you would put outside your letters an address to which they may be returned? Did you think we were blind, that we should not see through so thin a disguise? —W. SCOTT. Mr. Allen shows that where such and such qualities appear, such and such results follow. If the results you suggest followed from the silveriness of whitebait, natural selection would cause the species to become less silvery, generation after generation. But doubtless the colour is protective. In what you suggest as to creatures animate or inanimate having anything to do with the matter, of their own will, you are, of course, joking.—WILLIAM FREDERICKS. Is there a bump of spelling in your phrenological system? Or are we to suppose that the "prooff" and "infamation" you require are some articles of which we have not yet heard? We have not asserted that "to term a system a science it must have adherents among men of science," because that needs no assertion, and requires no proof. To be scientific, a system must be able to bear scientific tests.—J. BARETZ. You say, amongst other remarkable things, that the celebrated Courvoisier was a great admirer of Gall and Spurzheim: surely you must mean Corvisart. Courvoisier was a murderer.—W.B.G. You are right, but the fact is pretty well known that Archimedes showed the sphere and circumscribing cylinder to have equal curved areas, as a preliminary to establishing the relation between their volumes. It has never yet been shown that the π relation in the great pyramid should replace that given by Herodotus as the one determining the pyramid's height—namely, that surface of each face is equal to square on diameter. The pyramid fulfils this quite as closely as the other.—COSMOS. We sympathise with your views; but we have to combine several qualities in order to appeal to as many as possible.—W. CAVE T. Longmans, Chatto & Windus, and Smith, Elder, & Co.—S. E. O'DELL. No, sir. We are obliged to you, but desire no continued articles in favour of phrenology, or against t

either.—ZARES. When did we promise to insert authentic abnormal mental phenomena? We promised to insert accounts of scientific experiments relating to mental matters. The story you relate, "told you by a reliable person," can hardly be so described. It is, in fact, one of those which we considered likely to reach us too freely if we opened the columns of KNOWLEDGE to accounts of spiritual manifestations.—H. WOOLLEY. We are inclined to agree with you, perhaps because it would save us much trouble to exclude such letters as you refer to. AMOS HINTON. Because fresh cold air continually replaces that which had been in contact with the body, which in a calm would be warmed. A CROSSLEY. The nebular hypothesis of Laplace is seldom correctly explained. An interesting account of it is given in Nichol's "Architecture of the Heavens."—G. H. MAPLETON. Printing the star-maps on a separate loose sheet involves extra expense, and this is a rather important consideration in a journal so cheap as ours.—A. T. C. Absolutely impossible to find room for your solar puzzle. But it is certain that if you had got up while the phenomena were in progress, and looked through the holes, at the sun, you would have seen some object, near or far off, obscuring his disc.—OSWALD DAWSON. You require our correspondents to be somewhat too precise. They know what they mean, pretty well, when they speak of the relative position of the sexes (to take one of your examples); why insist that they should define when, where, and how, in precise detail, they mean the sexes to be compared.—A correspondent, who gives us no name, asks us to explain the electro-magnetic theory of light. We know of no such theory. The writer who says the undulatory theory is fast being swallowed up piecemeal by the electro-magnetic knows very little about the matter. The evidence for the undulatory theory is simply overwhelming.—W. E. BLYTHE. Thanks for abstracts, which shall appear.—FRED. DENIER, Milwaukee. Thanks for encouraging words.—HARRIS, J. J. BRUM, and J. P. GILMOUR. Thanks; one sees the silver side, the other the golden: but it is the same shield.—J. CALVERT. Your advice to the eminent professor, coupled with that which you are good enough to give us, brings to the mind, somehow, the instructive lines:—

Teach not a parent's parent to extract
The embryo juices of an egg by suction;
The good old lady can the feat enact
Quite irrespective of your kind instruction.

A. J. MAAS notes that he receives KNOWLEDGE regularly every Sunday in Switzerland, so that booksellers who supply it in England on Tuesday or Wednesday might presumably do better. —EDWIN WOOTON. Fear we cannot in any way advance your scheme. Personally we are not in love with the society system of science work. Most scientific societies seem, somehow, to act as nurseries of disputes and difficulties.—RAVEN. Whether the account you refer to is reconcilable or not with the theory of evolution is a question not open for discussion in these columns. The account, whether right or wrong, is extra-scientific.—F. S. C. Your original polygon was an octagon. The polygon which your later communication requires could be readily drawn if any angle could be trisected, otherwise not. Consider the trouble taken by Euclid in Book IV. to show how polygons whose sides subtend particular angles may be described, and you will see that we cannot give as part of a solution such a direction as this: "The apex of angle being at centre of circle, inscribe a polygon which shall have three of its sides between sides of angle, commencing the polygon at one of the sides," without showing first how this is to be done. Your other communications thankfully received.—H. C. (i.) The accepted theory of light is that it arises from an undulatory motion in an ethereal medium occupying all space. (ii.) Cold water is heavier than warm. (iii.) We have heard of no new theory respecting the formation of the coal-measures. (iv.) I.H.S. stands for *Jesus hominum salvator*.—PHILADELPHUS. The writer of the article in question in no sense infringed our rule. He puts it as a scientific view that faith in dreams as supernatural visitations is one among many survivals of rude primitive philosophies. He indicates also pretty clearly his own belief that the phenomena of dreams are all readily interpretable without any appeal to the supernatural. This is unquestionably the attitude of science in the matter. I, at any rate, should be very much surprised to hear that any man of science viewed the matter differently. Well, now, you quote certain statements which do not seem reconcilable with these views. But science has nothing whatever to do with those statements. They are entirely extra-scientific. You might as well quote other statements, found in company with those you mention, to show that an account here of scientific views respecting floatation must be regarded as a breach of our rule that dogmatic religion should not be attacked in these columns. Or a Brahmin might as reasonably object to the views about distracted attention under head, "Mind Troubles," that they seem to him inconsistent with correct views about the Nirvana. Can you not see that the supernatural has no place in arguments relating to the natural? As to the inconsistency you indicate, we may or may not, Mr. Clodd might or might not, agree with you. What can it matter one way or another, when we positively decline to have such questions discussed here? In reply to your other question, Whitaker's Almanack decidedly overrates the prolificness of Bradford folk. It should be not 83·1 but 38·1 per thousand.—F. STANLEY. Newton's estimate of terrestrial compression was based on an incorrect hypothesis as to density at different levels below the surface. Joyce is not an authority, any way. The true compression is about 1-300th, or polar radius about 13¼ miles less than equatorial. A. J. MARTIN. Rightly understood, what we said was an answer to your question. The focal image of a planet is examined by the eye-piece (which is really a microscope), and cannot be examined with an eye-piece of more than a certain power, because its imperfections are such—no matter how excellent the object-glass—as to preclude more than a certain degree of magnifying. By receiving the image on a screen, even were the screen perfect, we do not diminish its imperfections, and we lose light. Therefore, we cannot use a microscope in the ordinary way with any advantage. In fact, if an eye-piece is used to throw the rays on the screen, the image so formed can be best magnified by simply increasing the screen's distance from the eye-piece. We are then magnifying without any of the optical disadvantages which would result from using a microscope. But we find no increase of distinctness in this way after a certain convenient distance has been reached—only loss of light and such increase of all imperfections that the image becomes confused and indistinct.—PAUGUL. Thanks; but at present no space.—EXCELSIOR. If you only knew how much labour we should save by doing what you ask! But then many would say it was pure selfishness.

Notes on Art and Science.

A NEW VARIETY OF GLASS.—A Vienna chemist has recently discovered a new variety of glass. It does not contain any silica, boric acid, potash, soda, lime, or lead, and is likely to attract the attention of all professional persons on account of its peculiar composition. Externally it is exactly similar to glass, but its lustre is higher and it has a greater refraction, of equal hardness, perfectly white, clear, transparent, can be ground and polished, completely insoluble in water, neutral, and it is only attacked by hydrochloric or nitric acid, and is not affected by hydrofluoric acid. It is easily fusible in the flame of a candle, and can be made of any colour. Its most important property is that it can be readily fused on to zinc, brass, and iron. Is can also be used for the glazing of articles of glass and porcelain. As hydrofluoric acid has no effect on the new glass, it is likely to find employment for many technical purposes.—*Wiener Gewerbe Zeitung.*

AN ELECTRIC BUOY.—A daily contemporary states that experiments are being made in the Lower Bay, New York, with a new electric buoy, the invention of Mr. Bigler, of Newburg. Mr. Bigier, it appears, owns the patent of the old Courtney Whistle Buoy, the principle of which he combines with an intermittent light, the same power which blows the whistle being used to generate the electricity that furnishes the light. The rise and fall of the waves compresses the air inside the buoy. When this pressure has reached a certain point, it works a dynamo machine and burner furnished by the Edison Electric Light Company. This machine is supposed to generate enough electricity to show an intermittent light. When the pressure is exhausted by the action of the machine, which makes about 300 revolutions per minute, the light goes out until the pressure is renewed by the motion of the waves. The more violent the waves the more powerful the light, up to a certain point. Thus the light is at its brightest during a hurricane.—*Scientific American.*

THE WINTER FLIGHT OF THE SWALLOWS.—The swallow is one of the best known, and, therefore, most interesting, of migratory birds. Excepting when kept in confinement, this bird knows neither the extreme of hot nor cold weather. As soon as the cold weather approaches, he migrates with his family to a warmer climate, and again to the northward when the temperature of its second home becomes inconvenient to its sensitive existence. In England, as a general fact, the swallow does not arrive until the second week in April, and takes his departure about the middle of September. Before the time of their flight, they assemble in vast numbers in a comfortable locality, and are seen chattering very eagerly, as if holding a huge convention for the settlement of affairs before starting on their long journey. Although starting off

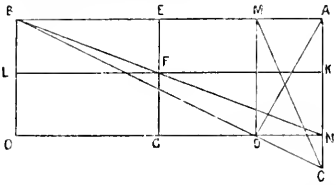
together, they do not remain so, preferring to separate into innumerable groups, like families or tribes, and sometimes making the long transit in companies of two, three, or five. While remarkable for the power and speed of their flight, they become fatigued in passing the sea, and will flock in great numbers upon the rigging of a ship passing their course for a rest. Sometimes the birds are so utterly worn out with fatigue, that when they have perched upon the side of a boat they are unable to take again to the wing, and, if disturbed, can scarcely fly from one end of the boat to the other. They have even been seen to settle upon the surface of the waves, and to lie with outspread wings until they were able to resume their journey. Guided by some wonderful instinct, the swallow always finds its way back to the nest which it had made, or in which it had been reared, as has frequently been proved by affixing certain marks to individual birds and watching for their return. Sometimes it happens that the house on which they had built has been taken down during their "season abroad," and in that case they exhibit a most pitiable distress, flying to and fro over the spot in vain search after their familiar domiciles, and filling the air with a mournful cry, announce to their friends that they have been dispossessed or evicted in the interest of local improvements. The swallow is widely spread over various parts of the world, being familiarly known throughout the whole of Europe, not excepting Norway and Sweden, and the northern portions of the continent.—*Frank Leslie's Magazine*.

Our Mathematical Column.

MATHEMATICAL QUERIES.

[11]—Can you inform me—(1.) Whether the axis of any cone passes through one of the *foci* of every ellipse formed by a section of that cone? (2.) Whether the two ellipses formed by the section of the two cones having a common apex and a common axis, by the same plane passing through both, at any angle to the common axis, are of similar eccentricity? (3.) Whether or not the angle of inclination of the ecliptic to the axis of rotation of the sun bears the same relation to the eccentricity of the earth's orbit, as the angle which the plane of any ellipse forms with the axis of its cone bears to the eccentricity of such ellipse?—NO MATHEMATICIAN.—[None of these relations hold. The simplest way to determine the foci is this:—Take a plane through axis of cone and at right angles to the cutting plane. A circle inscribed in the triangle in which the plane cuts cone and cutting plane will touch the axis of ellipse in one focus. The escribed circle touches it in the other.—ED.]

"T. R." sends an ingenious solution of No. 7, p. 148 (KNOWLEDGE, No. 7), in which he claims (erroneously) that no proposition beyond those in Euclid Book II. is employed. We have slightly modified the construction in what follows, in order that the figure may be more conveniently shaped, but the solution is in effect the same that "T. R." has kindly sent us.



We have in right angled triangle BAC, AD perpendicular to hypothenuse, DM, DN perpendicular to BA, AC; and we have to show that angle BMC = angle BNC.

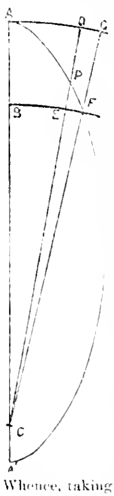
Rect. $BM.MA = MD^2$ (whereabouts in Books I. and II. is this proved? It might be given as a corollary from II., 14, but not without some proof bringing it within the range of Book III.). Hence, adding MA^2 to each, we have—

$$BA.AM = AD^2 = \text{(similarly) } AC.AN.$$

Complete rectangle $ANOB$, take $AK = AM$; draw KL parallel to AB, cutting BN in F; and draw EFG parallel to AN. Then rect. EO = rect. AL (FA and FO being complementary) = $BA.AK = CA.AN$. Hence BE must be equal to AC; and $EF = MA$. Hence triangle BEF is equal in all respects to triangle CAM. Thus angle ACM (= alt. angle CMD) = angle EBF = alt. angle BNO. Adding a right angle to the equal angles CMD and BNO, we have angle BMC = angle BNC.—QED. The proof is not so easy as either of those we gave, but it illustrates a useful method.—ED.

W. Ridd obtains a result, in examining the problem dealt with in query 92, p. 145, slightly different from ours. We gave for the eastwardly deflection of a projectile let fall from a height h, $\frac{2\pi ht}{P}\cos\lambda$ where t is the time of the fall, λ the latitude, and P the earth's rotation-period. He gets instead $\frac{2\pi(r+h)t}{P}\cos\lambda$. Mr. Ridd overlooks the circumstance that the point below moves eastward at such a rate as to be carried a distance $\frac{2\pi rt\cos\lambda}{P}$ eastward in time t, so that the actual eastwardly deflection is only the difference of these, or $\frac{2\pi ht\cos\lambda}{P}$. The result is not slightly, but very, different from that we gave, being more than $\frac{r}{h}$ times as great, so that if h be 88 yards, W. Ridd's result would be greater than mine in the same degree that the earth's radius, or about 3,960 miles, exceeds 88 yards, or $3{,}960 \times 20$ times, or 79,200 times! In fact, Mr. Ridd's error is the converse of Tycho Brahe's, who, in a letter to Rothmann, asked, "how it was possible that a ball dropped from the summit of a tower should always fall close to the foot of it, since the tower must have moved a considerable distance towards the east while the ball was falling; if the height of the tower were 100 feet, the falling body should strike the ground $1\frac{1}{2}$ miles westward from the foot of the tower, which is contrary to all observation."

But, as a matter of fact, the result we gave is only correct when we neglect the circumstance that during the fall the direction of gravity on the falling body varies, so that—first, the direction of the body's excess of eastwardly motion over the eastwardly motion of the point vertically below the point of suspension, is not always at right angles to the moving vertical, and, secondly, gravity acts during the fall to partly diminish this part of the motion. These may seem very unimportant matters, but, as a matter of fact, when they are taken into account, the calculated eastwardly deflection is found to be diminished from $\frac{2\pi ht\cos\lambda}{P}$ to $\frac{4\pi ht\cos\lambda}{3P}$.



We leave it as an exercise to the student to obtain this result by analytical methods. (If any difficulty should be found, we shall be glad to give the solution.) The following geometrical method will be readily understood by a larger number:—

Let A be the point of suspension, B the point vertically below it, C the earth's centre, BEF the earth's surface along a great circle through BA, and touching the latitude-parallel (or small circle) through B, so that BF may be regarded as part of this latitude-parallel. The body falling from A, with such eastwardly motion as belongs to the point of suspension A, travels in an elongated ellipse, AFA', having C, the earth's centre, as a focus, and reaches the ground at F, the arc APF being appreciably parabolic. Suppose that while this descent is taking place the point of suspension, A, is carried by the earth's rotation to D, and join DC and FC, DC cutting BF in E, and APF in P. Also let arc AD produced meet CF produced in G.

Then, since the point of suspension A, and the falling body when just leaving A, are sweeping out equal areas around C, and continue to sweep out areas uniformly during their motion (the former because of the uniform rotation of the earth, the latter by Kepler's second law), it follows that

Area ACD = area $APFC$.

Whence, taking away from each the area APC

Area APD = area CPF.

Whence approximately (since PE and EF are each very small, compared with AB, BF, &c.)

Area AD = area CEF,

or, approximately, $\frac{1}{3}AB.BE = \frac{1}{2}EF.BC$.

Whence $$EF = \frac{2}{3}\frac{AB.BE}{BC};$$

or, since $AB = h$, and $BE = 2\pi r\cos\lambda\left(\frac{t}{T}\right)$

$$\text{the easterly deflection } EF = \frac{2}{3}\cdot\frac{h}{r}\cdot 2\pi r\cos\lambda\left(\frac{t}{P}\right)$$
$$= \frac{4\pi ht}{3P}\cos\lambda$$

Our Whist Column.

BY "FIVE OF CLUBS."

THE LEAD.

THE customary way of treating leads at Whist is found perplexing by beginners. A number of suits are considered, and the proper lead from each is indicated, with perhaps the play second round; until the learner wonders how much he is expected to remember of what appears to him a perfectly heterogeneous collection of rules. Thus, take Captain Campbell Walker's very useful book, "The Correct Card." In this there are 36 cases of suits headed by an Ace, with the play for each; 18 cases of suits headed by King; 7 of suits headed by Queen; 9 of suits headed by Knave; 4 of suits headed by 10; and two of suits headed by a small card. In all, 76 cases are considered. The natural idea of the learner is that he ought to commit to memory all these 76 cases, with the exceptions noted in 19 notes, before he can lead properly; while, after that, he will have to learn an equally voluminous series of rules for play second hand, third hand, and fourth hand. He naturally despairs of accomplishing this without giving much more time to the matter than the game, good though it is, seems worth.

But even when the learner has committed all these rules to memory, he still finds that there is something—embodied, indeed, in them, but not obviously expressed by them—which it is absolutely essential that he should grasp. He requires to know not only what he should lead from a given suit, but what each lead means.

Now it does not seem to have been noticed by writers on Whist, that by beginning at this end they get rules much more easily remembered, because at once made practically available, and also much fewer in number. In point of fact, the rules which seem without system have a system at the back of them, and this system is at once displayed when we reverse the usual method of presenting the rules for leading, and begin by asking what particular leads may mean. Afterwards we may collect together a hundred or so of such rules and exceptions, as appear in Captain Walker's book, for then each rule will be easily remembered as a necessary inference from the principle on which the lead and the interpretation of the lead alike depend.

We begin then by considering, *not* the multitudinous leads from suits headed by an Ace, but from what suits containing an Ace, the Ace should be led. It will be seen that there are only a few cases in which Ace is led, and these easily remembered; and also that once we know when an Ace should be led, we know what the Ace lead means—

When, then, should an Ace be led?

From long suits, and from suits of not less than three, Ace is only led:—

(1) from Ace and four or more others (not including King).

(2) from Ace, Queen, Knave, with or without others.

From suits of two cards (which it can hardly ever be right to open—and never as an original lead), containing an Ace, Ace is always led.

From long suits, then, or suits of three, which only are in question in ninety-nine cases out of a hundred, there are only two cases to be considered. The play second round, supposing the Ace not trumped, will show from what sort of suit the Ace was led. For,

(1) If Ace is led from Ace, four or more, a small card is led second round.

(2) If Ace is led from Ace, Queen, Knave, and others, either the Queen or the Knave is played second round—the Queen, if the suit did not originally contain more than four cards, the Knave if it did.

Thus when your partner leads an Ace, you know at once that he has not the King. If you have the Queen or the Knave, you know he has not led from Ace, Queen, Knave, and therefore that he has four more cards in the suit.

If the lead is not an original lead, and the play has given reason to believe that your partner has been driven to a forced lead, the Ace may have been led from Ace and another. This can scarcely ever happen, but when it does happen the previous circumstances of the play and what follows the forced lead (together with the study of your own hand) will almost always show you that the lead has not been from strength.

In our next we shall consider *why* Ace is only led from long suits, under one or other of the circumstances stated above. We may note, indeed, in passing that on the Continent, Ace is led from Ace and three others (not including King), though the laws of probability point to the play as not the best. It is well to remember, however, when playing with Continental players, that this is the rule with them.

Our Chess Column.

TWO KNIGHTS' DEFENCE (*Continued.*)

IF White on his 8th move should play B. to Q.R.4., Black will soon obtain the better game, *e.g.*,

1. P. to K.4 / P. to K.4	2. Kt. to K.B.3. / Kt. to Q.B.3.	3. B. to B.4. / Kt. to K.B.3.
4. Kt. to K.Kt.5. / P. to Q.4.	5. P. takes P. / Kt. to Q.R.4.	6. B. to Q.Kt.5.ch. / P. to Q.B.3.
7. P. takes P. / P. takes P.	8. B. to Q.R.4. / P. to K.R.3.	9. Kt. to K.B.3. / P. to K.5.

In reply White has no satisfactory move, *e.g.*,

10. Kt. to Kt. sq. / B. to Q.B.4., or	10. Kt. to K.5. / Q. to Q.5., or	10. Q. to K.2. / B. to K.3.
11. P. to Q.B.3. / Q. to Q.Kt.3., or (*a*)	11. B. takes P.ch. / Kt. takes B.	11. Kt. to K.5. / Q. to Q.5., or (*A*)
12. Q. to K.2. / B. to Q.R.3.	12. Kt. takes Kt. / Q. to Q.B.4., winning the Knight.	12. B. takes Q.B.P.ch. / Kt. takes B.
(*a*) 11. Kt. to K.2. / Kt. to K.Kt.5.		13. Kt. takes Kt. / Q. to Q.B.4.
12. Castles. / Q. to K.R.5. with a winning game.		14. Q. to Q.R.6. / B. to Q.sq.
		15. P. to Q.Kt.4. / Q. takes Q.B.P.
		16. Q. to Q.Kt.5. / Q. takes B.ch.
		17. K. to K.2. / Q. takes R. with a winning superiority.

(*A*) 11. Kt. to Kt.sq. / B. to Q.B.4.	12. P. to Q.B.3. / B. to Q.B.5.	13. Q. to Q.sq. / Q. to Q.Kt.3.

And Black has a fine game.

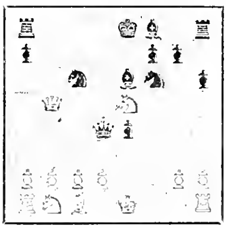
In this variation an interesting novelty has recently been discovered on White's 13th move, viz.,

1. P. K.4. / P. K.4.	2. Kt. K.B.3. / Kt. Q.B.3.	3. B. to Q.B.4. / Kt. to K.B.3.
4. Kt. to K.Kt.5. / P. to Q.4.	5. P. takes P. / Kt. to Q.R.4.	6. B. to Kt.5.ch. / P. to B.3.
7. P. takes P. / P. takes P.	8. B. to Q.R.4. / P. to K.R.3.	9. Kt. to K.B.3. / P. to K.5.
10. Q. to K.2. / B. to K.3.	11. Kt. to K.5. / Q. to Q.5.	12. B. takes P.ch. / Kt. takes B.,

instead of the usual move 13. Kt. takes Kt., which, as we have shown, results to the advantage of Black. White can also play 13. Q. to Kt.5.,* out of which reply some interesting variations arise.

POSITION of 13. Q. to Kt.5.

BLACK.



WHITE.

Should Black now play the likely looking move of 13. B. to Q.2., White would win by 14. Kt. takes Kt. / Q. to Q.3. 15. Q. to K.5.ch. remaining with two Pawns ahead. The proper reply to 13. Q. to Kt.5. is B. to Q.B.4.,† which we will examine. 14. Q. takes Kt.ch. / K. to K.2. White has now three moves at his disposal.

* Played at St. Petersburg, in a game between Prince Dadian, of Mingrelia, and M. Liselle.

† This defence was discovered by Signor Constantini, one of the strongest amateurs in Italy.

1.	2.	3.
15. Q. to Kt 7 ch. / K. to Q 3	15. Q. to B 7 ch / K. to B.sq.	15. Castles. / Q. takes Kt.
16. P. to K B 4 / P. takes P. e. p. or (1)	16. P. to Q B 3. / Q. takes P.ch. or (1)	16. P. to Q.3. / K. R. to Q B.sq.
17. Kt. takes P. / Q. to B 7 ch.	17. K. to Q sq / B. to Q B.sq.	17. Q. to Q R 4. / Kt. to K Kt 5.
18. K. to Q sq. / Q. takes Kt. P.	18. Q. to Kt 7 / Q. to K B.4. and wins.	18. P. to K Kt 3 / P. to K 6. and wins.
19. Q. to R 6 ch / K. to B.2		
20. Q. to K B sq. / B. to K R.6.		
21. P. to Q 4 / Q. takes Q.		
22. R. takes Q. / R. takes R.		
23. P. takes B. / Q. R. to Q sq.ch.		
24. B. to Q 2. / K. R. to K.sq.		
25. Kt. to Q.B.3. / Kt. to K.5.		
26. Kt. takes Kt. / R. takes Kt. Black wins.		

(1)
16. Castles. / R. to Q.B.sq.
17. P. to Q B 3. / Q. to Q 4.
18. P. to Q B 4. / Q. to Q 5.
winning the Knight.

(a)
16. Kt. takes B.P.ch. / B. takes Kt.
17. Q. takes B. / Q. takes P.ch.
18. K. to Q.sq. / Q. takes Kt.P.
19. R. to K.sq. / Q. to Kt.5.ch.
20. R. to K.2. / K. R. to K.B.sq.
21. Q. to Q.Kt.7. / Kt. to Q.4.
and wins.

If White on his 8th move should play Q. to K.B.3., Black ought, likewise to obtain the better game, *e.g.*,

1. P. to K 4. / P. to K 4
2. Kt. to K.B.3. / Kt. to Q.B.3.
3. B. to Q.B.4. / Kt. to K.B.3.
4. Kt. to K Kt.5. / P. to Q 4.
5. P takes P. / Kt. to Q.R.4.
6. B. to Q.Kt.5.ch. / P. to Q.B.3.
7. P. takes P. / P. takes P.
8. Q. to K.B.3.

In reply, Black has three moves, viz.,

8. —— / Q. to Q B.2. or	8. —— / Q. to Q.Kt.3., or	8. —— / P. takes B.
9. B. to Q R 4. / B. to Q 3.	9. B. to Q.R.4. / B. to K.Kt.5.	9. Q. takes R. / B. to Q.B.4.
10. P. to Q 3. / Castles.	10. Q. to K.Kt.3. / P. to K.R.3.	10. Q. to K.B.3. / B. to Q.Kt.2.
11. Castles. / P. to K R 3.	11. Kt. to K.B.3. or (4) / B. to Q.3.	11. Q. to K.Kt.3. / Castles.
12. Kt. to K 4. / Kt. takes Kt. to be followed by P. to K B 4., with a good game.	12. P. to K.R 3. / Kt. to K.5.	12. P. to Q 3. / P. to K.5.
	13. Q. takes B. / Kt. takes K B.P.	13. Castles. / P to Q Kt.5.
	14. Q. takes P. / Kt. takes R.	14. B. to K.3. / B. takes B.
	15. Q. takes R ch. / K. to Q.2., winning the Queen as he threatens Mate by Q. to B.7 ch., Q. to B 8.ch., and Kt. to B.7.ch.	15. Q. takes B. / Kt. to Q 4.
		16. Q. to K.Kt 3. / P. takes P.
		17. P. to Q B 3. / Q. to K.B.3., or to Q.Kt 3., and Black has a good game.

(4) If White should play Kt. to K.R.3., the following would ensue—

11. Kt. to K B. / B to Q 3
12. Castles. / Castles, Q R.
13. Kt. to Q.B.3. / P. to K.5.
14. Q. to K 3. / Q. to B 2
15. Kt. takes P. / B. takes P.ch.
16. K to R sq. / K. R. to K.sq.,

and Black has the better game. Of course he could not play 11 Q takes P.ch., for Black would reply with K. to Q 2., which would win, as he threatens R. to K sq., and the white Knight is *en prise*.

A. J. MAAS.—Thanks for end game, which shall be analysed.—H. A. N., R. KELLY, S. K. L. L., and others. Problem No. 5 cannot be solved, as you suggest, by 1. R. to Q.Kt. 7. If Black take Kt. with B., there is no mate, or Black may play Kt. to R.'s 5th ch., taking B. next move.—J. B. M. and S. K. L. L. In our note on problem 6, we should have said, "by adding a Black Knight at Q.'s sq., not Queen's 8th." Better consider, however, that a second solution is required, the position remaining unchanged.—D. SEE. If eight first moves are given as odds, none to be across the board, the receiver of the odds can make the game absolutely certain in a number of ways. We do not know that there is any way by which mate in a given small number of moves may be announced before Black plays a move. There may be, however.—H. A. L. S. Thanks. Your solutions of Nos. 7 and 8 correct, of course. The question was not as to the justice of rejecting No. 7, but of ignoring its author's statement, that, so far as he was concerned, it was original. As a part of the idea had been anticipated, there was a valid—though, we think, insufficient reason for declining to insert. You admit that the editor's is superior in finish, &c., to D'Orville's. The idea is one which would be apt to suggest itself to a problem composer, but in D'Orville's problem the solution can hardly be escaped; in the editor's there are several promising lines of attack.—JAS. D. MEEK. You are right. White could not draw; though, of course, he would not throw away his Rook by checking, as when Queen was made.

CURIOUS FREAK OF A DOG.—Dr. Onderdonk sends the following story of a freak of a dog to the *Scientific American*:—"Simmons" (that is the dog's name) is very remarkable for her sagacity, and often excites remark by the "reasonableness" of her actions. She is a constant companion of the boys, and seems to consider herself one of them. She has been a mother three times; the third time some ten days or so ago. At her two former *accouchements* she did herself credit by the respectable size of the family she brought to light; but this last time she gave birth to but one pup. Two or three days before the birth of this pup there was a litter of kittens born on the place. Simmons, disgusted at the smallness of her family, and evidently thinking that the cat had more than her share, captured one of the kittens in the absence of the old cat, and carried it in her mouth to where she kept her pup, and deposited it in her basket. In a short time she was suckling both the pup and kitten, who were hard at work side by side. The next day the kitten was taken away in the absence of Simmons, but on her return she hunted up her adopted child, and brought it back to her basket, where it has remained until now. Simmons has now been nursing the kitten for more than a week, the kitten seeming to be perfectly satisfied with her foster-mother.

Contents of KNOWLEDGE *No.* 9.

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Contents of No. 11.

A WINTER WEED.

By Grant Allen.

A DAY or two of warm weather, wafted to us by the westerly breezes, has brought out the daisies on the lawn as vigorously as if it were May, instead of January. The sward is dappled all over with their little timid white blossoms in a way that quite defies the decencies of the season. The fact is, modest and shrinking as they seem to be, daisies are very hardy and hard-working small plants, which never miss an opportunity of pushing their way in the world; and no doubt they have their reward, for probably no other flowering kind, except, perhaps, one or two grasses, have been half so successful in colonising the fields and hill-sides as these unobtrusive, wee things have been. In the spring, they are the very earliest plants to bloom; and since the early flower catches the bee, they begin setting their seed before the other blossoms are well awake; all through the summer and autumn they go on blooming uninterruptedly; and even when winter comes, they are ready at a moment's notice to take advantage of any brief gleam of sunlight which may happen to occur, putting forth their pretty buds fearlessly, and alluring the last stray insects of the season to visit their tiny golden bells.

Here in my hand I have grubbed up one entire daisy-plant, root and all, with my cane; and when one comes to look closely at its structure, the secret of its success in life is not difficult to decipher. In the first place, there are the leaves. These we seldom notice when we are examining a daisy, because they are so very retiring and unobtrusive. They lie flat upon the ground, in a small, round, spreading rosette, pressed as tightly as possible against the soil beneath. That is one of the tricks by means of which the daisy secures itself a place in the world. It grows generally in open pastures and commons covered with grass; and as the grass tends always to raise its tall blades as high as possible, the daisy might easily be overshadowed by that powerful competitor. Now, there are two ways in which different plants living in such circumstances can avoid being elbowed out of existence. One way is by sending up taller and bigger leaves than the grasses, so as to intercept the air and sunlight; and this is the plan adopted by such weeds as dock, burdock, coltsfoot, and some plantains. As a rule, however, such tactics can only be followed by plants which possess a reserve fund of food-stuffs laid by in their roots or stocks, for otherwise the young leaves would be choked and starved before they could grow high enough to overtop the competing grasses. Or, to put it more definitely, those kinds alone have succeeded in this way which happened to develope both large leaves and rich reserves of starch at one and the same time. The second plan is that followed by the daisy, the hoary plantain, and many other field-weeds. These plants have learnt to press their foliage closely down in a little circle upon the ground, so as to prevent any grasses from growing up around them and intercepting the sun and air. In other words, such individuals among them as happened to display this tendency, in a slight degree, survived the best; and among their descendants, such as carried it out further and further, spread most afield, while such as fell short of the desired quality, got killed off young by neighbouring weeds. Thus, at last, the daisy acquired its present successful habit of growing close to the ground, and so checking competition in the bud, or rather in the very seedling.

But why, it may be objected, do not all other plants do the same? The answer is, because all are not adapted for the same sort of life as the daisy. One kind survives in virtue of one point of vantage, another kind survives in virtue of another. The English meadow plantains are three closely-allied types of weed, hardly differing from one another in any essential point; yet each of them has solved this problem of foliage in a separate way. The great plantain sends up big, broad leaves on longish stalks, something like those of garden lettuce, which overtop most of its neighbours; the hoary plantain spreads a little tuft close to the earth, like the daisy; and the ribwort plantain meets the grasses on their own ground, by reducing its leaves to mere long, thin, lance-like blades. In each case, the explanation must be accepted on its own merits, without prejudice to different explanations elsewhere. The forms of leaves, indeed, are among the most difficult problems of botany, and it must not be supposed that we can account for them all at once by a single simple and easy formula. One might as well ask why the rabbit is not as big as the red deer, or why the fox is smaller than the lion. Each fills its own niche in nature; so each has been developed into exact correspondence with that particular niche and no other. And different means often subserve exactly the same end. The fleetness of the hare is produced by quite other adaptations than the fleetness of the stag; the foliage of the daisy succeeds by being compact and rounded, the foliage of the buttercup by being cut up into numerous small divergent segments. In short, whatever accidental habit happens to give a plant or animal any advantage in the struggle for existence is perpetuated in its descendants, and gradually perfected by natural selection, and thus the most diverse means often lead up in the long run to the same end.

The reason why the daisy is able to send up buds and blossoms at a moment's notice seems equally clear. The buds are always lying by in readiness close to the little perennial tufted stock. I cut it down the middle with my pocket-knife, and see, in the centre of the tuft, there are two or three unopened flower-buds even now lurking unseen and waiting for their turn to appear. Practically speaking, the daisy is an evergreen, for it always has green leaves upon it all the year round; and these green leaves are perpetually engaged, summer and winter, in manufacturing starch from the carbonic acid of the air, which starch is at once laid by in the root stock to feed

the young flowers when they are ready to sprout. So the moment a little warm weather arrives, the bud begins to start into life, and is supplied with food from the starch laid by in the root, as well as from the constant gains of the ever-busy leaves. All annual plants have to grow from the seed in a single season, and they have to produce a large number of leaves before they have digested food enough in these their expanded stomachs to feed the future flowers and seed; so that they cannot begin blossoming till comparatively late in the season. But the daisy, being a perennial, with slightly starchy root and practically persistent foliage, gets the start of them from the beginning, so as to put forth its flowers at the earliest possible moment.

And now let me look briefly at this flower itself. It is made up, as everybody knows, of two parts. The centre or disk is yellow, while the outer rays are white. But if one pulls it to pieces, one sees that the disk is really composed of many separate little golden bells, each one something like a harebell on a very small scale. The daisy head, in fact, is not one flower, but a whole lot of distinct flowers crowded together into a single truss. Taking one of the little central yellow bells in detail, I find that its petals are not separate, as in the buttercup, but are all united together into a long tube. The ancestors of the daisy had doubtless ages ago five distinct petals, like those of the buttercup; but at some time or other these petals showed a tendency to coalesce, and as this tendency proved useful to the plant, by more certainly securing its fertilisation by insects, it rapidly grew through survival of the fittest into a fixed habit, not only of the daisy, but of all the great group of flowers to which it belongs. The reason why the tubular shape is more useful than the arrangement with five spreading petals becomes clear enough if we recollect that the insect has to thrust his proboscis down to the bottom of the tube, past the pollen-bearing stamens and the sensitive pistil, in order to reach the tiny drop of honey concealed within. In doing so, a little of the pollen naturally adheres to his proboscis, and is then brushed off against the sensitive surface of the next blossom which he visits, so as thus to impregnate and fertilise its seed. To this day, however, the daisy still retains a reminiscence of the distant period when it possessed five separate petals; for each of the central florets has a vandyked edge of five points, these points being the last representatives of the original distinct flower leaves in its remote progenitors.

The tubular arrangement is common to many flowers besides the daisy family; but the daisies and their allies have carried their development one step further than the rest, for they have learnt to collect several tiny blossoms together into a single compact head, and thus to bid for the attention of insects far more powerfully than they could do in single display. More than that, in the daisy itself, and one or two others of its near relations, the outer florets of each head have become flattened into long ornamental rays, so as to play the part of petals to the compound group. In this way they make the little bunch very noticeable to all passing insects. The ray florets, when closely examined, look like tubes split down one side and opened out, so as to produce as much show as possible. They are the attractive part of the flower-head, and they do little active work themselves, having no stamens and no pollen, but laying themselves out mainly to look pretty alone. For this purpose they are coloured white, with pinky tips, instead of being yellow, like the central florets. Yet, of course, the whole plant is ultimately benefited by this arrangement, because the insects are thus induced to visit the entire little colony at once, and by carrying pollen from one floret to another, to fertilise the whole row of yellow bells then open. For if you look intently into a daisy, you will see that it does not open all over at the same time, but begins opening from the edge, and gradually proceeds towards the centre; so that in most daisies you will find a row or two of over-blown florets outside, a row just open or opening half-way through, and a lot of unopened little buds in the very middle. Doubtless, this arrangement also conduces to the good of the plant, by ensuring the highest and best sort of cross-fertilisation—that which is obtained by impregnating the blossoms of one individual with pollen brought from those of another.

PRECESSION OF THE EQUINOXES.

By the Editor.

THE precession of the equinoxes is, properly speaking, the observed motion of the points on the earth's orbit, where she is when the sun passes from north to south, or from south to north of the celestial equator, these points moving always in a direction contrary to that in which the earth circles round her orbit, so that they travel backwards. As precession really means going forwards, it may seem a little strange that this travelling backwards of the equinoctial points should have received such a name. But, as a matter of fact, the motion of these points (which are those where spring and autumn begin) in a direction opposite to that of the earth's motion, causes them as it were to meet the earth, shortening the time she takes in reaching them, so that the beginnings of spring and autumn *precede* the epochs at which otherwise they would have occurred.

But usually the term "precession" is understood to include all the circumstances on which the observed change depends, and writers will often speak of the precessional reeling of the earth. The reeling itself is, of course, not precessional, it is but the cause of precession.

The change in the position of the points where spring and autumn begin was first discovered by Hipparchus, though Ptolemy usually gets the credit of it. It was found that the point of the ecliptic where the sun is when spring begins—we may say when the year of seasons begins—is slowly moving backwards among the zodiacal constellations. It was once in Taurus, which was spoken of even by Virgil (long after the relation had ceased to exist) as opening the year with its golden horns. Then it passed to Aries, thence to Pisces, in which constellation it is now.

The effects thus produced in the position of the celestial equator, poles, &c., will occupy us hereafter. For the present we wish to consider their cause, or, rather, first we wish to describe what is the actual motion of the earth to which they are due, the real cause of precession being the forces which cause the earth to move in the particular manner in question.

Let us for the moment leave out of account the motion of the earth round her axis, and regarding her centre as at rest, let us consider what is the real nature of that motion of hers which causes precession.

Observation shows that the plane of the ecliptic, in which the earth travels, is almost unchanging in position. The course among the stars along which the sun (if we could see the stars when he is shining) would appear to travel, is almost exactly the same now as it was in the time of Hipparchus. Again, the inclination of the earth's polar axis to the plane in which she travels varies very slightly (the variation shall presently be described, and its physical cause indicated).

But the line in which the plane of the earth's equator

cuts the plane of her motion (the ecliptic) is constantly shifting in position. Suppose ABDF a view of the earth from a point on the northern side of the plane of her motion, the North Pole being at P. Then, again, let

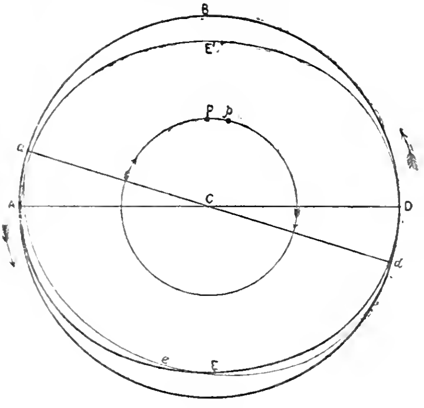


Fig. 1.

ABDF be a section taken through the earth's centre, C, and let AD be the line in which the plane of the equator, AEDE' (E' being on the farther or southern hemisphere),

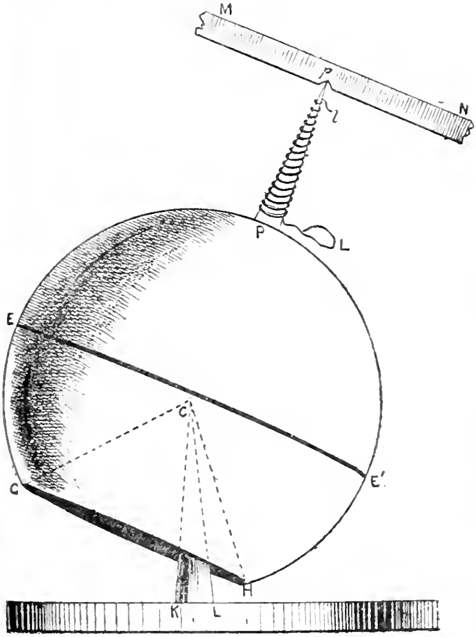


Fig. 2.

cuts ABDF. Then, if the arrow outside ABDF shows the direction in which the earth moves round her orbit, the small arrows inside show the direction in which this line of intersection is constantly travelling. In some thousand years, for instance, it would pass from the position ACD, to the position *a*C*d*; the equator would have passed to the position *aed* (one half only is shown), the northern pole from the position P to the position *p*.

Now, if we consider this motion carefully, paying especial attention to the movement of the pole, we see that it precisely resembles the reeling of a top. P might be the middle of the top's upper surface, C the peg, CP and C*p* two positions of the axis of the top as it reeled around C.

Or, instead of a top, the middle of the body of which is not the centre round which the reeling occurs, imagine a globe as GEPEH having a section such as is shown by the dotted lines in Fig. 2, GCH indicating where a conical hole has been cut into the globe, right to the centre. Such a globe set rapidly spinning in an inclined position (as in the fig.) on the top of a vertical spike like KCL, would be found to reel in a direction contrary to that of its rotation (just as in the case of the earth's precessional reeling), and if a circle EE' were drawn to represent the equator, and a point P marked exactly opposite the small circle GH, this point (or preferably a point *p* at the end of a polar spike P*p*), watched from above, would be seen to move round just as the pole of the earth supposed to be watched from the direction indicated in Fig. 1.

The motion of the earth, then, by which precession is caused, resembles that of a reeling body, like a great spherical top. The period of this reeling motion is, however, so much longer than that of the revolution, that its effects seem, by comparison, very slight. The whole lifetime of a man may pass, and only the astronomer would notice the effects which it had in reality produced.

Let us see what these effects must be.

The poles of the heavens are those points on the celestial sphere towards which the axis of the earth is directed, while the celestial equator is the great circle of the celestial sphere lying midway between the poles. The celestial equator, in fact, corresponds with the earth's equator, insomuch that, if we imagine a straight line drawn from the earth's centre to any point of the terrestrial equator, and prolonged indefinitely outwards, then this line as it was carried round by the earth's rotation, would sweep out the circle which we call the celestial equator.

Now, since the earth is reeling like a gigantic top, its axis moves—just as the axis of such a top moves—over the surface of a gigantic cone. Neglecting—as relatively insignificant—the range of the earth round her orbit, we may regard the earth's centre as the apex of this gigantic cone. The points in which a line perpendicular to the plane of the earth's motion meet the imaginary celestial sphere, or the poles of the equator, are those towards which the axis of the great precessional cone is directed. Around these points the poles of the heavens revolve in two small circles, the distance of each pole from the pole of the ecliptic round which it rotates corresponding to the inclination of the earth's axis, or in arc to about 23½°. The equator moves correspondingly, and a complete circuit is accomplished by each pole in rather less than 25,900 years.

Next week I propose to consider more particularly the effects of this reeling motion, so far as it affects the apparent position of the heavenly bodies—showing in particular how it has affected the positions of certain constellations. Then we have to consider how the seasons are affected by the change. Lastly, it will be well to consider how the reeling motion is produced, and how certain peculiarities in its progress are brought about.

In the meantime, I would invite the student to notice that very interesting illustrations of the earth's precessional

reeling can be quite easily made. A convenient way is to have a globe of iron hollowed out, as in Fig. 2, and set in rapid rotation which may most conveniently be done by having a projecting rod at the top, pierced to receive a string, as R, and pointed, so that a metal plate, as MN, with a hollow to receive the end of axis p, may be held against it while the wound-up string is sharply drawn off. The heavier the globe the steadier will its motion be found to be, the slower and the more constant its reeling.

There is, however, a more perfect method of illustrating the precession of the equinoxes or the earth's *reeling*, and also the nutation (or *nodding*, still to be described), in the instrument invented by my esteemed friend, the late Mr. Burr, of the Astronomical Society. This instrument I will sketch later.

NIGHTS WITH A 3-INCH TELESCOPE.

PRESUMING the reader to be now in possession of such a telescope as that described in our first article, and that he has placed it on a firm stand of convenient height, with the bar, BM, as nearly as practicable in the meridian, we will begin our examination of the heavens by turning towards the western and south-western sky, inasmuch as the stars there situated will set sooner and sooner every night until they disappear for the season. Arming the instrument, then, with a power of 160 (a lower eye-piece may be employed to find the object), we will begin by directing it towards the star marked γ in Cetus (*vide* Map on p. 119).

Fig. 4.—γ Ceti.

At the first glance, probably, the student will see nothing but a yellowish star of considerable brightness; but, by careful attention, he will not be long ere he catches its small companion, seemingly to the left of and just below a horizontal line, passing through the larger star. Its blue or dusky tint will at once strike the observer, as well as its small size as compared with that of its primary. This elegant pair form what is known to astronomers as a "binary system"; in other words, the stars are physically connected, and the smaller star revolves round the larger one—or both round their common centre of gravity—in a very long period, the exact duration of which is as yet undetermined. There are other objects of interest in this constellation, but the difficulty of recognising them without the aid of an equatorially mounted telescope, furnished with graduated circles, compels us to omit reference to them in these very elementary papers. Among them, 66 Ceti may be mentioned as a charming pair. It may be found with numerous other doubles—on Map 3 of Proctor's "Star Atlas."

Above, and to the right of that part of Cetus in which γ is situated, will be seen a curved line of three stars (Map on p. 201); the chief ones in Aries, the bottom, and least of which is remarkable as being the one of which Hooke wrote in 1664, "I took notice that it consisted of two small stars very near together; a like instance to which I have not else met with in all the heaven." It is almost needless to tell the student that double stars are now numbered by thousands. Viewed with a power of 160, γ Arietis presents the appearance shown in Fig. 5.

Fig. 5.—γ Arietis.

The components of this asterism will be observed to be pretty nearly equal in size. The apparently lower and slightly smaller star of the two will be seen to be of a greyish hue. If now the observer will follow an imaginary line from γ through β in the map, it will strike upon a star, not lettered there, but fairly well seen by the naked eye to the right of α. This is λ, a wide but pretty double. Here, again, the smaller star is more distinctly coloured than the larger one. Forming the apex of a right-angled triangle, with α and λ Arietis (whereof α is at the right angle), is a wide triple star, 14 Arietis. Sweeping where Aries and Triangula are conterminous, several pairs of small stars will pass across the field of view. Some 2° (four times the diameter of the Sun or Moon) above, and to the right of β, Arietis (as seen by the naked eye) will be found a beautiful close double star, which will tax the powers of the incipient observer to see fairly separated. It is 179 of Hour I. in Piazzi's Great Catalogue. The yellowish tinge of the larger component contrasting with the blue of the smaller one renders this a very pretty object.

And while his telescope is directed to this region of the sky, the student will not fail to turn it upon the planet Saturn, still employing the same power. The aspect of the planet as seen in an instrument of the size of that which we are supposed to be using is shown in Fig. 6.

Fig. 6.—Saturn.

It was drawn with a 3-in. telescope of the highest class expressly for these papers, and exhibits what the possessor of such an one may fairly expect to see under similar circumstances. A little careful attention will show how the ring is divided into two by a dark line which will be most easily traceable in what are called the "ansæ" (or handles—*i.e.*, in the easternmost and westernmost parts of it. The inky-black shadow of the ball of the planet, to the right of it, on the rings, and the slight curvature of this shadow will also be made out without much difficulty. The darker shading on the southern half of Saturn's globe, and the bright belt on the planet's equator between this polar capping and the ring, will be recognisable without diff

culty. His largest moon, Titan, will be seen at once, and Japetus (which was visible, as shown, when the drawing was made) may, perhaps, be picked up. Tethys, Dione, and Rhœa, are too severe tests for a 3-in. object-glass, and the other three satellites are hopelessly beyond much larger instruments. The incipient astronomer must not expect to perceive all the wonderful Saturnian detail shown in astronomical books. Should his 3-in. telescope show him exactly what is exhibited in the engraving, he may rest assured that he is the possessor of a first-rate instrument. Neptune, who may be found from the map, will be undistinguishable from a fixed star with the optical means at our command. Jupiter, however, represented in our next figure,

Fig. 7.—Jupiter.

is a brilliant and most conspicuous object, and the eye of the novice will have to become accustomed to the brilliance of his light before much detail can be made out upon his surface. At the period in his rotation, corresponding to that at which our sketch was taken, the great red spot which has been such a conspicuous object on his disc for the last two or three years will be at once recognised; a dark belt to the east, and somewhat above (or south of) it, bending down seemingly towards it. Then, north of the great spot, we note a well-defined band, the darkest portion of the planet's surface. The northern edge of this, and the southern edge of a belt nearly on the planet's equator, enclose an irregularly-formed lighter area between them. They are succeeded by a third dark belt, after which the whole of the planet's limb up to his north pole is covered with a continuous shading. Jupiter has four moons, but their positions are so continually shifting as to render it impossible to insert them in any drawing, unless its exact epoch be given. They may all four be outside of the planet's disc, or on the same side, or some on one side and some on another. Or they, or one or more of them, may be hidden in the shadow of Jupiter, or be passing across his face. In this latter case, the shadow of the crossing satellite may be detected like a little circular dot of ink upon his face. Our sketch of Jupiter, we must warn the young observer, is made on a smaller scale than that of Saturn, the latter having been enlarged to exhibit detail. When the observer has gazed his fill upon this superb planet, he may raise his telescope to that lovely object γ Andromedæ (above Triangula in the map). The contrast between the yellow of the large star and the exquisite green of the small companion is very striking. π Andromedæ to the right of β is a very pretty object, the contrasting colours being in this case very pale yellow and blue. 59, Σ 3, P. XXIII., 240, and other beautiful pairs will be found marked in Proctor's "Atlas."

Exchanging now his high power for the lowest one supplied with his telescope, the beginner should fish a little above to the right of ν Andromedæ (see map, p. 204) for that most remarkable object, 31 of "Messier's Catalogue," the well-known great nebula in Andromeda. Sir John Herschel quotes Simon Marius as describing the appearance of this nebula as resembling that of a candle shining through horn; and this really does not give a bad idea of it, as viewed in such an instrument as that which we are using. We purposely abstain from giving any figure of this nebula, inasmuch as no woodcut is competent to reproduce the peculiar effect of a nebula, and our object is to show as exactly as possible what the reader, furnished with a first-class 3-inch achromatic, may expect it to show him.

None of the larger stars in Taurus present any features of interest in small telescopes. χ Tauri is a somewhat wide, but pretty pair. It is the one above the letter S in the word "Taurus" in the map on page 119. Identification of the smaller ones without graduated circles is almost hopeless. Using a low eye-piece through, the Pleiades present a fine spectacle; and about two diameters of the moon, above and to the right of ξ Tauri, will be found a pale, elongated nebula. A low eye-piece, too, must be used for this. Nearly over head, just now, Perseus will be observed; a constellation rich in objects of interest, of which, however, we can only give an account of a very few suitable for the instrument we are employing. Reference, as before, must be made to the maps on pages 97, 119, and 204 for their identification. ϵ is a very fine pair, but the smaller star requires some little looking for. It is as shown in Fig. 8 below, and *just* to the right of its primary. ζ Persei is really a quadruple star, although the student will scarcely discern more than three out of its four components with the aperture we are considering. η is another pretty pair, too, but somewhat difficult, from the faintness of the companion. Perseus contains several interesting clusters—notably one of the most glorious fields of stars in the whole heavens, in what is called the "Sword-handle." This may be seen by a sharp-sighted person with the

Fig. 8.—ϵ Persei.

naked eye between Perseus and Cassiopeia (map on p. 75) as a bright spot in the Milky Way. This superb object requires the lowest eye-piece in the observer's possession to do it anything like justice. No view of it, however, with so small an aperture will give any idea of the gorgeous effect it presents in a large instrument.

South of Aries and the Pleiades lies the straggling constellation Eridanus. It contains numerous interesting pairs of stars; but for them the student must sweep, if confined to the maps in Knowledge, as they are not numbered there; and hence, any attempted description of their localities could only be confusing. 32, 39, 55, and P III., 98 will all be found to be beautiful and attractive objects, and are marked in Proctor. A curious planetary nebula H IV. 26, seen best with a low power, will be found there too. Having then furnished the incipient star-gazer with a good hard night's work, we take leave of him for the present. In our next lesson we propose to deal with that splendid and all-repaying constellation, Orion.

FOUND LINKS.

By Dr. Andrew Wilson, F.L.S., &c.

PART II.

TURNING now to the last-named fishes (*Lepidosiren* and *Ceratodus*), we discover that their fish-characters exist on the very surface of things. Their blood is cold; their bodies are scaly; they have fins and fin-rays; and above all, they possess gills existing in the sides of the neck, and in which, so long as they swim in the water, their blood is purified. But here the fish-characters end. Another aspect of the mud-fishes and the barramunda reveals characters which startle us as being not those of fishes, but those of frogs; and frogs, toads, and newts form, as every one knows, the second higher class of vertebrate, that of the *Amphibia*.

Firstly, then, the *Lepidosiren* possesses a heart, which is not that of a fish, but modelled on the type of the frog or reptile heart. Instead of being two-chambered, it is three-chambered; and no other fish save itself possesses such an advance on the ordinary type of fish-heart. But, secondly, their "paired fins," which represent in all fishes the "limbs" of higher animals, resemble—in the mud-fishes at least—rudimentary limbs. Then the nostrils, thirdly, open into the mouth—a character agreeing with frogs and all higher vertebrates, but possessed by one other fish-group only—the low hag-fishes, which are poor relations of the lampreys. These characters, then, are the characters of frogs, and not of fishes. But a far more interesting likeness to the frogs and higher vertebrates yet remains for notice. The "air bladder" of the mud-fish and of the "Jeevine" alters wonderfully, both in form and function, from its nature in other fishes. It becomes divided in two, and it opens into their throat by a windpipe, at the top of which is a "glottis," corresponding to part of our own organ of voice. Furthermore, it is divided internally into cells—in a word, the air-bladder of the mud-fish and its neighbour *has become a lung*. But this wonderful transformation is not quite ended with the recital of the altered structure of the air-bladder in these fishes. A lung is an organ which not merely receives blood in an *impure state*, but which, as in ourselves, returns that blood *pure* to the heart for re-circulation through the body. If, therefore, the "lung" of the fish is to be accounted a true "lung," we should be able to show that it performs the functions and discharges the duties of an organ of breathing.

Now the life of these fishes exhibits exactly the peculiarities which demand the exercise of an air-breathing organ like a lung. The mud-fishes inhabit their native rivers during the wet season; but when the dry season approaches, they bury themselves in the mud, and lie there, baked as in a kind of mud-pie, until the return of the persistent rains. During this land-existence their "lungs" come into play. So long as they live in their native waters, they breathe by their gills like ordinary fishes; but, ensconced in the mud, they breathe air directly from the atmosphere, like ourselves. The air-bladder purifies the blood, which the heart pumps into its vessels, and from the "lungs" the purified blood is returned to the heart. The fish is thus truly a "double-breather;" it exhibits in itself the combination of the characters of the frog and the fish. Dr. Günther tells us that whilst the mud fishes remain in the "torpid state of existence, the clay balls containing them are frequently dug out, and, if the capsules are not broken, the fishes imbedded in them can be transported to Europe, and released by being immersed in slightly tepid water." The "Jeevine," with its similar "lung," is said to leave the Australian rivers at night, and to waddle its way to the marshes and swamps, there to feed upon the vegetable matter that forms its staple food. In the nocturnal journeyings of the fish we can readily perceive the utility of the "lung."

It may lastly be remarked that other fishes are known to leave the water and to exist for a time on land. The climbing perch of India, and the *Ophiocephali*, also of India, illustrate such fishes: but in these forms the breathing in air is contrived in a different fashion from that process in the mud-fishes, and has no connection with any "lung."

Let us now reflect that a frog itself begins life as a fish. The "tadpole" has gills and a fish-heart, whilst it has no lungs. Ultimately it acquires lungs and loses gills and tail as its mature shape is attained. Summing up these plain facts of zoology, I think it is not difficult to see that in the mud-fishes and "Jeevine" we find a "link" between the lower water-living fishes and the air-breathing frogs. If we suppose that a form like the mud-fish could rid itself of its gills when it became adult, and that it could throw off the scales of the fish, and develope the limbs of the frog, we might figure to ourselves the ascent of the frog-type from the fish-type. There is nothing more wonderful or impossible in this idea than in the veritable fact that every frog is at first a fish, then a tailed newt, and only ultimately becomes the amphibian. Anyhow, one fact seems clear enough, that fishes and frogs—two utterly distinct classes—are "linked" by the mud-fishes and "Jeevine;" and this single fact in itself supports powerfully, in a rational view of matters, the theory that the air-breathing tribes of animals sprang originally from water-living forms. We shall see in future papers that "links" even of stranger kind unite classes of animals as dissimilar as the fishes and the frogs.

FALLACIES ABOUT LUCK.

By the Editor.

WALKING down to the boat-houses, one day, when I was at Cambridge, a friend (now a clergyman) who was taking part, like myself, in the four-oared scratch races, remarked that his boat was sure not to draw the unlucky first place that day. "How's that?" I asked. "Why, because we have had to row first every day until now, and the luck is sure to change to-day." (It may be necessary to explain that boat-races on the Cam are bumping races—unless where time races are rowed between the last two or three left in—and that the first place is, of course, the worst in a bumping race, for the simple reason that the first boat shares with the others, all but the last, the risk of being bumped, but cannot make a bump. In the eight-oared races, indeed, the first place is the place of honour, attained by bumping boats below; but where, before a race, lots are drawn for position, the first place is the worst, the last place the best.) I tried to explain to my friend what seemed so obvious as to need no explanation, that his fate in the day's drawing could not possibly be affected by the resuits of previous drawings. The simple circumstance that to draw a given place day after day, for six days (five were past), was a thing unheard of, so far as he knew, sufficed to assure him that his coxswain would not that day draw a particular ticket. It so chanced that what he was sure would not happen did actually happen, though it would in no way have affected my argument if his hopes had been fulfilled.

The mistake made by my friend on this occasion is one of the commonest fallacies respecting the laws of chance. Of course, it requires no mathematical knowledge or reasoning to show the opinion to be quite erroneous that past events can in any way influence events which are of their very nature entirely independent of them. If there is an urn in which we know that there are a number of white and a number of black balls, and we draw one after another several white balls, *not returning them*, we have some reason for thinking that we are more likely to draw a black ball at the next trial, for every white ball drawn diminishes the chance that the next one drawn will be white. But if each ball after being drawn is replaced, it is evident that the chance of drawing a white ball at any given trial must be the same as that of drawing it at the first or at any other trial. Or take the tossing of a coin. Antecedently it seems so unlikely that head (say) will be tossed ten times running, that we can easily imagine how anyone who had tossed head nine times running might entertain for a moment the idea that he was less likely to toss head the tenth time. But if he had any reasoning power at all, and used it, he would see that no number of past trials could in any degree affect the next tossing.

There is a fallacy equally common, and held commonly by the same persons who make the mistake just considered, which yet is opposite to it in character—in fact, directly contradictory to it. The mistake we have dealt with above may be called belief in the change of luck, and in a somewhat disguised form it is this foolish fallacy which leads the weak-minded pigeon to fall an easy prey to the rooks, from the fond delusion, in which, of course, they encourage him, that though he has lost—or rather because he has lost for a long time—he must presently begin to win. The fallacy we have next to mention is faith in luck. You will hear people say that they never have luck in games of chance, or that they always have luck; and you will find hundreds ready to believe in the good luck or bad luck of others. We say that this belief is contradictory to the other. If it be considered for a moment, this is seen to be the case. What does belief in a man's good or bad luck mean but that, because he has been fortunate or unfortunate for a long time he will continue to be so? and what does the other belief mean but that, because the luck has been one way for a long time, it will no longer continue to be so? One would suppose that two ideas so incompatible with each other could not exist in company; that everyone must see one or other to be fallacious, or (which, of course, is the actual case) that both are so. Both views are in fact ridiculous, though both, with many other equally preposterous superstitions, are entertained by persons who are not supposed to be wanting in keenness of perception, and in other matters are intelligent enough. Here, for instance, is an account given by one keen card-player of another who was as keen, or keener. "He was very particular about cutting the cards; he always insisted on the pack being perfectly square before he would cut, and that they should be placed in a convenient position. There is an old adage that a slovenly cut is good for the dealer, but whether there is truth in the statement we know not. He was superstitious to a degree that was astonishing." (It must be a rather startling superstition that would seem astonishing to a man who could gravely ask whether there is any truth in the preposterous adage just quoted.) "We are not aware that any one has ever attempted to solve the problem why so many great minds" (among card-players, fighting men, and men who have to work much at odds with fortune) "are superstitious. This is not the time or place to attempt that solution. We record the fact. He believed in dress having something to do with luck, and if the luck followed him, he would wear the same dress, whether it was adapted to the weather or not. He believed in cards and seats. He objected to any one making a remark about his luck. He had the strongest objection to our backing him, because of our bad luck, and we have often had to refrain from taking odds, because of this fad. He was distressed beyond measure if any one touched his counters. His constant system of shuffling the cards was at times an annoyance." This was a great card-player.

It will be asked, perhaps, how cases of notoriously lucky men are to be accounted for, if there is no such thing as luck. If the laws of probabilities say that no man can be regarded as a lucky or unlucky man in matters of pure chance, how is it that so many men have been lucky or unlucky? But science by no means denies that men have been or will be lucky or unlucky; on the contrary, the laws of probability can *prove* that among the millions who try their fortunes in matters of pure chance, thousands must be exceptionally lucky or unlucky, and a few must have luck perfectly marvellous to all who witness it. Given the nature of any chance game and the number who play at it, science can tell, within very narrow limits of error, how many will have ordinary luck, how many will have moderately good, or moderately bad luck, how many will be very lucky or very unlucky, and how many will have absolutely astounding luck of one sort or the other. When Science is asked how, with her absolute rejection of all faith in luck, she can account for men who have had amazing runs of good or bad luck, Science can reply not only that she has no difficulty in accounting for them, but that she can prove this to be to all intents and purposes inevitable.

What, then, is it that science rejects as untenable, or how, with such views, can science be truly said to have no faith in luck? The answer is, that the laws of probability—and (rightly understood) the laws of common sense—forbid our believing that a man *is* either lucky or unlucky. He may have been so; but, so far as matters of pure chance are concerned, the man who has been most unlucky is as likely as not to be lucky at any given trial as one who has been exceedingly lucky. He is not more likely to be so, as the fallacy respecting change of luck implies, nor is he less likely, as the fallacy of faith in luck implies; he has simply just the same chance as another, neither better nor worse.

If twenty million persons in England were to begin tossing a coin, each stopping so soon as he tossed "tail," and each to receive a pound for one head, two for two heads, four for three, eight for four, sixteen for five, and so forth, it is practically certain that several would win a prize of £131,072 after tossing head eighteen times running, and all but certain that some would get the prize of £262,144 for tossing head nineteen times running, and one or two perhaps the prize of £524,288 for tossing head twenty times running. These would all have been very lucky persons (and as long as they kept their winnings, we may say that they were in luck afterwards as well as before). The laws of probability show that among so many trials there must be some such lucky persons. But, supposing the experiment repeated, science assures us that those persons who had been so lucky would have neither a better nor a worse chance of success than those who had had but moderate luck, or the unfortunates (some ten million in number) who had tossed tail at the first trial. What would believers in the two fallacies we have considered, think? If they had watched one of the luckiest tossers, would they say that, as he had tossed head so many times running, he was unlikely to toss a single other head in the second trial

or that, as he had shown himself a lucky man, he was sure to continue tossing heads in the second trial also? One idea is as consistent with the common fallacies about luck as the other. Both cannot be true; but, in point of fact, they are alike erroneous. Carefully studied, each is seen to involve an absurd mistake.

THE PRINCIPLE OF THE VERNIER.

By J. R. Campbell.

The vernier is a short scale, which, applied to the edge of another, on which we measure a length, enables us to read to within a certain fraction of the smallest division of the latter scale. Thus, suppose the scale we measure on to be divided into tenths of an inch, by means of a suitable vernier we can read to within the hundredth of an inch.

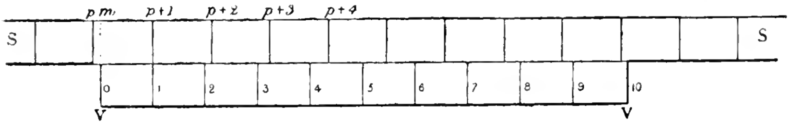


Fig. 1.

In Figs. 1 and 2, SS represents a portion of an ordinary scale of equal parts, reading from left to right, p being p^{th} graduation from the zero on the left, and $p+1$, $p+2$, $p+3$, &c., the graduations which follow; the distance between each being = 1 unit. Suppose m to be some point lying between p and $p+1$, at a distance x from p, then the vernier VV is a contrivance for giving us the numerical value of x, to within a constant fraction of the unit, supposing its zero to be on m.

Let us suppose the fraction to be $\frac{1}{10}$. In this case VV has a length = 9 divisions of SS, but is itself divided into ten equal parts, figured 1, 2, 3, to 10. Each division of the vernier will, therefore, be $\frac{9}{10}$th of a unit. If now we find that (as in Fig. 1) the graduation 1 on the vernier coincides with the graduation $p+1$ on SS,

$x = 1 - \frac{9}{10} = \frac{1}{10}$, and for the measurement we read $p + \frac{1}{10}$.

If 2 on VV coincides with $p+2$ on SS,

$x = 2 - 2 \times \frac{9}{10} = \frac{2}{10}$, and we read $p + \frac{2}{10}$.

If 3 on VV coincides with $p+3$ on SS,

$x = 3 - 3 \times \frac{9}{10} = \frac{3}{10}$, and we read $p + \frac{3}{10}$,

and so on.

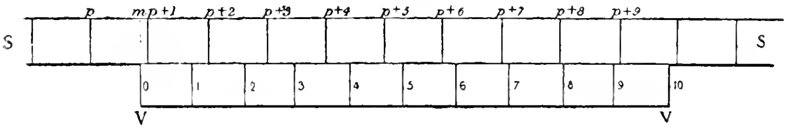


Fig. 2.

In Fig. 2, 8 on VV *more nearly* coincides with $p+8$ on SS than either 7 with $p+7$, or 9 with $p+9$. In this case, therefore,

$x = \frac{7}{10} +$ a fraction less than $\frac{1}{10}$, and we take $p + \frac{8}{10}$ as the measurement required.

Were the length of the vernier = 29 divisions of SS, and that length portioned into 30 equal parts, each would be the $\frac{29}{30}$ th of the unit; and it is easy to see that such an arrangement would read to within the $\frac{1}{30}$ th of a unit on SS. Generally, if the vernier be of a length $= n - 1$ units on SS, and consist of n equal divisions each $\frac{n-1}{n}$, it will measure to within the $\frac{1}{n}$ th of a unit.

The advantage gained from the employment of a vernier arises from the fact that the eye can determine the coincidence of two lines, when it cannot accurately judge the distance of other lines which do not coincide, thereby rendering a minute subdivision of each division on SS unnecessary.

Verniers are mostly applied to the measurement of arcs of circles, and form an important element in the theodolite, sextant, and other instruments of that class. In these, the scale SS forms a portion or the whole of a circle, and the vernier is an arc having the same centre. In measuring an angle by means of these instruments, we move the vernier, the scale of degrees, SS, being fixed.

COLOURS OF ANIMALS.

The colour and flesh of trout are affected by the water and bottoms they haunt. I know a river, the upper water of which passes over gravel and sand. Below that length, it streams through bog and alder scrub. In the first length, the fish are golden, well-formed, and good food. In the bog length they are black, bull-headed, and flabby—utterly good-for-nothing. He must be a starving otter who would make a meal of them. When these trout passed into good water, they recovered shape and condition. Again, I once lived in a wild part of Ireland. Opposite, and close to my house, there was a lake connected to another lake by an outflow. The water of the one, or upper lake, was clear, resting on brilliant white sand—a thoroughly raw bottom. The trout in it were bright and light in colour, not plump, white fleshed, and tasteless as dace. The lower lake was on bog and blown sea-sand. The water was darkish. In this, the trout were something to look at. Dark backs, merging into bright olive on the sides, and shading into white on the belly. They were beautifully spotted. Cooked, these fish excelled in delicacy of flavour any salmon I ever tasted. Now, these lower lake trout were the same as the white lake trout, but changed in appearance by different conditions of water and food. That the upper lake trout came down to the lower lake was notorious; but none was ever caught in its original condition. Was this change for concealment?

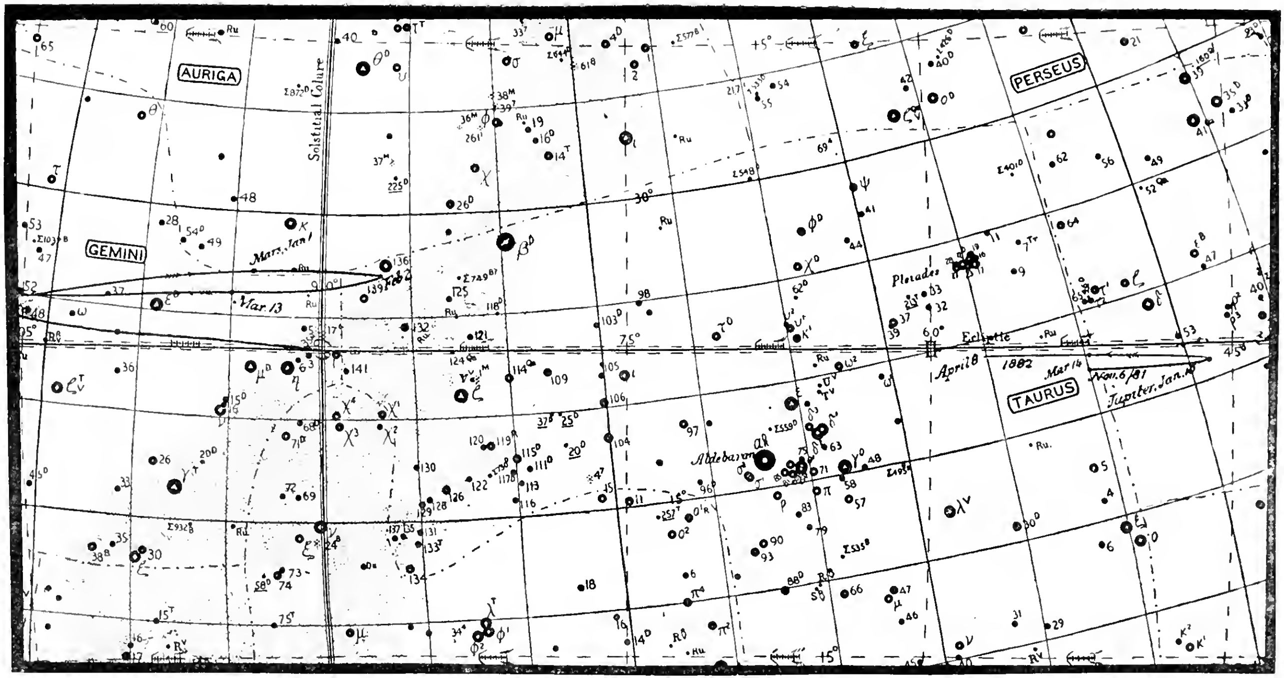
The flying-fish in the Gulf-stream have markedly the blue tint of the stream, and are of darker hue in the darker sea-water. Was this for concealment? [We should say yes.—Ed.]

A lady, accurate in observation, has told me that the eggs of the cloth-moth vary in colour with the colour of the cloth they are deposited on. Is that for concealment, or from an explainable cause?

I cannot accept Mr. Darwin's worm theory in its totality, and I repeat that which Dr. Wilson affirms is "a trifle too near silliness," namely, that the theory of worm action on the soil is an elaboration of exceptional minutiæ into huge paradox. [Might it not be well if our correspondent would mention some statement or conclusion in the work, to which he objects?—Ed.]

Dr. Wilson's answer to 'Ornithorhynchus," on the toad's immunity from the common effect of bee poison, misses the fact. The toad that munches a bee is not stung. He darts his long rapier tongue like a flash of lightning at the bee, and as rapidly withdraws it, with the bee fast to his mouth. The bee is killed before it recovers sense to sting. Neither bee nor wasp will sting when in a state of sudden terror. B. Donbavand.

Erratum on Page 179, No. 9.—In the sixth line of the fourth paragraph of the article on "Primary Colours," insert a colon after "not," and delete the full stop after "theorists."—E. H.



ZODIACAL MAP.

WE give a map this week showing on an enlarged scale (as compared with the monthly map) the part of the Zodiacal map best suited for observation during this month and the next. (The map is reduced from the editor's larger Star Atlas, Map IV.) The path of Mars during its present approach to our earth is shown, as also the path of Jupiter. Many of the stars, of which "A Fellow of the Royal Astronomical Society" will have occasion to speak during the next few weeks, will, we doubt not, be within the limits of the region pictured in our map

THE PYRAMID OF MEYDOOM.

By Amelia B. Edwards.

To open the Pyramid of Meydoom was one of the unfulfilled projects of the late Mariette Pasha. Prof. Maspero takes Egyptologists by surprise in achieving this great work during the first weeks of his second year's campaign. For more than a month it had been known to a few of his private friends that he had a gang of fellaheen at work upon this pyramid; but the secret has been carefully kept; and not till success was actually achieved was it even suspected in the public offices and hotels of Cairo.

The Pyramid of Meydoom, situate about a mile and a-half to the north-west of the village of Meydoom, is built in three stages, each inclined at an angle of 74° 10′. It rises to a height of 122 ft. from the midst of a high hill of fallen masonry and rubble. The first stage shows a height of 69 ft.; the second measures 20½ ft.; the third, which is much ruined, measures 32 ft. in height. The outer masonry is of admirable workmanship, and consists of polished blocks of Mokattam limestone. The general effect of the structure is very imposing. It has been aptly compared to the keep of a Norman castle, if we can imagine a keep built in three superimposed stages. Archæologists have till now believed that this pyramid was inviolate. Dominating a vast burial-field containing the *mastabas*, or built sepulchres, of many nobles and "royal relatives" of Seneferoo, the last king of the Third Dynasty, it is supposed to be the pyramid of that monarch. It would in such case be of earlier date than the pyramids of Gheezeh.

Professor Maspero began his work by opening a vertical trench down the northern face of the mound out of which the pyramid rises; the first result being to show that the masonry goes down to the level of the desert, and that the mound is entirely formed of accumulated sand and *débris*. Professor Maspero believes this *débris* to be very ancient, and thinks it may even date as far back as the end of the New Empire. The entrance was found precisely in the centre of the north face of the first stage, about 20 mètres above the level of the plain; which would give about 134 feet for the actual height of the first stage as now laid bare. This entrance was opened on the 13th of last month. It gave access to a descending passage about 1 mètre 60 centimètres square. The incline of this passage is very steep, and for the first 10 mètres it is lined with masonry finer, if possible, than that of the external facing. At a depth of 10 mètres the passage strikes the living rock, and becomes in its continuation an excavated sloping shaft of the same dimensions as before. The pyramid, for at least half its height, is therefore formed upon a core of rock, around which the pyramidal structure is elevated. About 5 mètres from the entrance there was once a "stopper" stone, which closed the mouth of the passage. This "stopper" has disappeared; but the construction of the passage shows that it was contrived quite differently from the portcullis-stones of the Gheezeh pyramids. There is no void in the roofing above where it is placed. It must, therefore, have been inserted after the mummy was laid in the sepulchre; then filled up to the level of the outer wall, and covered with the same *revêtement*, so leaving no external trace. The removal of this stone must have been effected at a very distant time, there being three hieratic inscriptions of the period of the XXth Dynasty scrawled upon the ceiling at the very point which the stone formerly occupied. These inscriptions, written in accordance with a brief formula common to the epoch, merely record the visit of two Egyptian tourists—the scribe Sokari and the scribe Amenmes. Hence it would seem that the pyramid of Meydoom was open, and visited by the curious, as early as the XXth Dynasty. The descending passage has been cleared to the depth of 40 mètres, without any landing, branch-passage, or chamber having yet been reached. Thus far, the structural arrangement seems to reproduce the internal plan of the Great Pyramid. The work is of extreme difficulty, owing to the want of air and light, and the overwhelming heat. The workmen faint from time to time, and have to be carried out. Prof. Maspero finds it impossible to stay inside for more than half-an-hour together.—From the *Academy*.

VEGETABLE POISONS.

The evidence in reference to the death of Percy Malcolm John was of a character which seems to leave no doubt that the deceased was poisoned by aconitine, however or by whom it may have been administered. The medicinal qualities proper to various of the most active members of the vegetable kingdom, such as opium, hemlock, aconite, hellebore, cinchona, and others, have long been known to be due to the presence in their juices of substances called their active principles, and known chemically as alkaloids. Of these, quinine, the active principle of cinchona bark, and morphia, the active principle of opium, are of such comparatively slender potency that they are constantly administered in medicine, and their names have become household words—even to the extent, in the case of morphia, that it has lost its original French designation of morphine, and has become Latinised or Anglicised in common use. When the active principles were first discovered, they were almost entirely manufactured or separated in France, for the reason that the process required the employment of a large quantity of rectified spirit or pure alcohol, and that the excise duties on alcohol had not then been relaxed in favour of chemical manufactures, and were practically prohibitory in this country. The analysis of vegetable poisons, in quest of the active principles which they might contain, fell almost exclusively into the hands of French chemists; and hence these active principles, when separated and identified, were naturally called by French names. With those which are so powerful as to be dangerous in any but the most skilful hands, and which have never been popularly known or talked about, the French terminology has been commonly retained; and hence the active principle of hellebore is still called veratrine, and that of aconite aconitine. The aconite itself is the plant known to botanists as *Aconitum Napellus*, and familiar in many old-fashioned country gardens as monkshood, and its graceful form, dark and deeply indented foliage, and tall spikes of large dark blue flowers, are attendant upon one of the most deadly poisons which are contained in the vegetable kingdom. The root bears a superficial resemblance to horseradish, and there have been numerous instances in which this resemblance has led to fatal consequences. It cannot strictly be said to resemble horseradish in taste, but it produces upon the tongue a peculiar sensation of tingling, followed by prolonged numbness; and it has been eaten as horseradish over and over again, in sufficient quantities to cause the deaths of many persons, and serious illness to many others. When such is the virulence of the root itself, it cannot be matter for surprise that its alkaloid, or active principle, separated from every inert constituent, should be dangerous to man in a dose of the fiftieth of a grain, and fatal in the dose of a tenth of a grain; or that it should produce, in an intensified degree, the peculiar tongue sensation which is produced by chewing the plant itself. It is by these characteristics that the analysts, Dr. Stevenson and Dr. Dupré, have been able to identify aconitine as the poison which proved fatal to the deceased.

The vegetable alkaloids, when administered for criminal purposes, were long thought to present peculiar difficulties to chemists. The fatal dose might be so small in absolute amount as to be easily lost among the other ingredients of a meal, or the other constituents of the animal body; and the substances themselves do not offer the facilities for analysis with which we are familiar in the case of the mineral poisons. A preparation having a metallic base, such as arsenic or antimony, is practically indestructible; for, if it is swallowed in sufficient quantities to cause death, this base must always be present in the remains in a condition in which it can be separated and identified by processes of the simplest kind, with which it is the business of every medical student to be familiar. The alkaloids, on the other hand, instead of having permanent bases peculiar to themselves, are composed of the same elements which build up the fabric of the body, and of the food from which its sustenance is derived, and there are very few of them which, in the presence of any tests, yield reactions that can be described as positively characteristic. In these circumstances physiology has come to the help of chemistry, and the practice which is pursued is, in the first instance, to treat the contents of the stomach, or the substance of the viscera, in such a way as to extract from them any alkaloid which they may contain, and then to test this extract by its effects when administered to small animals, instead of by its behaviour in contact with re-agents. The evidence of Dr. Stevenson is to the effect that he made an extract from the matters submitted to him for analysis, and that he administered minute quantities of this extract by injecting it under the skin of mice, which thereupon died with symptoms of aconite poisoning. It further produced upon his own tongue and upon that of Dr. Dupré the characteristic tingling and numbness which would be produced by aconite, and which lasted from four to six hours; and, finally, some mice were poisoned with genuine aconitine, and their symptoms were compared with those exhibited by the mice poisoned by the extract. In this way the chemists were able to arrive at a conclusion which they appear to have placed beyond dispute. The body of the deceased contained aconitine enough to have killed a dozen people; and no one who had swallowed the alkaloid in the amount in which it was shown to have been present could have had, from the first moment, even the smallest chance of escape. In its legitimate uses, indeed, aconitine is scarcely ever employed by physicians except as an external application. A minute quantity of it is sometimes pre-

scribed as the basis of an ointment, which is rubbed into the skin in some forms of obstinate neuralgia, but its internal administration would be attended by perils, from accidents in dispensing or in the measurement of the dose, which no prudent practitioner would be likely to incur.—*Times.*

THE NEW STAR IN CASSIOPEIA.

THE following particulars respecting this wonderful star may be of interest to your querist and other readers (see query 65). The star known as the "Pilgrim," which suddenly blazed out in Cassiopeia in 1572, was, according to Smyth, first seen by Schaler, at Wittenberg, in August, 1572. Tycho Brahé, whose name is usually associated with the star, first saw it November 11. It increased in brilliancy until it surpassed Jupiter, and equalled Venus in brightness when it was visible at noonday. This state of things was not, however, of long duration, as it gradually diminished, and in March, 1574, had completely disappeared. Its curious changes are thus described:—"As it decreased in size, so it varied in colour; at first its light was white and extremely bright; it then became yellowish, afterwards of a ruddy colour, and finished with a pale livid colour." Smyth incorrectly gives its position as north of 55 Cassiopeiæ. This mistake has not been corrected by Chambers in his new edition of the Cycle. A reference to the original sketch, by Tycho Brahé, accompanying his description of the star, shows that it was situated close to κ Cassiopeiæ—a star of about fourth magnitude. The place deduced by Argelander, from Tycho Brahé's observations, is, for the year 1878, R.A. 0h. 18m. 2s. 1; N.P.D. 26° 31′ 43″. The position thus indicated is very void of stars to the naked eye, and even in an opera-glass.

Within one minute of arc of the place assigned by Argelander, d'Arrest, in 1865, observed a small star of the eleventh magnitude, of a reddish hue. By frequent comparisons made by Hind and Plummer in 1873, they found that this small star, which is No. 129 of d'Arrest's catalogue, is sensibly variable to the extent of nearly a magnitude. "It frequently presented a more blurred appearance than the neighbouring stars, and on several nights was remarked to flash up very sensibly for moments, assuming at these instants a redder tinge than at other times. The star assumed to be Tycho's will be readily identified by means of a bright ninth magnitude—8·9 according to Argelander, which is No. 22 of his Zone 60. It follows this ninth magnitude 29°·6, and is south of it 10′ 1·1.″" D'Arrest's star was also observed by Espin in 1878. Sir J. Herschel thought it probable that the object was identical with temporary stars which are said to have been seen in the same region of the heavens in the years 945 and 1264. Should this be the case, the next few years may possibly witness a repetition of this extraordinary phenomenon. J. E. Gore.

ACTINIUM; A METAL FOUND IN WHITE ZINC PIGMENT.

DR. T. L. PHIPSON'S experiments have at last proved successful, and he describes the process by which he has isolated the oxide and sulphide of the new metal, "actinium" (to which white zinc pigment owes its remarkable property of darkening in the sunlight, returning to its white state in the dark, and not being affected in this manner under a sheet of glass), in a state of tolerable purity. He says:

Perhaps this process may be improved hereafter, but it is not very complicated, though it has required an enormous number of experiments to arrive at it. First, one word as to the manner in which the pigment found in commerce is prepared. Ordinary zinc scrap is dissolved in sulphuric acid, and a considerable excess of zinc is left in the solution in order to keep out iron, lead, arsenic, and other metals. The liquid is drawn off, and then precipitated by a solution of sulphide of barium; the precipitate is dried, calcined, raked while hot into cold water, dried again, ground, &c. It then consists of sulphide of zinc, oxide of zinc, and sulphate of baryta, with minute quantities of iron, lead, arsenic, manganese, &c.

The manner in which I have obtained the oxide and sulphide of actinium from this pigment, is as follows, and the process will doubtless serve for the treatment of other substances in which the presence of the new metal may be detected:—

About 15 grms. of finely-pulverised pigment are left for twenty-four hours in dilute acetic acid (strongest acetic acid and water, equal parts), and the mixture well stirred or shaken occasionally. This takes out most of the iron, manganese, magnesia, lime, and oxide of zinc. The residue, after being washed, is treated exactly in the same manner with dilute hydrochloric acid (acid 8 parts, water 92 parts), with the object of completing the action of the acetic acid. The residue, well washed, is then treated with strong hydrochloric acid, to which a little nitric acid is added from time to time. The solution of the chlorides thus obtained is filtered to separate free sulphur and the insoluble sulphate of baryta, and remaining sulphur in suspension after filtration being oxidised by a few crystals of chlorate of potash. To this solution of chlorides, somewhat diluted, a considerable excess of caustic soda is added, and the solution heated. The zinc oxide goes into solution, and the white oxide of actinium remains; the latter is received upon a filter, washed, dissolved in hydrochloric acid, and the solution again treated with excess of caustic soda (these operations may be repeated two or three times, in order to eliminate the zinc oxide as much as possible). Finally, the oxide of actinium, still impure, is washed on a filter, and dissolved in a considerable excess of hydrochloric acid. The solution is neutralised by ammonia, and then the latter is added in excess. All but a little iron oxide remains dissolved (if not, dissolve again in HCl, and add ammonia in excess, which this time will only precipitate the iron). The iron oxide is separated by the filter, and to the filtrate sulphide of ammonia is added, which throws down the sulphide of actinium as a bulky, pale, canary-yellow precipitate, the colour of which is best seen when it is received on a filter.

Oxide of Actinium.—The hydrate, as precipitated by soda or ammonia, forms a bulky white precipitate, more gelatinous than oxide of zinc; unlike the latter, it is only very slightly soluble in caustic soda, even when the liquid is heated; it is not precipitated by ammonia from solutions containing ammoniacal salts. It is a permanent white, with a slight tinge of salmon colour when seen in bulk, and it does not change colour when exposed to the air, as oxide of manganese does, neither does it appear to be affected by the direct rays of the sun. It is readily soluble in acid. The anhydrous oxide is not volatile nor decomposed by heat. It has a pale, fawn-coloured tint.

Sulphide of Actinium.—The hydrate, as precipitated from its neutral or alkaline solutions by sulphide of ammonium, is a bulky pale canary-yellow precipitate, insoluble in excess of sulphide of ammonia, scarcely at all soluble in acetic acid, readily soluble in mineral acids, even when they are diluted. When exposed to the direct rays of the sun, it darkens and becomes quite black in twenty minutes, except in those places where it is protected by a piece of ordinary window glass.

The amount of actinium sulphide obtained from the white pigment amounts to no less than about 1 per cent. This yield is enormous. The presence of this new element in zinc will probably account for the discrepancies noticed in the equivalent of this metal, as determined by various observers. The new element differs very essentially from manganese, zinc, and cadmium, but has, perhaps, some points of similarity with lanthanum. It exists, evidently, in considerable quantities, in at least some kinds of commercial zinc.

FOSSILS IN METEORIC STONES.

THE question whether life exists, or ever has existed, in any the celestial bodies, under conditions similar to those which prevail upon the earth, has been one of deep interest ever since astronomy in modern times dissipated the old theories and set up correct ideas in regard to the solar system. It has been, however, a mere matter of affirmative belief, without any sort of proof to give it a foundation. It has been quite rationally argued that the All-Wise Father of the Universe would not allow such vast worlds, as we are now certain the heavenly bodies are, to remain waste places, without making them the abodes of life. Such was one of the sublime ideas of Thomas Dick, the famous "Christian Philosopher." And some of the materialistic evolutionists of more recent times, who exclude every idea of special creation from their doctrines, have suggested that possibly "in the beginning" life originated upon this earth in one or more germs which fell from some of the circling orbs above us, coming down with or upon those meteoric stones. But aside from the fact that science has shown a strong analogy between the physical conditions of our earth and some of the planets, there has not been until now any other proof of the existence of life as it is manifested to us outside of our own earth. But in the *Popular Science Monthly* for November, Mr. Francis Bingham has a very remarkable and interesting article on the discovery of organic remains in meteoric stones, in which he avers that "we are able to see with our own eyes the veritable remains of animate beings from another celestial body." These stones, which have been falling upon our earth during all historic time, and doubtless from periods far more remote, have been believed by astronomers to be the remains of a planet which had been destroyed in some manner—shattered into fragments by forces which to us may be set down as unknown or mysterious. Two German scientists—Dr. Hahn, a geologist, and Dr. Weinland, a zoologist—have recently investigated

this subject to some extent, and the result of their labours is that they find in these stones fossil sponges, corals, and crinoids. They are also of the opinion that they have discovered a trace of vegetable remains. The best defined fossils are those of corals and sponges, and in regard to these there would seem to be no doubt. The corals belong to classes which on the earth are found only in some of the oldest geological formations, and they differ from ours in being extremely small. Of the sponges, the remains were in such excellent preservation, that "Dr. Weinland succeeded in actually determining three genera."

"Of one characteristic bluish sponge which occurred in several favourable shavings (into which the stone was cut), both as young and old specimens, he was able to make a drawing of its interior construction as easily as from a living specimen."

These startling discoveries point to the fact that in the world where these rocks and animal forms originated, the course of organic evolution was very similar to that which has taken place upon our earth. But these eminent savants are of the opinion that discoveries will yet be made of organisms for which no place can be found in our systems of zoology. The petrifactions all belong to the subaqueous classes of animals of the lower and more primitive forms, and up to this time no trace has been found of any higher animals, as mollusks, &c.

Dr. Hahn has published a book giving an account of this wonderful discovery, which is illustrated with engravings of more than one hundred specimens of these organic remains, no two of which are alike. These have been reproduced by photography, which does not lie. Further investigations in this new and untrodden field of research will be awaited with the most profound interest.—*Chicago Herald.*

DURATION OF LIFE.

An article lately appeared in the *British Medical Journal* on "Has the duration of Human Life in England increased during the last thirty years?" by Dr. Rabagliati. I have condensed most of it for your readers. From 1838-40, the average death-rate was 22·3 per 1,000; in 1876-9, it was 21 per 1,000; or an average of 44·8 years against 47·6 years; this gives an addition of 2·8 years to each one's life. If our population is 25,798,922 people in Great Britain, then 72,236,981 years of life are added to a generation.

The improvement, he says, is due to better ventilation and drainage, less overcrowding, the destruction of fever dens, &c. This addition, when analysed, shows that infant life is more certain, whilst males above 35, and females above 45 are dying faster than thirty years ago.

Between 1851 and 1878, 360·5 male children under five years died per 1,000; in 1879 only 319·5 per 1,000; this is an addition of 11·1 per cent. to life. If we still compare these dates we find that between 5 and 10 years, 19 per cent. was added; 10 and 15 years it was 24·1 per cent.; 15 and 20 years it was 22·6 per cent.; from 20 to 25 it was 22·6 per cent.; from 25 to 35 years it was 11·3 per cent. In females, comparing same data, it was 11·1, 22·1, 24·4, 25·1, 23·7, 16·7; and from 35 to 45 years there was a saving of 5·8 per cent. in 1879. Males in 1879 from 35 to 45 years died 1·5 per cent. faster than before; 45 to 55 years, 2·6 per cent. faster; 55 to 65 years, 11·9 per cent.; 65 to 75 years, 7·8 per cent., and above 85 years, 7 per cent. faster than from 1851 to 1878. Females between 45 to 55 years 6 or $\frac{6}{10}$ per cent. died faster; from 55 to 65 years, 12·2 per cent.; 65 to 75 years, 11·6 per cent.; 75 to 85 years, 9·5 per cent.; above 85 years, 4·8 per cent. died faster. Examine, then, every 10 years and we find in 1841 to 1850 the death-rate was 22·4 per 1,000; 1851 to 1860 it was 22·2 per 1,000; 1861 to 1870, it was 22·5 per 1,000; and from 1871-9 (nine years) it was 21·5 per 1,000. The improved rate of health has taken place since 1870. Deaths from zymotic (infections) diseases have lessened; thus from 1850 to 1854 they were at the rate of 5,234 per million living people; 1855 to 1859, they were 5,039 per million; 1860 to 1864, they were 4,899 per million; 1865 to 1869, they were 5,172 per million; 1870 to 1874, they were 4,849 per million; 1875 to 1879, they were 3,911 per million of living beings. From 1850 to 1854 the death-rate was high from these causes; in 1877 to 1879 it was lower from them; but from other causes it was higher than from 1850 to 1854, minus these. Thus it seems that in those days the people were healthier than our present race—it was a case of survival of the fittest, the weakly ones then died and so increased infant mortality; now the weaker ones live, and die soon after manhood, and swell its mortality, instead. The diseases that attack young lives are also becoming less fatal, such as scrofula and consumption, whilst diseases of more advanced life are, on the other hand, increasing in fatality, such as cancer. This reads us a terrible lesson, and one to which everyone's attention should be drawn, for we undoubtedly hold our lives in our own hands; so it depends with ourselves, in a great measure, whether we die at 35 or 75 years.

T. R. Allinson, L.R.C.P.

KNOWLEDGE FOR THE YOUNG.*

An idea seems to prevail that the best way to make knowledge palatable to the young, or to beginners generally, is to dilute it: if a reader finds one fact in a page he can digest it, and better still if he finds one fact in half-a-dozen pages. Then, again, many imagine that knowledge must be sweetened to suit the taste of young folks: we must be poetical and use flowery language, or they will turn from knowledge in disgust. We believe all this (we may say we know it) to be entirely erroneous. If it were true, the book before us would be utterly unsuited for young people; but, as a matter of fact, it is just the sort of book they want. There is no poetry, no flowery language, no attempt to interest by fine talking; and yet the book is full of interest from cover to cover, and full of poetry, too, for those who can read between the lines. Take the opening words. "Coal.—When wood or woody matter such as roots, bark, leaves, moss, peat, and so on, lies buried in the earth for a very long time, it changes into coal. Wherever coal is now found, there were once forests, or woods, or peat, or else mouths of rivers or other places where plants grew, or their dead parts, or leaves, or branches, and roots got heaped together. Then such places get covered with water, often because they sank below the level of the sea, and the sea covered them, and they were at the bottom of the sea. Then the mud which rivers bring down to the sea sank down and covered them, and the shells and skeletons of dead sea animals fell also upon them, so that they got covered deeper and deeper." Then the later stages of coal formation are described, without a wasted sentence. And at the foot of the second page the coal has been followed to ashes, cinders, coke, and breeze (broken up coke), and so the first chapter ends.

How much more sensible this than a page or two of pretty talk about the cheerful blaze of our home fires, the comforting warmth of stoves, and so forth, followed by vague mention of the work coal does in manufacture, &c., with then, perhaps, a reference to the forest primeval, and poetical picturings of the work done by river and sea. We have before us a work, on another subject, in which just such a course is followed. Six pages are employed in saying what might very well be said in half a page, and then the author has the face to say, "let us recapitulate",—the reader being all the while treated as a child might be to whom a bitter medicine was to be given in a spoonful of sweetstuff.

Now, children as a rule (though elementary books are best written when not specially meant for children) care very little for poetical descriptions, and a great deal for facts. They often find a good deal of poetry *in* the facts; indeed, we can imagine few better lessons for those who want to put poetry into their science books than to tell their facts to a bright child, and to listen to its comments. If you want thoroughly to interest a child you must not dilute, but condense. An intelligent child's digestive capacity for information is greater (not less, as many fondly imagine) than that of most grown persons. The usual way of treating readers of "first books" is as unwise as would be the use of baby food for growing boys.

Let parents try the experiment. Give a boy a so-called scientific book full of poetry and platitudes, with the science disguised out of all knowledge, and note how he will weary of it. Then give him such a book as Professor Guthrie's "First Book of Knowledge," and see how much the lad will prefer good meat to pap. Some reviewers criticise such books as this on the score of what they call a common-place style, not knowing that the true eloquence of the scientific writer resides in clearness and plainness of speech. So judged, Professor Guthrie's style is excellent.

The subjects dealt with in this capital book are classed under the following heads:—"Things and Stuffs used for House-building, The Elements, Wood, Heating and Lighting, Finishing and Furnishing (of houses), Clothing, Food, Cleaning, Writing, and Printing." There are questions at the end of each chapter, but we believe that what is learned for question-answering is not properly learned at all. A clever boy should take such a book as this for the mere pleasure of satisfying the mental craving (natural in all healthy minds) for information,—which is the mind's plain food.

* "The First Book of Knowledge." By Frederick Guthrie, F.R.S. (Marcus Ward & Co., London.)

Letters to the Editor.

[The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with their writers. All communications should be as short as possible, consistently with full and clear statements of the writer's meaning.]

All Editorial communications should be addressed to the Editor of KNOWLEDGE; *all Business communications to the Publishers, at the Office, 74, Great Queen-street, W.C.*

All Remittances, Cheques, and Post-Office Orders should be made payable to Messrs. Wyman & Sons.

**** All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.*

All Letters or Queries to the Editor which require attention in the current issue of KNOWLEDGE, *should reach the Publishing Office not later than the Saturday preceding the day of publication.*

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

Our Correspondence Columns.

A PRETTY GEOMETRICAL PROBLEM.—FOX AND GUNS.—INTELLIGENT DOG.—FOUR FOURS.—MISSING LINKS.—THE MAGIC WHEEL.—FUNGI AND LICHENS.—FLESH FOOD.—URSA MAJOR, VENUS, AND THE NORTH POLE.

[184].—"Student" gives the following puzzle, to which he says there are twenty-five solutions; but there are really many more. We withhold his solution:—

Take a square piece of paper, and remove from it a corner square (equal to one-fourth of it), leaving three small squares in one piece. It is required to divide this three-square piece so that the parts can be put together to form a single square.

At the same time we submit the following puzzle, sent us by "Mogul," who promised, but did not send, a solution:—

Given any rectangle, to divide it by the fewest possible straight cuts, so that the parts can be put together to form a square.

J. Rae points out that from the way in which the string is fastened, which Mr. Henslow considered a fox might pull "out of the line of fire," our interpretation of these words (letter 158, p. 187), cannot be correct. The fox would inevitably be in the line of fire in touching the string; and in the condition in which the string usually is, would run great risk. Such a fox would "do a most foolish thing, as foolish as a sportsman who should drag his gun on full cock by the muzzle through a hedge."

The Rev. J. P. Sandlands describes how a strange dog stopped him in Westbourne-park, until he responded to the appeal in the dog's eyes that a gate leading to the dog's master's house should be opened.

E. Clarkson, Geo. D. Brown, J. A. Miles, Nemo, F. J. J., and others, give solutions of the four fours, for numbers from 1 to 20 inclusive, except 19. Other problem, Yarletoman gives for 19 the formulæ.

$$(+)\ \frac{4}{\cdot 4} + \frac{4}{\cdot 4} \text{ and } \times\ \underline{|4} - 4 - \frac{4}{4}$$

F. J. J. gives $\sqrt{\frac{4}{\cdot 4}} - \frac{4}{4}$

which is manifestly erroneous. E. Clarkson and F. G. Hooton give for 19, $\frac{4 + 4 - \cdot 4}{\cdot 4}$ J. Bosworth points out that four 3's will give the same numbers as three 5's (except for number 17); and that 19 may be given for any number whatever by the formula $\frac{x + x - \cdot x}{\cdot x} = 19$. It seems to me that as "Amicus Scientiæ" gives, like the others, for 11, the formula

$$\frac{44}{\sqrt{4} + \sqrt{4}}$$

in which the first 4 in numerator really stands for ten times 4. ·4, which stands for 4 divided by 10, is admissible for 19. Otherwise 11, as well as 19, must be regarded as insoluble. The solutions given by "Amicus Scientiæ" are as follows:—

No. 1. $\frac{44}{44}$

2\. $\frac{4}{4} + \frac{4}{4}$

3\. $\frac{4 + 4 + 4}{4}$

4\. $4 + 4 - (\sqrt{4} + \sqrt{4})$

5\. $\frac{4 \times 4 + 4}{4}$

6\. $\frac{4 + 4 + 4}{\sqrt{4}}$

7\. $\frac{4 \times 4 - \sqrt{4}}{\sqrt{4}}$

8\. $\frac{(4 + 4) \times 4}{4}$

9\. $\frac{(4 \times 4) + \sqrt{4}}{\sqrt{4}}$

10\. $4 + 4 + 4 - \sqrt{4}$

No. 11. $\frac{44}{\sqrt{4} + \sqrt{4}}$

12\. $4 \times 4 - (\sqrt{4} + \sqrt{4})$

13\. $\frac{44}{4} + \sqrt{4}$

14\. $4 \times 4 - (4 \div \sqrt{4})$

15\. $\frac{44}{4} + 4$

16\. $4 + 4 + 4 + 4$

17\. $4 \times 4 + \frac{4}{4}$

18\. $(4 \times 4 + 4) - \sqrt{4}$

19\.

20\. $4 \times 4 + (\sqrt{4} + \sqrt{4})$

"Another Ignoramus" writes that the argument of the evolutionists is not strengthened by showing that there are many missing links. He supposes Prof. Wilson "would have us believe that man is connected with some anomalous creature which exists only in his own imagination." Dr. Wilson holds in reality, as every evolutionist holds, that man is related to the Simians; and it is not a mere fancy, but an obvious common sense deduction from what is known about evolution, that the Simians must have changed from the common ancestor as much as man as changed; so that since man differs widely from all Simians, however obviously related to them, both man and Simian must differ widely from the common ancestor of both. "Another Ignoramus" seems to overlook the circumstance that no one has called him an ignoramus but himself. At the close of his letter he says he can see no reason why we should cease to admire the wisdom of God in creation. Neither does any one else. What the evolutionists really say is that we do not quite so thoroughly understand that wisdom as men formerly thought they did. Instead of a finite, and therefore possibly intelligible plan, we find a plan which is infinite alike in time and space, and therefore for us absolutely unintelligible. We can follow its workings over an ever widening domain, but, with the widening of this domain, the domain around, which may be regarded as that of the unexplained, is ever enlarging. Man in the past may be compared to a child, who, having been in ignorance up to a certain age of all the laws of nature, all which constitutes what we call knowledge, should have been told that everything had been fashioned just as he sees it, in a moment of time, out of nothing. If such a child, observing what takes place around him, should gradually learn something of the operation of the simpler laws of nature, he might imagine in his ignorance that they implied, not the operation of a Being living in and through all things, but the blind action of mere matter. Or if, instead of observing these things himself, he was simply told about them by others, he might wish to close his ears and his eyes, lest he should "cease to admire the wisdom of God in creation." The fear would be very idle and foolish, we know. Even the opponents of evolution must see that. But they act in this way themselves, and expect to be regarded as exceptionally devout admirers of the wisdom of God, because they would limit His domain in time, and narrow it in space.

C. H. Wingfield points out that by having eleven slits (at equal distances) instead of ten, the horses round the magic wheel seem not only to move their limbs but to advance. Certainly with the same number of slits as horses, the horse presents a somewhat singular appearance, flourishing his legs about, but not getting over the ground. In *La Nature* the picture in the *Scientific American* is given without change, viz., twelve figures of the trotting-horse, the uppermost of our view and the one next to it on the left being given twice over. We have tried the magic wheel with this arrangement, and find that the horse seems to trot with a singular extra kick once in each double stride. We have not yet tried Mr. Wingfield's plan, but have no doubt it would act as he says.

E. C. Cowley desires to learn Mr. English's address from Mr. W. W. Highbury, and to know in what sense the "subscription list is open" to any one desiring a copy of the book.

"A Fellow of the Chemical Society" points out that if it can be shown, as he believes Mrs. Kingsford has shown, that man in physical formation is nearest to the apes and in striking contrast to the flesh-eaters, the natural food of man must be that which we find apes eating. Might one not argue, almost as effectively, that men should limit their pursuits to those which satisfy their Simian cousins? I do not say men might not live, and thrive, and work, and think, on a diet of nuts, fruits, and vegetables, or that they do not, as a rule, eat much more flesh than is good for them; but we must not adopt a false system of reasoning even to establish the truth. "F. C. S." states what is more to the point, that many who

have suffered from dyspepsia when living on mixed food, have been freed from the malady after (and presumably on account of) a change to a vegetable diet. I wish there were space for his letter in full, but there is not.

"A. G. P." has seen parts of Ursa Major in lat. 20° south, and a shadow thrown by objects in light of Venus. He asks what phenomena beyond total absence of centrifugal force might reasonably be expected at the pole? Extreme cold, we should say, the pole of the heavens over head, sun visible for more than half a year and invisible for the rest of the time. RICHARD A. PROCTOR.

CHEAPENING OF APPARATUS, AS AN AID TO THE DIFFUSION OF SCIENTIFIC KNOWLEDGE.

[185]—At the beginning of a New Year, I venture to suggest that the most worthy and laudable end to which KNOWLEDGE so especially addresses itself—that of the diffusion of sound popular scientific information—might be very considerably advanced by that invaluable class of men, the optical and philosophical instrument makers, if they would only address themselves to the production of apparatus at a cost to bring it within the means of those whose pecuniary means are (like the joint-stock companies) "limited." Conducting some experiments in chromatics recently, I required a double-image prism, for the purpose of superposing coloured discs; but on a friend inquiring on my behalf at two separate shops, he was asked 12s. 6d. for one at one of them, and 15s. at the other! Now, we know pretty well what Iceland spar costs in the rough, and I feel tolerably confident that a prism mounted in a simple bit of brass tubing might be sold at a good profit for half-a-crown. Such, though, is the trade conservatism among those concerned in the manufacture of such things, that they would much rather sell one prism at 15s. than eight for a sovereign. *Mutatis mutandis*, these remarks would equally apply to the transit instrument. There can be no doubt that a real want exists for a simple meridian instrument, which shall be at once trustworthy to use and cheap to buy; but £15 represents the present price of the smallest transit made which is really serviceable. Here, then, is an opening for some enterprising scientific artificer, at once to benefit himself and the community at large. An economical transit instrument once produced, it may be worth the while of clock and chronometer makers to try whether a cheap compensated sidereal clock is quite an impossible thing to construct? The achromatic telescope has been cheapened, and a really efficient photographic camera can now be bought for a small sum. In other types and classes of instruments, however, the opticians are content *stare super vias antiquas*. It is in hopes of rousing a few of the more energetic among them to a course of action which must ultimately benefit them as much as it undoubtedly must the great army of scientific students in this country, that these lines are written.

A FELLOW OF THE ROYAL ASTRONOMICAL SOCIETY.

INSTRUMENT FOR DESCRIBING THE CONIC SECTIONS.

[186]—In answer to "F. R. H. S.," I may say that in Fig. 2 (p. 160) the central leg of the instrument is held at an angle corresponding with the amount of eccentricity of the intended ellipse, while the moving leg slides up and down the same, thus describing a section of a cylinder.

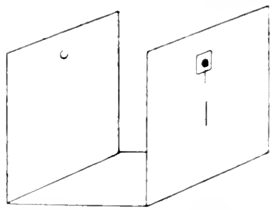
As to Fig. 1, the idea was suggested by the shadow of the upper part of the globe on the gas bracket, cast by the glimmer of light usually left in, on the walls of my bedroom, the style shown moving round a circle simply performing the part played by the rays of light. THOMAS THORP.

A LUNAR ILLUSION.

[187]—"S. H. W." will perceive, on reconsideration, that he has made a mistake in his letter, No. 64, with the above title. The illusion mentioned is only an illusion, and it is solely connected with the projections of the moon and sun on the sphere of vision, which may have any radius we choose to give it, and not with their unequal distances from the eye. In the case supposed by him, viz., when the moon is in the zenith, there will be no illusion; the observer would naturally stand facing the sun, and he would look "square" at both moon and sun, and be able to produce the sagitta correctly, by the eye, to hit the sun. "S. H. W." seems to be thinking of the plane containing his eye and both luminaries; he is thinking, too, of the sun itself, and not of its above projection. The sagitta never points to the sun itself, except at half-moon. If we knew the angle between the sagitta and the line joining the moon and the sun itself, and observed the angular distance of those two luminaries, we could get their proportional linear distances from the earth. But we can never see that angle, since we are in the plane of it; we know, however, its value at half-moon, viz., zero; but we cannot tell, by the eye, any better than Aristarchus, when it is half-moon precisely, and therefore should fail, as he did, to get the relative distances of moon and sun. M.

OPTICAL ILLUSION.

[188]—Take a strip of thin card about three inches long and one wide, bend up the ends at right angles, perforate one end with a pin-hole and the other directly and concentrically opposite with a square hole about one-eighth of an inch square; place a small pin in



From *Nature*, vol. xxiv., p. 53, 54.

this end, so that the head may be in the middle of the square hole; apply your eye to this end, and look through both holes towards the sky, and the pin will be seen apparently beyond the pin-hole and reversed in position. A. T. C.

THE FIFTEEN PUZZLE.

[189]—If we are allowed to turn the board we can pass from the lost to the won position in thirty-nine moves.

For instance, we may move the pieces numbered 12, 8, 4, 3, 2, 1, 5, 9, 13, 15, 14, 12, 8, 4, 3, 2, 1, 5, 9, 13, 10, 11, 12, 8, 4, 3, 2, 1, 5, 9, 13, 10, 14, 11, 7, 6, 10, 14, 15.

There is no solution in less than thirty-nine moves, for thirty-seven moves are needed to move the blocks by the shortest paths from their first to last places; and the other two moves are needed because the four blocks in the middle cannot move round until one of them steps out into the outer row, from which it must again return.

I gave the above solution and proof in the *Brighton Herald* in May, 1880.—Yours, &c. ARTHUR BLACK.

J. Watson gives a similar solution, thus:—4.8.12.R.—12.3.U.—5.9.13.L.—12.14.15.D.—3.4.8.R.—1.2.U.—6.10.11.L.—8.12.D.—2.3.4.R.—5.6.1.U.—9.13.L.—11.14.D.—6.7.R.—9.5.U.—13.14.15.L.

PNEUMATIC BELL.

[190]—The pneumatic bells referred to by me (letter 79, p. 100) were supplied and fixed in my house for me by a firm at King's-cross, where "E. R. C." can obtain every information. I could not instruct in the manufacture of the article. Although the fixing of the three requisites, viz. (1) pipe, (2) press-button, and (3) bell, is very easy, yet space in KNOWLEDGE is, I apprehend, too valuable for such detail as gas-fitter's work. All I can say and assure "E. R. C." of is this—a more simple, reliable, cheap, and instant signal, produced only by compression of the air we breathe, cannot possibly be obtained.—Yours truly, M. TESTER.

P.S. I am sorry to say I could not procure KNOWLEDGE at the bookstalls of Birmingham or Lichfield, where I have been staying the last fortnight, so I have not seen the recent issues until my return to London.

INFLUENCE OF SEX ON MIND.

III. HISTORICAL EVIDENCE.

[191]—History is conclusive as to man's mental supremacy. The inventing, creating, cogitating mind is masculine. Men carry on the world's business in thought and action. The ideas on which depend all the marvellous acts of human intelligence, scientific discoveries, jurisprudence, political, civil, military institutions, maintaining the social structure, are elaborated by men. In the domain of pure intellect it is doubtful if women have contributed one profound original idea of any permanent value. Men legislate, govern, invent, colonise, fight, build, and dig. So little demand is there for woman's *direct* help in the mental departments which are men's special province, that, could all the male intellect in the world be suddenly paralysed or annihilated, there is not in woman

sufficient development of the abstract principles of justice, morality, truth, causality, inventive, and executive power to hold society together for one week.

"In matters intellectual and moral the long strain beats them dead. Do not look for a Bacona, a Newtona, a Handella, a Victoria Hugo. American ladies tell us education has stopped the growth of these. No; these are not in nature. They can bubble letters in ten minutes which you could no more deliver than a river can play like a fountain. They can sparkle gems of stories, flash little diamonds of poems. The entire sex has never produced one opera or one epic that mankind could tolerate for a minute—and why? These come from long, high-strung labour." (Mr. Charles Reade, in "White Lies.")

Women lack the highest quality of the human mind—*justice!* They never see two sides of a question. A woman makes a firm friend—a dangerous enemy.

The eternal subordination of woman is conclusively exemplified in her exaggerated admiration for the male prerogatives—strength and intellect. Were intellectual sexual equality not an idle dream, it would long ago have produced practical results. The strong-minded woman would have proved her pretensions. Woman's individuality and independent action in important matters are more apparent than real. Savage life shows the nearest approach to sexual equality, physical, mental, and moral. Yet among savages woman is a slave! In civilised countries, where she is free, almost every woman is steered through life by the reflecting brain, strong will, and protecting arm of a husband, father, brother, or son. A woman with no male relative has her spiritual director, her confessor, or favourite preacher, her conscience-keeper, whom she regards as a superior being. Even revolutionary women are guided by men. Platform ladies worshipped Mr. J. S. Mill. They could not understand his works on Political Economy and Metaphysics, but he advocated Woman-Suffrage! If there is one woman without such a director, she is guided by male public opinion, supplemented by oracles uttered by men in the past.

Mentally, morally, and physically woman is subordinate to man; although the meek idolator sometimes adores a brazen god!

J. McGRIGOR ALLAN.

POSSIBLE VARIATION OF PENDULUM.

[192]—A letter from "Cogito" in KNOWLEDGE (No. 8, p. 143) refers to mine (in No. 6, p. 88). He speaks of "want of precision of ideas;" true, I am alluding to a stationary pendulum; he is speaking of a vibrating pendulum.

The point is this: suppose a pendulum suspended in a railway carriage. On the train starting, motion must be communicated to the bob through the rod, and the pendulum will be thrown out of the perpendicular backwards; on slackening, the bob will be thrown forwards, momentum having been given and then withdrawn. The motion of the earth round the sun in the short space of twelve hours may be considered as in a straight line, a b, the centre of the earth, travelling uniformly a to b, but the sides nearest to and furthest from the sun will travel, the one 1 000 miles faster, the other 1,000 miles slower (as the upper and lower points of a carriage wheel). The velocity of the bob in the direction a b will, therefore, vary 2,000 miles per hour between midday and midnight, the acceleration and retardation being communicated from the point of suspension through the rod to the bob. If this action were rapid, the effect would be evident enough, the difference of velocity being great, but the time is long, and in consequence the movement of the bob would no doubt be extremely small. The question is, would it, with a rod say of 100 ft. in length, be perceptible under suitable delicate measurement?

HENRY CARR.

RATTLESNAKES.

[193].—You enjoy one great advantage over me. You are like a priest in a pulpit, who from his elevated post can, with impunity, hurl his censures on the devoted heads of obscure sinners like me, who occupy the low free seats, and are debarred from saying a word in our own defence publicly.

Mr. Darwin's discourse on the rattlesnake is to be found at pages 107—110, in his book on "Expression," &c. (first edition). I maintain that the passage is susceptible of the meaning I attached to it; for if your explanation is correct, viz., that Darwin attributes "the habit of the snake to its development."—[We did not say this. "Rather," we said, "he would," &c.—ED.]—then we are landed in the poor, paltry platitude, that when an animal possesses an organ, the creature makes use of it. Most wonderful, truly! But surely it was hardly worth while to write three pages of "information" to establish such an obvious, vapid circumstance—such a feeble inanity.

The cause and mode of developing the rattle are, however, a secondary consideration in comparison with the use to which the snake is said to devote its organ, when it is found, viz., "to frighten its enemies." I contend that the means of accomplishing the assigned purpose are simply suicidal, and that the noise of the rattle attracts the snake's enemies who seek its destruction.

I have read somewhere that in America, when the workmen in the woods hear the rattle they are sure there is no danger, but directly the sound ceases they infer that the snake is bent on mischief. So here it is not the noise of the rattle, but its silence, which is a source of alarm.

You are quite right in thinking that Mr. Darwin has never done me any injury. On the contrary, I believe him to be an estimable man, and incapable of injuring any one intentionally; but his writings appear to me to be a great offence to the Creator, to Nature, and to common sense, and therefore that his productions ought to be denounced. They have also set an example of a lax scientific method. In your own columns, at page 153, a disciple of Darwin's writes in this style on the origin of the grape:—"*Suppose*, however, that any plant *happens* to have its seeds covered with a moderately hard and indigestible coat," &c. And again: "If such a tendency were ever to be *set up* even to the slightest degree by a *mere sport* or *chance* variation," &c.

I venture to think that we shall never properly comprehend God's works in all their might and majesty of original design while we deal with them in such a puerile fashion; and with this sentiment I now respectfully take my leave of you.

NEWTON CROSLAND.

[We insert this letter, though doing so is, we fear, rather hard on Mr. Crosland.—ED.]

LONGEVITY IN ANIMALS.

[194]—In your article on "Food and its Relation to Muscular Work," you only appear to treat with what I would call active beings. I should like to know what you have to say about inactive animals, such as turtles and tortoises. I presume to call them inactive on account of their sluggishness and their dormancy. It is a well-known fact that they will live a long time without having partaken of any visible means of sustenance. I have known turtles to live without food for weeks out of their own element. A wrong idea exists with a good many people as regards the heart of a turtle. Many do believe that they have three hearts. I have examined the heart of a turtle, and I have come to the conclusion that what is taken to be three hearts is but one externally divided into three parts, not like the human heart, which is divided into four, and then encased in a membrane, called a pericardium. Can you tell me if I am right? The late Frank Buckland, when looking at the turtles in the tank at the Adelphi Hotel, Liverpool, supposed they live on the water they take in, then on their fat and blood. How is it they live a long time without water and pass urine? If any of your readers can explain, it would be cordially received by those readers physiologically inclined.—Yours most respectfully,

PHYSIO.

TOADS, &c.

[195]—In reference to query 87, and the doubts expressed as to the existence of toads in rocks of an earlier than the tertiary formation, it is an acknowledged fact amongst miners that toads have been extracted living from the solid coal, at various depths. I heard of one found two months ago in a pit 400 yards deep, near Oldham, where the coal, with the hole where the toad had been imprisoned for countless ages, was preserved, and the toad, although alive when reached, died on being exposed to the atmosphere. If "A Fellow of the Royal Astronomical Society" will make inquiries of scientific men in that district, he will learn the whole truth.

I have heard miners relate the extraction of one from the Lanarkshire coalfield, which was heard croaking before it could be liberated, and so frightened the working colliers that they fled, and would not return without the manager was present, and under these conditions the toad was hewn out, in the presence of several witnesses. Although it was rolled carefully in a wet cloth, it died before it could be taken out of the mine. It was deposited in a local museum, and no one dared to contradict the fact at that time.

ZARES.

[196]—I am only an inquirer without time or means for systematic observance, but I am interested in the query of "Arachnida" (87), and submit that I was disappointed in "A Fellow of the Royal Astronomical Society's" treatment of it (138), page 165, which must be my apology for troubling you with the copy of "cutting" from my scrap-book herewith enclosed.

A tradesman at Bromley, Kent, observed me examining flint pebbles which abound in that district and volunteered the statement, that on breaking a similar one to that I held in my hand, a

have toad hopped out, not very long ago. No allusion had been made to anything of the kind previously, and he could have had no motive for saying what was untrue.

I presume these fossiliferous flints in "the crag" are well known, although I find no particular notice of them in geological works in my possession; but considering the cells in them and the channels by which they are approached from outside, is it possible for a new tenant to be inducted and developed such as the toad?

If Sir W. Thomson could speak seriously of a Colorado beetle surviving a voyage through space in a meteoric stone (see report of British Association Meeting at Plymouth, 1877—Mathematical and Physical Section), I hope you will pardon me if I have strained a point on the capabilities of a toad.—Yours, &c., W. B.

"A CURIOUS FACT.—Many years ago a friend of my father's built a country house, which he fitted up and furnished according to his own taste; to accomplish this he caused to be brought from Italy a piece of pure white marble, out of which a mantelpiece was constructed for his own particular sitting-room. The mantelpiece was of singularly pure marble, in one block, and free from flaw, save in one part. Shortly after its erection, the owner of the house noticed a small damp-looking stain, no bigger than the nail of his little finger in the very centre of the mantelpiece. This, however, was so slight a blemish that it did not trouble him, till, as months and years went by, it became evident that the mark slowly but surely increased in size. For twenty years the good man of the house sat in his arm-chair facing the curious stain and marvelling what caused its certain spread. At the lapse of that period it had increased to the size of the palm of his hand, and he could no longer rest in patient contemplation of it. Masons were sent for and desired to take down the marble and break it in two, so as to disclose the mystery. This was done, and to the amazement of all, out hopped an enormous toad!"—"H. A. F.," in *Chatterbox*.

INTEREST ON A FARTHING.—AN APPLICATION OF LOGARITHMS.

[197]—As the nature of compound interest is little understood by many, we will assume that a farthing was placed out, at compound interest at 5 per cent., payable yearly, commencing at the Birth of Christ, and extending over time till the end of the year 1880. Now, the mean diameter of the planet Jupiter is 88,645 miles; the weight of a cubic foot of pure gold equals 17,486 oz.; and the value of the gold being at the rate of £3. 18s. per ounce; how many globes of pure gold, each as large as Jupiter, would that interest purchase?

The principal and interest of £1 for one year	1·05 log.	0·021189
Multiply by the years		1880
		1695120
		169512
		21189
Raised to the 1880th power		39·835320
Subtract the log. of the farthings in a £.	960	2·982271
Log. of the amount of interest for the given time, equals	7129 +	36·853049
7129 + (thirty-three more figures).		
Diameter of Jupiter in miles	88645 log.	4·947654
Feet in a mile equals (1760 × 3)	5280 „	3·722634
Feet in Jupiter's diameter		8·670288
Raise this quantity to the 3rd power		3
The diameter raised to the 3rd power		26·010864
Add the log. of $\frac{1}{6}$ (3·1416)	·5236 „	·1·719000
Solid contents of Jupiter		25·729864
Weight of a cubic foot of gold in oz.	17486 „	·1·242680
Value of one ounce of this gold, £3. 18/.	3·9	·0·591065
Value of a solid globe similar to Jupiter equals	3661 +	30.563609
3661 + (twenty-six figures)		
Amount of Interest (Log.)	36·853049	
Value of Jupiter	30·563609	
Log. 1947331	6·289440	

It therefore appears that the interest upon a farthing for 1,880 years, at 5 per cent., would purchase 1,947,331 solid globes of pure gold, each as large as the planet Jupiter. Yours, &c., PASCAL.

[The calculation is a pretty illustration of the value of logarithms. The mean diameter of Jupiter is much less than 88,000 miles so that the legal representatives of the original owner of the farthing can claim from the bankers with whom that farthing was placed at interest, a much greater number of gold Jupiters. But that is a detail.—ED.]

THE HOG PUZZLE.

[198]—The following problem may serve to amuse some of the many young readers of KNOWLEDGE who are conversant with the elements of Algebra. It was given me by a young lady, but the analysis is my own.

Question.—Three Dutchmen, Hendrick, Elas, and Cornelius, and their wives, Gurtrün, Katrün, and Anna, purchase hogs. Each buys as many as he (or she) gives shillings for one. Each husband pays altogether three guineas more than his wife. Hendrick buys 23 more hogs than Katrün, and Elas 11 more than Gurtrün. Require the name of each man's wife?

I call this a "puzzle," because I venture to think that nineteen out of twenty would attempt its solution by the common process of simultaneous equations, and would certainly fail, because there are more unknown quantities than the number of independent equations it is possible to construct. The solution is, however, obtained in a very simple manner, thus*:—

For brevity, denote the men and women by their initials H, E, C, G, K, A, and let the corresponding small letters h, e, c, g, k, a, represent the number of hogs (equal to the payment for one) purchased by each respectively.

Then h^2, e^2, c^2, g^2, k^2, a^2 are the sums expended by each. Thus H purchases h hogs for h^2 shillings; E, e hogs for e^2 shillings, &c. Also 3 guineas = 63 shillings.

Observe (1) that h, e, c, g, k, a must be positive integers; and (2) that if m and n are any positive integers, such that

$$m^2 - n^2 = 63$$

or $(m+n)\ (m-n) = 9 \times 7 = 21 \times 3 = 63 \times 1$,

there are three, and only three, possible values of $m+n$ corresponding to three of $m-n$.

If $m+n=9$, $m-n=7$, which gives $m=8$, $n=1$

„ $m+n=21$, $m-n=3$, „ $m=12$, $n=9$

„ $m+n=63$, $m-n=1$, „ $m=32$, $n=31$

Suppose now m to be the price (in shillings) paid for a hog by a man, and n that paid by his wife. It follows that m may have three values, *viz.*, 8, 12, 32, corresponding respectively to three values, 1, 9, 31 of n. Also, since each man is the husband of some woman, and each woman the wife of some man, whatever arrangements may exist between the quantities h, e, c, and g, k, a, each is susceptible of three values. Any one of the quantities, h, e, c, may have a value of m, provided its corresponding quantity in the groups, g, k, a, has the corresponding value of n.

But there are two equations of condition.

$$h - k = 23 \quad . \ . \ . \ . \ . \quad (1)$$

$$e - g = 11 \quad . \ . \ . \ . \ . \quad (2)$$

Referring now to the values of m and n, we find that to satisfy

(1) we must have $h=32$, $k=9$;

. . . . (2) . . . . $e=12$, $g=1$.

We may infer from this that $c=8$, $a=31$.

But to verify our inference, substitute for h, e, k, g in the general equation

$$h^2 + e^2 + c^2 - (g^2 + k^2 + a^2) = 3 \times 63$$

the values just found, and we have

$$a^2 - c^2 = 897,$$

which can be satisfied with no other possible values of a and c than $a=31$, $c=8$.

Having obtained the number of hogs each man and woman has purchased, we at once observe that

$h^2 - a^2 = 63$, and therefore that A is the wife of H,

$e^2 - k^2 = 63$, „ K „ E,

$c^2 - g^2 = 63$, „ G „ C.

Yours, &c., I. R. CAMPBELL.

MOCK SUNS.

[199]—On Monday, Dec. 19, 1881, between two and half-past in the afternoon, on Wandsworth Common, I saw two spectra, irregular in shape, with apparent diameters about twice that of the sun, one on each side of and apparently equidistant from the sun. They were at the same apparent height from the horizon as the sun, and, by guess, appeared to be 25° from the sun. The colour-bands were

* The young reader should try to make out the solution for himself, before reading what follows.

vertical and distinct, the red band being nearest the sun. A band of white light descended from each spectrum half way to the horizon. These bands appeared slightly curved towards one another. The sun was shining through light, ill-defined, massy clouds. FRED. W. FOSTER.

[Distance, theoretically, about 22½° from the sun.—ED.]

SHAPE OF SATURN'S SHADOW.

[200]—Is not the distorted shadow caused by the irregularity in the density of Saturn's rings? I offer this as a suggestion, because I have frequently observed most eccentric forms thrown by a strong light upon mist, totally out of drawing with the object causing the shadow,—Yours, &c., F. YBLES.

[The irregularity of the shadow may be partly due to this cause, but cannot be wholly thus explained, as Mr. Ybles will see if he considers that we see the shadow in nearly the same direction that it is thrown.—ED.]

RING OF LIGHT ROUND MOON.

[201]—Last night (Jan. 1), at 10.5 p.m., I and several others saw a complete ring of light round the moon at a distance from her of about twenty-six moons' diameters. The sky was somewhat hazy at the time, and half-an-hour later was covered with heavy clouds. If you can afford space for an explanation of this fact I shall be much obliged.—Yours, &c., E. W. P.

[What you saw was a lunar halo, caused by the refraction of the lunar rays through ice crystals. You must have considerably under-estimated its apparent distance from the moon, which would be nearer forty-two than twenty-six diameters of the moon.—ED.]

WINDMILL ILLUSION.

Referring to the letter 161, p. 187, I dare say you will remember, at Cambridge, another "illusion," whose discovery was ascribed to Dr. Whewell—viz., look from a point about a quarter of a mile distant, edgeways, at the sails of a windmill in rotation, and you can make them go whichever way you please. THOS. S. BAZLEY.

[So also, if any one at some like distance whirls round a ball attached to a string, the plane of the ball's motion being aslant, so that the ball seems to describe an ellipse, it is difficult to tell which way the ball is travelling, when the distance prevents us from seeing whether it passes on the nearer or farther side of the swinger, when lowest.—ED.]

RICHTER'S DREAM.

[202]—When lecturing at Sheffield in connection with the Gilchrist Fund, you, on two occasions, concluded your lectures with a splendid recitation about an angel taking a man through the realms of space. Will you kindly inform me who is the author of the poem, where I can obtain it, and—if it be not asking too much—the price? I have been told it is one of Jean Paul Richter's "Rhapsodies," and have ordered it at my booksellers, but he could not obtain it for me, though he tried for several weeks. I suppose you would consider it too long to find a place in KNOWLEDGE.—J. W. STANIFORTH.—[It is given in my book, "The Stars and the Universe" (Longmans), also in Mitchell's "Stellar and Planetary Worlds."—ED.]

POPULAR FALLACIES.

[203]—The letter of your correspondent "X." (No. 136, page 188) affords the opportunity of suggesting that KNOWLEDGE might advantageously publish an article or two on "Popular Fallacies," with a view to the destruction of some of the many time-honoured but baseless notions which, handed down from generation to generation, even amongst the fairly educated classes, are accepted without investigation, and believed in with the firmest faith. As examples, I might point to the common practice of placing the poker cunningly over a dull or dying fire, with the idea that "it will draw it up," to the frequently expressed belief in the maleficent influence exercised by the moon when "lying on its back" (whatever that may mean) on the weather, &c.

In the meantime, however, let me advise "X.'s" housemaid to bestow as much care on her fire when the sun shines as when the day is dark and cold, and she need not then fear its going out. I am a pretty regular smoker myself, but I have never yet observed my cigar manifest a greater tendency to go out in sunshine than in cloud.

I suppose that the notion of the sun putting the fire out has arisen from the fact that the brilliancy of the sun's rays overpowers the feeble light of the fire. Let "X." light an ordinary spirit lamp in the sunshine. He will hardly see the flame at all. But if he will put his finger where the flame should be, he will, I think, be less inclined to believe that "bright sunlight interferes with ordinary combustion" than he now professes to be.—Yours, &c. WM. H. A.

GHOSTS.

[204]—Referring to your article on this subject (p. 183, Dec. 30, 1881) with an extract from Dr. Wilson's work, may I ask if all the ghosts, of whose appearance we have well authenticated accounts, are to be accounted for on the principle of the "physical derangement" of the person who saw them? How many such accounts we have of persons seeing an individual exactly at the time of his death, though the said individual was miles away at the time, and had not been thought about for years. Lord Brougham, for instance, made an agreement when a young man at college with a friend, that whichever died first should appear to the other, and very many years aftewards was startled by an apparition of his friend, just at the time of his death.—Yours, &c., EBENEZER KELBY.

[205]—Will Professor Andrew Wilson kindly explain how he reconciles the case of the War Office ghost with his disbelief in ghosts? In that case I think three friends in different parts of England saw the deceased officer at the corrected date of his death abroad. T. D.

COMMUNICATION WITH THE MOON.

[206]—I have seen it stated that a French philosopher suggested the possibility of communicating with the inhabitants of the moon, if such there were, by means of mathematical figures, constructed on a very large scale, in some such region as the Desert of Sahara, Siberia, or the like. Can any readers of KNOWLEDGE give me information as to this idea, and the reasoning by which it was supported? X. DAVINE.

[Probably some reader may be able to answer "X. Davine's" question. I have not myself seen the passage he refers to, but believe it was a German, not a French writer, who suggested the idea. [I find a passage in an old number of the *Magasin Pittoresque*, running as follows:—"If the Selenites are beings endowed with reason, men may establish a system of intellectual correspondence with them. In fact, if the arts and sciences are as much advanced among them as they are amongst us, they must often have gazed on the immense globe which shines in their firmament, having an apparent surface sixteen times as great as that of the full moon as we see it. That globe is the earth on which we live. . . . . They can see our rivers, our great lakes, our chief cities. . . . . Thus, some gigantic telegraphic signal might be seen from the moon . . . What signal should be employed? to what language can we have recourse? All the signs by which we translate our thoughts are arbitrary and conventional. . . . . How are we to correspond with beings who, perhaps, have nothing in common with us but intelligence? Yet there is a method—the mathematical sciences furnish it." The writer goes on to show that if the Selenites have succeeded in constructing telescopes to see such signals as we might make, they must have also mastered the first principles of mathematics. Among the fundamental propositions is one due to Pythagoras, called the theorem of the square of the hypothenuse (*théorème du carré de l'hypothénuse*)" (viz., that the square on the longest side of a right-angled triangle is equal to the sum of the squares on the two shorter sides). He shows that if the Selenites are mathematical at all, they must have discovered this truth. "If, then," he proceeds, "we were to construct, as a German geometrician has suggested, a figure illustrating this theorem on a scale large enough to be seen by lunar telescopists, they would understand its meaning, and would probably reply with another figure, or some other signal. Then we should know that inhabitants existed in the moon, and that they were endowed with reason. Once this correspondence was started, who can say where it would end?" Our author says he sees the smile of incredulity on the lips of his readers. The idea must seem to them ridiculous, and none wonder that it had its origin in a German brain. "But do not judge too hastily:" the word impossible is hurtful to human pride; and if communication with beings in other worlds is not impossible, it must be conducted in this way, for there is no other.—ED.]

Queries.

[162]—INSTRUMENT FOR DESCRIBING ELLIPSE.—On page 160, No. 8, vol. 1, of KNOWLEDGE, Mr. Thomas Thorp gives a design in Fig. 2 of an instrument for describing ellipses. I shall esteem it a favour if he will describe the mechanism of it.—W. G. [It seemed to me clear that the stem of the instrument is intended to be held in a fixed slanting position, while the moveable branch is carried around, sliding at the same time backwards and forwards on the fixed stem. See Mr. Thorp's reply.—ED.]

[163]—SUN AT HIS NEAREST.—At what season of the year are the British Isles nearest the sun?—GEO. D. PATTERSON. [About Jan. 1, when the sun is nearest to the earth.—ED.]

[164]—NON-ACHROMATIC TELESCOPES.—Would a 3-in. OG non-achromatic telescope be of any use for a beginner, or do the tints very much affect the clearness of the object? Also is there any simple way of overcoming the difficulty?—G. W. L. [Such a telescope would be of very little use; a very small achromatic—one, perhaps, an inch in diameter—would show much more, and a great deal more conveniently. There is no way of correcting the difficulty.—ED.]

[165]—HORSERADISH.—What are the properties of horseradish, and how does it act on the gastric juice when partaken of?—T. HENEBMAN.

[166]—SHORTHAND WRITING.—Can you, or any of the readers of KNOWLEDGE, inform me which is the best system of shorthand? Has there been any system of late years which is thought an improvement on, or is more easily learnt, than Pitman's?—ENQUIRER. [There is no doubt whatever that Pitman's is the best, most easily learnt, and scientific; it is used by all the most skilful stenographers.—ED.]

[167]—STAR CATALOGUES.—Will you kindly favour me with the titles and dates of one or two star catalogues, both of ancient (comparatively) and modern dates. Is there one to be depended on of an earlier date than "Flamsteed's Catalogue of 1676?" I cannot meet with one.—ALFRED CHAMPNESS.—[Flamsteed's is the earliest reliable catalogue.—ED.]

[168]—THE MOON.—How is the absence of atmosphere and water in the moon accounted for? Lockyer, in his elementary astronomy, says that the dark portions of the moon are old ocean beds, but does not attempt to account for the disappearance of water.—G. P.—[The general belief is that, as the moon's mass has cooled, the water originally forming lunar seas has been withdrawn (soaked up as it were) into the moon's interior, the atmosphere partly following, partly entering into chemical combination with the substances forming the moon's surface. The moon probably shows the condition the earth will have attained in two or three hundred millions of years.—ED.]

[169]—LIGHT AND LANTERN.—Required names of good book or books for the study of "Light" with the use of the lantern.—J. W. STOW.

[170]—SCENTS.—What is a scent? That it is a substance, I suppose, will be generally admitted, for it is blown along by the wind. It has occurred to me that objects which emit scents do, for the most part, lose some of their weight or bulk—that is, the substance shrinks or becomes lighter, and if so it must throw off something in infinitesimally small particles or otherwise, which affect the sense of smell in animals and human kind. Please explain what is known about it. PRESTER W.

[171]—CHOANITES.—Are the flints, commonly called choanites, "silicified *sponges* from the chalk," or are they petrifactions of a higher kind of animal, an anemone (if, for instance, indeed a sponge can be called an animal at all, which seems to be doubtful with Professor Ansted)? Are sponges, similar in form and arrangement to the flints above named, found in a living state? I have never seen any; on the contrary, living specimens are similar to the sponge of commerce, and to a few only of the petrifactions; exhibiting perpendicular tubes only, not lateral or horizontal, as in choanites. Is the spiral worm round the body or cup of the choanites, a parasite or a part of the intestinal arrangement of the animal itself? I am aware that, if the worm is a parasite, such a conclusion is fatal to the idea of an anemone, for how could a parasite obtain admission into the bodily substance of an animal so organised? And some choanites are found without the worm. Nevertheless I incline to the belief that the choanite is the sea-anemone silicified. PRESTER W.

[172]—SHORTEST DAY.—Please tell me why the sun rises latest (8h. 9m.) on Dec. 29, and sets earliest (3h. 49m.) on Dec. 12, though the shortest day is Dec. 21; and also rises earliest (3h. 44m.) on June 16, and sets latest (8h. 19m.) on June 26, though the longest day is June 21.—J. E. H. P.—[These and similar anomalies arise from our using the mean sun, not the real sun, to measure civil time. At civil noon the sun's centre is not due south, except on certain days in each year, when the imaginary mean sun and the real sun are together.—ED.]

[173]—Will you tell me, does the electric circuit begin in a battery at the zinc or at the copper plate in the acidulated bath?—[Not known: any more than whether action of sun on a planet begins at the sun or at the planet.—ED.]

[174]—STUDENTS' PHILOSOPHICAL AND SCIENTIFIC SOCIETIES.—I should be much obliged if you could tell me whether there are in London any Students' Philosophical and Scientific Societies, as I should much like to belong to one.—HORACE DAVIES.

[175]—HELIX.—In a recent lecture, Mr. Spottiswoode said, "The curve fulfilling these conditions will be a helix, whose *pitch* is half a right-angle." What measurement is this? In the most ordinary form of helix, viz., a screw, the *pitch* is merely a distance, or, say, a straight line, the space between two threads, measured from apex to apex, parallel to the axis. What is this angle?—COGITO.—[Mr. Spottiswoode was, of course, quite right. The pitch of a helix is an angle, not a distance. It is the complement of the angle at which the curve is inclined to the axis.—ED.]

[176]—BRICK CLAY.—Are the blue streaks in brick clay caused by dissolved sea-weed?—M. WEBB.

[177]—THE NAUTILUS.—Is it known for what use is the gut running through the chambers of the nautilus?—M. WEBB.

[178]—SATELLITES OF JUPITER.—I have seen two of the satellites of Jupiter with my naked eyes; is that an unusual thing?—M. WEBB.—[Exceedingly.—ED.]

[179]—FAURE ACCUMULATOR.—Will you kindly tell me if the *quantity* of electricity that a Faure Accumulator can contain depends upon the size or upon the number of the lead plates? Would one or two large plates contain as much as half-a-dozen small ones?—R. P. H.

[180]—THE POLE.—Kindly state how Arctic explorers will be able to tell exactly when they reach the North Pole. How will they know that they are not some miles wide of the mark?—PILULA. [Astronomical observations showing any given star at same height all round the heavens would give their place near enough.—ED.]

[181]—BEES.—I want to keep bees, and want a list of the best honey-producing flowers to cultivate in my garden. Will you be good enough to tell me where to get this knowledge?—PILULA.

[182]—EVOLUTION.—Will Dr. Andrew Wilson or Mr. Grant Allen kindly let me know what is the probable origin of Evergreens? I have tried hard to think it out for myself, but am unable to see what advantage it is to the plant, unless it be that it gives it an opportunity of spreading while the other plants are at rest, or that the exposure to the cold makes it more hardy, and, therefore, better adapted to carry on the struggle for existence.—PLESIOSAURUS.

[183]—EFFECTS OF EFFLUVIA ON HEALTH.—In our dye works here we use a great quantity of bullock's blood, which comes to us in a highly putrid state; in fact, the smell of it at a distance of several yards from the tanks is overpowering to strangers, and causes one to cough. Is it healthy to work amongst it? This is a question which has often occurred to me, and, naturally, I look to the health of those who work during a whole day with this effluvium continually; but, instead of finding them weakly, and exhibiting symptoms of pulmonary disease, I am astonished to see them grow fat and ruddy, and in many cases persons of known weakly constitutions have improved much in health by being put to work in the dye-house.—J. MACCINSLANE.

[184]—ALMANACS AND CELESTIAL MAPS.—Could you kindly inform me through your "Correspondence Column"—1. Where can I get "Dietrichsen's and Hannay's Astronomical Almanac" [no longer published.—ED.] and "The Nautical Almanac" for 1882, and the price of each (where are they published)? [Murray, price 2s. 6d.—ED.]; 2. The publishers of "Middleton's Celestial Atlas" and of "Gall's Atlas," and the price of each?—W. HARDY.

HE that wyll wryte well in any tongue, muste folowe thys councel of Aristotle, to speake as the common people do, to thinke as wise men do: and so shoulde euery man understande hym, and the iudgment of wyse men alowe hym. Many English writers haue not done so, but vsinge straunge wordes as latin, french, and Italian, do make all thinges darke and harde.—ROGER ASCHAM, 1545.

Replies to Queries.

[55]—GREEK FUTURES.—Mr. Ernest J. Wernham points out, in answer to "Castor and Pollux," that the rules for forming Greek futures are given in Matthiæ's Greek Grammar. He kindly makes an extract, but we cannot afford space for grammar rules occupying so much space.—ED.

[105]—PRESERVATION OF FUNGI.—"J. S." may preserve fungi in the following manner:—Take 2 oz. of sul.copper, or blue vitriol, and reduce it to powder, and pour upon it a pint of boiling water, and when cold add half-a-pint of spirits-of-wine; cork it well, and call it "the pickle." To 8 pints of water add $1\frac{1}{2}$ pints of spirits of wine, and call it "the liquor." Be provided with a number of wide-mouthed bottles of different sizes, all well-fitted with corks. The fungi should be left on the table as long as possible, to allow the moisture to evaporate; they should then be placed in the pickle for three hours, or longer if necessary; then place them in the bottles intended for their reception, and fill with the liquor. They should then be well corked and sealed, and arranged in order, with their names in front of the bottles.—J. G. PATTERSON.

[109]—RAILWAY COLLISIONS.—In the worst of these accidents the carriages are "telescoped," the seats of the compartments are driven together, causing fractured legs. To avoid this, draw the legs on to the seat and clasp the knees tightly with the arms. I know of cases where this has been successfully tried.—ENGINEER.

[116]—JOHN BULL.—Dr. Arbuthnot, in his ludicrous "History of Europe," represented an Englishman as a bluff, kind-hearted, bull-headed farmer. The character he called John Bull, and since it has become the national nickname.—J. J. W.

[123]—HIGH FERMENTATION.—In this process, which is the one used in the manufacture of English beer, the yeast rises to the surface of the fermenting liquid; hence its name. It is carried on by running a wort prepared from malt, or from a mixture of malt and other saccharine bodies, into a circular vat, partially covered on top to maintain proper temperature, and to prevent access of excess of air, at about 70° Fahr. Yeast is now added, and, having a plentiful supply of the food necessary for its growth, it soon converts some of the soluble albuminous matter of the malt into protein bodies of its own structure, while at the same time it converts a small portion of the glucose sugar present into the cellulose necessary for the construction of its cells. The mother yeast, while thus reproducing itself, not having sufficient oxygen for its action, decomposes the glucose, changing it into alcohol and carbonic acid gas according to the following equation:—

$$\underset{\text{Glucose.}}{C_6 H_{12} O_6} = \underset{\text{Alcohol.}}{2 C_2 H_5 HO} + \underset{\text{Carbonic Acid.}}{2 CO_2}$$

Small quantities of glycerine, succinic acid, and other products are also formed, and the action continues, unless checked by reduction of temperature, until the whole of the saccharine matter is decomposed. "Low Fermentation." This process commences at about 50° F., and the yeast on reproduction sinks to the bottom of the liquid. It is the process mainly employed on the Continent, and beers so fermented cannot be preserved in warm or temperate weather, unless surrounded by ice. The decomposition of the saccharine matter is the same as in "high fermentation." Stahl, Willis, Liebig, and other writers considered fermentation to be due to the oxidation of complex albuminoid bodies which decompose the saccharine matters present, in order to obtain sufficient oxygen for such oxidation; but, according to Pasteur and the greatest of other modern authorities, it is due to the action of living organisms which fall accidentally into the liquid from the air, or are purposely added, as in the case of adding yeast to wort. The yeast organism is the one which sets up alcoholic fermentation, and the reason why it is artificially supplied to wort is to supersede other fermentations that may be induced by germs of another nature gaining access to the liquid by means of air. The other most common ferment germs are those which set up the lactic and butyric fermentations.—E. M. D.—[Answered in the same sense by T. G. Browne.]

[133]—I observe a correspondent inquiring where he can find the Rev. W. H. Dollinger's papers. If he will read the index to the recent volumes of the *Popular Science Review*, he will find papers by that gentleman on microscopical subjects therein noted. The "Proceedings of the Royal Society" should also give him Mr. Dollinger's papers.—ANDREW WILSON.

[137]—HUMBLE BEES.—I beg leave to state that in December, 1880, at the request of some persons in New Zealand, I shipped eighteen fertile humble bees (*Bombus Lucorum*) by the *John Elder*, one of the Orient line of steamers, to Melbourne, Messrs. F. Green & Co., the owners, kindly instructing their officers to take all needful care of them, and see to their re-shipment in the *Arawata* to New Zealand. From a newspaper (the *Timaru Herald*) sent me, I learn that two out of the eighteen arrived alive, and when released, on the morning of Feb. 5, 1881, flew away briskly to seek, as we may hope, nests in which to multiply and increase, and thus bring about that long-desired work, the fructification of red clover. I may mention that these bees were searched for and sent to me by Mr. Duncan Keir, an intelligent nurseryman at Paisley. I packed them in small boxes, and supplied a little sugar in case they might require food in a warmer latitude; but the great point is if possible to keep them dormant during the voyage, and for this purpose I placed the package under the care of the butcher, to put in his ice-house. It is well known that none other than fertile mothers hybernate. Three other humble queens were sent Jan. 20 of last year by the steamship *Norfolk*, which sailed direct to New Zealand, under precisely similar circumstances, and the supply was obtained from the same source, but no tidings concerning them have yet reached me. The experiment has not been repeated this winter, owing to the very scanty encouragement received of these and previous efforts. Your correspondent, no doubt, alludes to an attempt made some years since by the late Mr. Frank Buckland, in which I had the pleasure of giving some assistance.—ALFRED NEIGHBOUR.

[140]—ICE.—Ice contracts and expands as other solids do, by variation of temperature. This is best seen in travelling over the ice on a large lake (say Lake Winnipeg) in winter. If a cold "snap" suddenly comes on, and the temperature falls 15° or 20° in the course of the night, loud noises, like pistol shots or distant thunder, are heard; if encamped near the shore, on resuming the journey in the morning, large cracks, several feet wide, caused by contraction, will be met with, and are often difficult to cross; if the severe cold continues for a day or two, these cracks freeze up. Milder weather comes, the ice expands, and there being now more ice on the lake than before, ridges are forced up. Another period of great cold occurs, there are fresh cracks formed, with subsequent ridges when the temperature rises. These contractions and expansions, caused by changes of temperature, I believe to be an important element in the motion of glaciers.—J. RAE.

[146]—CHEMICAL.—(1). It is a property of red blood-corpuscles to absorb oxygen. This absorption changes them to bright red. Carbonous oxide can displace O, and thus acts as a poison. Nitrogen has no such power. (2). The value of carbon hydrates as food can only be estimated thus in compounds of the same class; *e.g.*, we cannot compare ether with its 22 per cent. of O, with sugar having 51 per cent., the fact being that sugar, &c., are more assimilable than many substances having less oxygen.—C. T. B.

[146]—CHEMICAL.—E. M. must remember that the "Carbohydrates" are just those bodies which contain the hydrogen and oxygen in the proportion of water—that is, in a saturated condition—none free. Cane sugar, for example, is $C_{12} H_{22} O_{11}$, in which "E. M." will find eleven times H_2O, leaving 12C to answer his purpose.—LEWIS ARUNDEL.

[147]—B.Sc. EXAMINATION, EDINBURGH UNIVERSITY.—In reply to "A Philosophical Brushmaker," I may say that attendance upon the University classes necessary for the above degree (or upon classes conducted elsewhere, but recognised by the University Court), is imperative upon candidates. Full information may be obtained in the University *Calendar*.—ANDREW WILSON.

[152]—NICKEL PLATING.—In the first part of "Amateur Work" there is a recipe for this:—Make a bath of tin tartar, boil with some grain tin, and after cooling, throw in some red-hot nickel oxide. When the solution turns green, dip in your articles (brass or copper, of course, and perfectly clean) and in five minutes they will be coated. Though I understand chemistry well, I should be glad to know what tin tartar is.—LEWIS ARUNDEL.

[153]—DOUBTFUL ORGANISMS.—The creatures you allude to are called Protista; they partake equally of the nature of the lowest animals and the lowest plants.—LEWIS ARUNDEL.

[153]—There are certain living beings—if these are what "H. J. C. W." means by "doubtful organisms"—which present in themselves a singular combination of the characters of animals and plants. Such are the Monads. If "H. J. C. W." will read Huxley's lecture on "The Border Territory between the Animal and Plant Kingdoms" (to be published in his new volume of essays, "Science and Culture"—Macmillan), he will find therein a full statement of the likenesses which exist both to animals and plants in certain of the lower forms of life—ANDREW WILSON.

[154]—Frogs are excluded from the class Reptilia (as any ordinary primer of zoology would have shown "H. J. C. W.") because they possess gills in early life. No *reptile* (*i.e.*, tortoises, snakes, lizards, and crocodiles) possesses gills at any period of existence, but breathes from the first by lungs. Frogs, newts, toads, &c., are *amphibians*. They all begin life with gills, and afterwards develop

lungs; the gills either disappearing when lungs appear (as in the frogs, toads, and newts) or persisting (as in proteus, the axolotl, &c). Frogs and other amphibians further differ from reptiles in undergoing a *metamorphosis* during their development, and in the want of scales or bony plates on the skin.—Andrew Wilson. I find what "I had always supposed" (viz., that the class Reptilia included the Amphibia, and therefore the Batrachians) is an error. (It was not given, be it noticed, as a reply.) I know, of course, that some naturalists regard the Amphibia and Reptilia as distinct classes; and I see that in Prof. Newton's primer they are so given. I know also that Linnæus's mistake in classing the abranchiate reptiles as amphibia had been corrected. But there must have been some change in classification of late, if that I mentioned has been definitely rejected; for I find Prof. Owen, in "Brande's Dictionary of Science" (1867), writing (1) under head "Reptilia," that those which retain gills during a part of their existence are called Batrachians; (2), under head "Amphibia," calling those which retain their gills during the whole of their existence peribranchiate reptiles; and (3), defining Batrachia as "an order of Reptilia, including all reptiles which, like frogs and toads, have naked skins and external branchiæ in the early stage of existence."—Ed.]

[155]—I have known of a tortoise living ten years; but I should say the duration of life is longer, especially in the larger species. I should imagine they do not remain "under the earth," in ordinary circumstances, longer than the winter or cold season.—Andrew Wilson.

[157]—Aluminium.—Any good Chemistry, as Fownes', or Watts', or Miller's, gives a full account. It is too much to expect in these pages. Spell aluminium with two i's, and emphasize the "min."—Lewis Arundel.

[170].—"E. M.," in Knowledge, for Jan. 6, thinks that the existence of parasites in meat is an argument for vegetarianism. Permit me to remind "E. M." that parasitically-infested meat is not normal meat. If "E. M.," as a vegetarian, acquired a tapeworm guest from a lettuce, he would not abjure his vegetable food, I suppose, because of his liability to infestation therefrom, any more than he would cease drinking water because some waters contain young flukes. The lesson taught by my paper is not the abjuration of flesh as an article of food, but the careful selection of healthy flesh; and, I will add, of healthy and normal food of all kinds—vegetables and drinks included. The *pros* and *cons* of vegetarianism lie quite outside your correspondent's letter and my article.—Andrew Wilson.

[180]—"An Amateur" inquires the "use" of bugs, fleas, flies, and other "apparently useless insects." He should first learn that the word "use" has a sole meaning and application (as he employs it) to human life. He should learn next that each animal and plant exists for its own sake, and independently of any "use" which men may conceive it adapted to serve. Thus "An Amateur" might as legitimately ask the use of seals, and might receive the reply "to furnish sealskin jackets for ladies;" and humming-birds might similarly be regarded as existing for the purpose of decorating ladies' hats. In a word, science can take no heed of such a question as that put by "An Amateur." Like the Smith of Perth, each animal and plant fights "for its own hand," and lives and exists independently of all human ideas of use and no use.—Andrew Wilson.

Answers to Correspondents.

*** *All communications for the Editor requiring early attention should reach the Office on or before the Saturday preceding the current issue of* Knowledge, *the increasing circulation of which compels us to go to press early in the week.*

Hints to Correspondents. 1. *No questions asking for scientific information can be answered through the post.* 2. *Letters sent to the Editor for correspondents cannot be forwarded; nor can the names or addresses of correspondents be given in answer to private inquiries.* 3. *No queries or replies savouring of the nature of advertisements can be inserted.* 4. *Letters, queries, and replies are inserted, unless contrary to Rule 3, free of charge.* 5. *Correspondents should write on one side only of the paper, and put drawings on a separate leaf.* 6. *Each letter, query, or reply should have a title, and in replying to letters or queries, reference should be made to the number of letter or query, the page on which it appears, and its title.*

W. A. C. Mr. Hutton points out that if we cease to eat meat we shall destroy animals more certainly and more cruelly than at present, by leaving them no vegetable food; and he is an anti-vivisectionist of the most pronounced type. We had better not insert your objections to the toad-pressing story, lest boys should try the very experiments you say the story suggests—which certainly would not have occurred to us.—A. St. Clair. We know of no French educational paper whose editor would take English stamps in payment.—W. H. H. Soames. Certainly the moon occults the planets. Such phenomena are announced in the "Nautical Almanac." The outer satellite of Jupiter often passes considerably above or below the planet. In an article we wrote in 1867 for the *Popular Science Review*, we discussed the question fully.—Fred. Blackett. Our price is small, our modesty great; beyond sending specimen copy, we would not force ourselves on your society's notice.—W. R. Bland. The proprietors consider that such notice should appear as an advertisement.—W. H. Pertwee. Arctic explorers want, I suppose, to find what there is at the Pole—land or water, greater cold or less cold, and so forth. As to the other question, evolutionists may or may not believe in the statement you refer to, or some of them may and some may not; it is no business of ours. If you had asked me what I believed on that or kindred subjects, I might have replied that it was no business of yours, which, though it would have sounded abrupt, implies something which may be usefully remembered. A man says he believes a certain scientific fact, and he is asked—without rhyme or reason—whether, so believing, he can believe something else which the inquirer says he and others regard as a truth of religion. What sensible answer can he make to a question so absurd, without being somewhat plain-spoken? To such a question, as to my belief, asked me in public at New York in 1874, I answered, "My belief is that there is a deal of nonsense in the world," and I believe still that I could not have answered the question more precisely, unless for "nonsense" I had used a stronger expression.—Frank R. Thomas. It certainly appears that "the mind is only partially active, and that the sounder part of it—the reason—is dormant when we are dreaming." You ask, "to what does this point?" We are not quite sure that we understand you. It seems to us what might very naturally be expected.—E. H. Thanks for pointing out that Newton, in his work on fluxions, described the Witch of Agnesi, calling it a "conchoidal." We agree with you that the curve cannot properly be called a quadratrix.—F. Wilson. The laws of perspective are purely geometrical, and the slightest acquaintance with geometry would show you that perspective cannot possibly account for the disappearance of a hull of a ship before its masts. The hull is unseen because there is something in the way, namely, the rotundity of the earth.—Wm. H. Allen. Thanks for extract on actinium. Very little is known yet respecting the properties of the new element. Several have been discovered since spectroscopic analysis was invented.—Desideratum. The delay you refer to is explained by the circumstance that we receive so many letters like the one signed "Desideratum."—A. Champness. We cannot reply by letter to queries, even when a stamped and directed envelope is sent.—An Inquirer. If we put in your query about cats falling on their feet, we shall have all our school-boy readers trying experiments—applying the inductive method, with projective illustrations.—Æolus. No; the Americans do *not* (we believe) "fire their storms at us out of a gun." Ships reaching America give news of storms travelling eastward across the Atlantic, which had been already traced across part of the American continent. The prediction is based on the belief that such storms will travel farther east before they die out. You are more correct as to solar heat being the cause of storms; but they may arise without the sun being "in some vagaries." The heat he pours on the earth is tolerably constant.—One who wants Knowledge asks for information respecting the manufacture of Portland cement, or titles of books relating thereto.—E. S. We have not room for your letter on primary colours. If a definition of primary colours were given, which should carefully distinguish what is physical in the matter from what is physiological, we might get more satisfactory statements. As to your questions relating to religion, I reply that to exclude religion, in its wider and nobler sense, from our columns would be to exclude science. But dogmatic religion we cannot away with. One might as reasonably speak of a Mussulman spectroscope, or of Brahminical hydrogen, as of Christian science. Science is neither Christian nor un-Christian, but extra-Christian. We do not want such matters to be treated in a calm, philosophic spirit, but simply left altogether untouched. They have no proper place here. As a mere matter of detail, we may note that they never are treated in a calm, philosophic spirit, perhaps because no calmly philosophic person would be so unwise as to treat of them at all.—Daleth. About comets' nuclei later.—Mercury. For names of Mr. Wallace's books apply to Messrs. Macmillan. They will well repay study. Read also Darwin's "Origin of Species" and "Descent of Man."—Mabel. W. Laing recommends "E. H." to read Dr. J. Reade's "Experimental Outlines for a New Theory of Colours, Light, and Vision," 1818, and W. Crum's "Experimental Inquiry into the Number and Properties of the Primary Colours," 1830 (books entirely out of date); also, if he reads Dutch, N. Folmer's "Alpha van het Alphabet der Kleuren,"

Groningen, 1875. Prof. Ogden Rood's book on "Modern Chromatics" (Kegan, Paul, & Co., price 5s.) is far better worth studying.—Ernest J. Wernham. There are many proofs, and as many disproofs, of the theory that character can be learned from the handwriting. The method has no scientific interest.—B. Donbavand. We insert one of your letters, but your last, on the colours of animals, is too long, and we have not time to abridge it. You take three pages of MS. for initial sarcasms against a valued correspondent, who has really shown a very kind desire to explain matters; another to show that "a tank naturalist must be contemptible in all the ins and outs of him"; then *possibly* you come to business. We cannot say—life is too short to get through more of such letters. Stay. We will not at once consign your letter to W.-P. basket, but only the first four pages, keeping the rest for another trial when we have refreshed ourselves with matters more *ad rem*. Understand, you are as free to say in so many words that Prof. Wilson seems angry, as he was to say that a remark of yours seemed to him silly. But we cannot find space for elaborate sarcasms. We want to get at facts, and invite our correspondents to join in the work, not to try who can say the smartest things.—M. E. Pendred. It would take much more time than we can afford to go through your letter of some score of closely-written pages, scoring out all that is not to the point; and we can only insert what is to the point.—J. H. Symington. We misunderstood you about the Collie. Excuse us.—H. A. Bulley. Lunar influence on weather is conceivable, though never shown to be really effective; but planetary influences, or the absurdities called Astrometeorology, we cannot even discuss here. We must draw the line somewhere.—Zares. Dr. Ball's experiment, of dropping one of his namesakes on the floor of a railway carriage, has altogether the advantage over yours of firing a bolt from a catapult. How did you test the horizontality of the bolt's flight, and how eliminate effects of atmospheric resistance? We are sure, when we drop a ball in a railway carriage, that its only initial motion is horizontal, and that air-resistances are the same as on a body let fall from rest.—Pilula. You are evidently not aware that advertisements of the kind cannot be refused.—C. J. Brown. "Assuming the earth's diameter to be 8,000 miles," (or 8, or 8 millions, if preferred,) the pressure is greatest at the centre.—R. W. If the solid be transparent, farther edges should be shown like nearer. The cases are different. Science finds no evidence of man's having become degraded. It does find evidence that some creatures have.—Jas. Elias. Your letter is too long for insertion; but your idea is well worth considering, that, in some cases, frog's spawn might have been deposited in some fissure where running water passed, and so a tadpole develope there, which, adapting its mode of living to its narrow surroundings, would become a frog, and might live comfortably enough. A quarryman, as you say, would not be apt to notice the fissure, and the stone would naturally split in a plane running through the cavity.—H. D. Kingdon. Quite impossible to appoint meeting.—Daniel Jackson. Gambling on the Stock Exchange is undoubtedly no better than gambling elsewhere. The two books you refer to are the "Expanse of Heaven" and "Infinities Around Us," published by Chatto.—J. K. Mellor. Thanks, but fear we can find no space for biographies at present.—Jas. Douglas. The change of eccentricity of earth's orbit, and not the precessional reeling *per se*, causes changes of climate you mention.—E. W. Will try to find room for your letter in full. In the meantime, we may note that the applications of the differential calculus to Physics are so much more difficult than those to ordinary mathematical problems, that the course pursued in books seems natural enough. Do you mean to say that, given the linear dimensions of a curve, and the length of the limits between which you integrate, you find difficulty in ascertaining the number of square inches, feet, or miles in the area? I cannot see where the difficulty comes in. In a series of papers I once wrote on the calculus for the *English Mechanic*, I showed how a number of problems in everyday life may be dealt with by means of it. The translation of the symbols into concrete quantities should be one of the first, not one of the last, things to which the student's attention is directed.—A New Subscriber. We shall presently give a description of some comets of the present century; but we must refer you to treatises on comets for an account of all, or even of the principal comets.—Pleiades. (1). It is better for querists to give their real name (not for publication), but not essential. (2). We may presently give weekly notices of astronomical phenomena. (3). No. 1 of Knowledge has now been reprinted for the third time; the proprietors are not likely to reprint again, so that if you wish for No. 1 you should apply in good time. (4.) "Abstract" in headings of letters means that we have had to abridge them.—One Wishful to Learn. Heat is not generated at the sun by his attractive action on planets. When a cord is pulled, or a bar either pulled or pushed, the heat is generated by the impressed forces, *i.e.*, comes from without.—E. B. T. Thanks, but the idea is much older than Babbage's time, or R. F. Gardiner than Rev. Mr. Hitchcock's. W. Hardy. "Dietrichsen's Almanac," no longer published; "Nautical Almanac" for 1882, 1883, 1884, and, probably, 1885, can be obtained of Murray, price 2s. 6d. each.—Florence E. Boyce. Pray excuse us for leaving your interesting communication so long unnoticed. It got lost in our troubled sea of letters, MS., &c. The proof for sum of squares is very neat.—Arthur Vizard. Your remark answered by last number. But the analysis of chess openings need not be hurried. We must not make a toil of pleasure. Chess and whist are our scientific recreations.—F. H. S. Have been obliged to limit answers to magic square questions.—J. Russell. How far should we be advanced if we agreed to regard comets as having "the same place in the inanimate solar system as is occupied by jelly-fish in the animal world?" J. Sharp. You article somewhat too diffuse, and much of its contents generally known.—Ursula. Reply to query about rainbow on page 212, second column, lines 11, &c.—Lewis Arundel. Your remarks about my replies to 153, &c., remind me somewhat of Molière's "*Nous avons changé tout cela.*" I was not answering "according to Cuvier," but according to Owen and other more recent authorities. In explanation of a certain property of worms, I referred to their being articulated. Do you conceive that when worms were formed into a sub-kingdom Vermes, they ceased to be articulated, or that the chain of ganglia I mentioned ceased to be present in their annelidan bodies? They have no articulated limbs, but they are articulated animals whether classed as Arthropoda or Vermes, whether called Abranchiate Annelidans, or common earthworms. Equally, classing toads and frogs as Batrachian Amphibia has not made them change their reptilian habits.—D. C. Jones. Recently answered.—A. T. C. Pardon me for repeating that there must have been something near or far off between the sun and the holes. In saying that the sun's light ceased to pass through the holes (there being nothing between the holes and the opposite wall at the time), you in effect say that something intercepted the sun's rays. If you insist on it that there was nothing between the sun and the blind to cast a shadow—cloud, flight of birds, or of dust, or of leaves, or flying object of some sort, near or far off—all one can say is that nothing can explain what you saw. No shadow ever yet existed without something to cast it.—Major. The illusion about letter S is well known to proof correctors. The lower half looks very much larger upside down.—C. F. B. The reason why the sun and moon appear larger when near the horizon is, I take it, because the heavens appear to form a flattened not a spherical dome, and the sun or moon subtending really the same angle when near the horizon (appreciably) seem to be nearer than the sky beyond. The eye seems to misinterpret what it actually sees, making the sun seem nearer instead of the sky seeming farther.

Notes on Art and Science.

Mr. J. H. A. Macdonald, Q.C., the late Solicitor-General for Scotland, has constructed an "electric holophote course indicator," which he has lately been exhibiting in Edinburgh. An electric light with a reflector is placed in a prominent position on the deck of the vessel, and is controlled by the movement of the helm. The direction of the powerful beam of light indicates the course of the ship, and at the same time shows whether or not the sea is clear over a large area. The model is to be shown at the Crystal Palace Exhibition.

Mr. Jacob Reese, in a paper read before the Engineers' Society of Western Pennsylvania, remarks: "The great want of the present age is a process by which the static caloric of carbon may be set free by non-luminous combustion, or, in other words, a process by which coal or oil may be oxidised at a low degree within an insulated vessel." This cannot be too prominently brought forward. "If it can be accomplished," as Mr. Reese says, "we would be able to produce from twelve to fifteen million foot pounds of electricity from one pound of petroleum, or from ten to twelve million foot pounds of energy from one pound of good coal."—*Athenæum.*

Are Bees a Nuisance?—An unusual case is being tried in the Cumberland County (Penn.) Court this week, that of testing by a jury whether the keeping of a large number of bees in a town or borough is a public nuisance or not. The case is from West Fair-

[illegible], a small town on the opposite side of the river from Harrisburg. Two citizens had about 130 skeps of bees, and as the summer was scarce of material such as the bees feed upon, they came in large numbers into the houses, stores, grape arbours, and wherever there was anything for them to feed upon. In one instance they swarmed in a neighbour's kitchen, and were there for days, he not being able to hive them, the queen being killed. They were especially bad about canning and preserving time, compelling the housewife to do her preserving in the evening, and in one instance the wife had to climb in and out of the window for days, not daring to open the doors, for the bees would go in by hundreds; persons were stung passing along the streets and highways; entire houses became infested with bees, so much so that the inmates could not retire to rest at night without being stung by the bees; trays of fruit put out for drying were entirely consumed. Indeed, a reign of terror was experienced for several months, until a committee of citizens agreed to abate the nuisance, and, after several efforts, appealed to the court. The defence claimed that the raising and keeping of bees was an industry, and as such could not come under the head of a public nuisance, and that suit could not be brought nor damages recovered for the keeping of honey-bees. The attorneys on both sides presented the opinions of several judges and the law points in the case, after which the court decided the case should be tried, and the testimony was received. But one case seems to be on record in the State, and that was tried before Judge Pearson, in Dauphin County, years ago, in which the defendant was adjudged guilty, and had to pay a fine and abate the nuisance.—*Scientific American.*

THE TAPE-WORM. Most of my readers know that the domestic pig is subject to a disease known as "measles," in which the muscles are more or less filled with *cysts*, which render the pork unfit for food; but I think few are acquainted with its cause. Man, it is well known, is occasionally infested by a parasite—the so-called "tape-worm" (*Tænia solium*)—which may be described as having a tape-like body of varying length, with a differentiated "head," or *scolex*, at one extremity. This apparently single animal is in reality a colony of mothers and daughters, the scolex being the parent of all. This "head" is provided with a *rostellum*, or, as it might be called, proboscis, encircled by a crown of hooks, below which are the suckers; each segment added to the scolex is a complete individual containing a complicated and perfect reproductive system. The last segment—*proglottides*—which are filled with eggs, break off at intervals, and either the eggs are set free within the intestine of their host, when they are passed out with the fæces, or the segments themselves are evacuated. The tape worm feeds on the juices of the bowel by absorbing the nutriment through its skin, and does not appear to seriously inconvenience its host in any way. In Abyssinia *tænia helminthosis* is constant and general; indeed the animal is there regarded as a sort of hygienic agent, and cultivated rather than discouraged, yet the people are healthy; certain it is also that wild animals, almost without exception, harbour at least one species of tape-worm as a natural condition. But what has this to do with "measles"? Now to the point. Let us suppose one of the before-mentioned eggs taken into the stomach of a pig, either by its eating the excrement of a person affected or through the water or air; here it hatches, not into a tape-worm, but into an animal of oval form, transparent, contractile, in the middle of which are six stylets arranged in pairs; with these it cuts its way through the tissues until the muscles are reached, when, having arrived at its destination, it stops burrowing and surrounds itself with a sheath. Here the stylets atrophy, a new and quite different crown of hooks is produced, and the parasite becomes a *cysticercus*, or vesicular worm, the cyst being about the size of a hazel nut. This constitutes "measles"; the exhaustion or even death attendant on the disease is caused by the scores, hundreds, or even thousands of animals boring through the tissues. Once encysted there is no further suffering or danger. The cysticercus remains encysted for months or years, or until the piece of flesh enveloping it *is introduced into the stomach of man*, in which case it instantly quits its torpid condition, leaves its sheath, makes its way to the intestine, where, attaching itself by its suckers and hooks, it grows—or rather reproduces—so rapidly, that in a few weeks a tape-worm of several yards in length is formed, which reproduces eggs, and so *ad infinitum*—from pig to man, from man to pig. Should the eggs be introduced into man himself or animal other than the hog, the cysticercus penetrates the tissues in the same manner, but it is "not at home," and instead of resting in the muscles, it makes its way to other organs, such as the brain, heart, or eye, where its presence has caused in man several instances of insanity and death. Should a piece of meat containing a vesicular worm be eaten by a pig or animal other than man, a tænia is developed, but it also is "not at home," and does not attain its full development. Both eggs and cysticerci are killed by a temperature of 200° Fah., so there is no danger in eating well-cooked pork, even if it contains cysticerci. To prevent hogs contracting "measles," it is only necessary to prevent them having access, either through their food or water, to the secretions of man, and they will not suffer. Throughout the genius *Tænia* we find this dual life; for instance, the cat has a tape-worm, the cysticercus of which she gets from the mouse; and the dog one which he obtains from the sheep.—*Scientific American.*

Our Mathematical Column.

ALGEBRAICAL EQUATION. — In reply to "E. H.," there is no method of solving the equation

$$\sqrt{100-y^2} + \sqrt{8y-64} - 6 = 0$$

other than by reducing it to a cubic, and applying one of the approximate methods to the resulting equation. We get by squaring both sides, &c.,

$$y^4 + 16y^3 - 192y^2 - 3200y + 25600 = 0$$
$$\text{or } y^3(y-8) + 24y(y-8) - 3200(y-8) = 0$$
$$\text{Whence } y = 8, \text{ and } y^3 + 24y - 3200 = 0$$

As "E. H." points out, 9·74 is an approximate root of this equation. The other roots are imaginary.—ED.

[12]—"F. B.," admitting that if $\Sigma_n = \frac{2n\,(2n-1)\,\ldots\,(2n+1)}{1\,.\,2\,.\,3\,\ldots\,n}$,

$$\Sigma_n + 1 = \Sigma_n \left[\frac{2(2n+1)}{n+1}\right]$$

asks whether we might not at once conclude from this that if

$$S_n = 1 + n^2 + \left[\frac{n(n-1)}{1.2}\right]^2 + \&c. + n^2 + 1,$$

and it can be shown that

$$S_n + 1 = S_n \left[\frac{2(n+1)}{n+1}\right]$$

then $\Sigma_n = S_n$.

We might, if we could prove the relation in question, but we must do this first. How does "F. B." propose to show that when n becomes $n+1$ in the expression for S_n, the result, or $S_n + 1$, is equal to S_n multiplied by $\frac{2(2n+1)}{n+1}$? It is easy to infer this after showing that $\Sigma_n = S_n$, but as a step towards proving this it is not at all easy.—ED.

SQUARE AND CUBE NUMBERS.—J. A. Miles sends the following, respecting some curious properties of square and cube numbers:—

The first term of an arithmetical progression of n terms having a common difference d, and whose sum is n^a is equal to

$$n^{a-1} + \frac{d}{2}(1-n).—$$

If $S = n^2$, the first term is $= n + \frac{d}{2}(1-n)$—

Every square n^2 is the sum of an arithmetical progression of n terms, the first term of which is unity, and the difference 2.

Every square n^2 is the sum of an arithmetical progression of n terms, the first term of which is $\frac{n+1}{2}$ and the common difference 1.

If $S = n^3$, the first term is

$$= n^2 + \frac{d}{2}(1-n)$$

Every cube n^3 is the sum of an arithmetical progression of n terms, the first term of which is unity, and the common difference $2.(n+1)$.

Every cube n^3 is the sum of an arithmetical progression of n terms, the first term of which is the root n, and the difference $2n$.

Every cube n^3 is the sum of an arithmetical progression of n terms, the first term of which is $n^2 - n + 1$, and the difference 2.

Every cube n^3 is the sum of an arithmetical progression of n terms, the first term of which is a triangular number $\frac{n^2+n}{2}$, and the difference $= n$.

Every cube n^3 above 1 is the sum of an arithmetical progression of n terms, the first term of which is $(n-2)^2$, and the difference $= 8$.

Our Whist Column.

By "Five of Clubs."

THE LEAD (*Continued*)

LEADING AN ACE (PLAIN SUITS).

WE omitted to mention one case—quite exceptional—when an Ace is led from Ace, King, and others. This is dealt with in the next section.

LEADING A KING (PLAIN SUITS).

From a long suit, or from a suit of three at least, King is only lead under two conditions, viz:—

(1) From Ace, King, and others.

(2) From King, Queen, and others.

In the case of a forced lead from King and one other, the King is always led.

Here we may answer a question often asked by young players when corrected for leading Ace from Ace, King, and others. What difference can it make, they ask, seeing that both cards are of equal strength? To this they add sometimes that as it is a recognised rule to lead the highest of a sequence (following suit with the lowest) there seems a disadvantage in making what appears like an unnecessary exception.

So far as making the strong cards of your suit is concerned, it is a matter of indifference whether you lead Ace or King. But if you follow the rule of leading King from King, Ace and others, you enable your partner to understand you better. You make your Ace leads more intelligible. If you led Ace uniformly from Ace King, an Ace lead might mean any one of three things; (i.) Ace four or more, (ii.) Ace, Queen, Knave, with or without others, and (iii.) Ace, King, and others. Your partner would often be in doubt which of the three you led from; whereas he can scarcely ever be in doubt which of the two ordinary cases is in question, even though you should be unable to follow up your lead.

As for the lead of King from Ace, King, and others being an exception to the useful general rule, "lead the highest from a sequence," the point is of no importance; for the exception is not one that can ever cause any confusion. In fact, it is becoming a recognised whist principle, that one of the great uses of general rules is that they afford an opportunity for giving your partner information, by departing from them in certain recognised cases. Of this we saw an example last week, in the lead second round from Ace, Queen, Knave, with or without others. After winning with the Ace, the Queen would be the proper lead, if we followed the general rule of leading the highest of a sequence. When the original suit is only of moderate length, (three or four) the Queen is led; but when the suit is of more than average length (five or more) we depart from the rule, and lead Knave second round. Thus, whether we follow the general rule, or depart from it, we give our partner information, yet without in any way affecting the strength of our suit.

There is one case, and one only, in which from Ace, King, and others, Ace should be led:—

If, before getting the lead, we have trumped in one suit, and should then lead King of another suit, our partner, if he had no cards in the suit (a contingency always to be considered) might see an opportunity of establishing a cross ruff or see-saw, by which, perhaps, four or five tricks might be made. He would, therefore, trump your King, considering that Ace might lie with fourth player, and lead the suit which you had trumped. To avoid this, you lead in such a case your Ace first, then your King.

When a King has been led first round, your partner knows from the way the cards fall whether the lead was from Ace, King, and others, or from King, Queen, and others. If you have led from King, Queen, and he does not himself hold Ace, one of the opponents will cover your King with Ace. If it passes, he knows you have Ace. But as a matter of fact, no one at the table remains in doubt about the meaning of a King lead, unless the King is trumped. For if the King makes, Ace follows at once if the lead was from Ace, King; and a small card if the lead was from King, Queen (in which case, of course—the first round having passed—the Ace is with partner).

Thus, just as when the Ace is led, in any case except that of a forced lead, the second round at once shows which of those two suits from which Ace should be led has actually been opened; so is it when a King is led—we can always tell from the second round at latest what suit has been led from—Ace, King, and others, or King, Queen, and others.

There is one exception—very seldom advisable in plain suits—viz., when the King is led from Ace, King, Knave, with or without others. Then leader sometimes changes suit, that he may be led up to and finesse with the Knave.

LEADING QUEEN (PLAIN SUITS).

The Queen is led from a long suit, or from a suit of three, at least, only in the following cases:

(1) Queen, Knave, ten, with or without small ones.

(2) Queen, Knave, and one small one.

In the case of a forced lead from Queen and one other, the Queen is always led.

After Queen from suit (1), Knave is led, if there is only one card or none below the ten; ten if there are more.

LEADING KNAVE (PLAIN SUITS).

The Knave is led from a long suit, or from a suit of three, at least, only in the following cases:

(1) Knave, ten, nine, with or without others.

(2) Knave, ten, and one small one.

In the case of a forced lead from Knave and one other, Knave is always led.

After Knave from suit (1), ten is led if there is only one card or none below the nine; nine if there are more.

LEADING TEN (PLAIN SUITS).

Ten is only led in the case of a forced lead from ten one other, or from ten two others.

LEADING A SMALL CARD (PLAIN SUITS).

A small card is led from Ace, two or three small ones (except by continental players, who lead Ace, as already mentioned, from Ace three small ones); from King and others, not including Queen; from Queen, Knave, and small ones (two or more), from Queen or Knave and small ones; from ten and small ones (three at least); from a suit of four small ones, when the lowest is played; and from a suit of fewer than four (a forced lead), when the highest is played. N.B.—From a suit of five cards or more, not headed by the Ace, the lowest but one is played. This lead is called the *Penultimate*.

Our Chess Column.

SOLUTIONS.

Problem No. 5, p. 171.

White.	Black.
1. K. to Q.2.	1. K. takes Kt. (best).
2. R. to Q.Kt 7.	2. Anything.
3. Mates acc.	

Problem No. 6, p. 190.

White.	Black.
1. Q. to K.Kt.5.	1. P. takes Q. (best).
2. Kt. to Q.Kt.5.	2. Anything.
3. B. or Kt. Mates acc.	

Problem No. 7, p. 190.

White.	Black.
1. B. to K.Kt.5.	1. P. takes B.
2. B. to K.B.3.	2. P. moves, or takes Kt.
3. B. or Kt. Mates acc.	

Problem No. 8, p. 190.

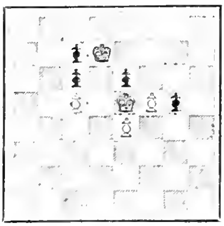
White.	Black.
1. B. to Q Kt.6.	1. P. takes B.
2. B. to Q.B.1.	2. P. moves.
3. B. Mates.	

TWO END GAMES.

THE following end-game has been sent to us by Mr. A. J. Maas. We leave it for awhile as an exercise to our readers. It occurred in actual play:—

Problem, No. 9.

Black.



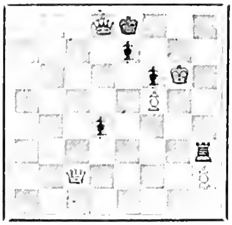
White.

White to play and win.

[T]he following end-game, Mr. Morphy (white), who had given [o]dds of a rook, drew the game.

PROBLEM No. 10.

BLACK.



WHITE.

White to play and draw.

GAME No. 6.

Played in the International Tournament at Berlin, on the 15th of September, 1881.

RUY LOPEZ.

WHITE.	BLACK.
Herr S. Winawer.	Dr. C. Schmid.
1. P. to K.4.	P. to K.4.
2. Kt. to K.B.3.	Kt. to Q.B.3.
3. B. to Q.Kt.5.	Kt. to K.B.3. (a)
4. Castles.	Kt. takes K.P.
5. Q. to K.2.	Kt. to Q 3 (b)
6. B. takes Kt.	Q.P. takes B.
7. Q. takes P.ch.	Q. to K.2.
8. Q. to Q.R 5 (c)	B. to K.3.
9. P. to Q.3.	Kt. to B.4. (d)
10. B to B.4. (e)	Castles (f)
11. Q takes R.P.	Q. to Q.Kt.5.
12. Kt. to K.5 (g)	Black resigns.

(*Sonntagsblatt*, Berlin.)

NOTES BY MEPHISTO.

(a) The defence of P. to Q.R.3. justly deserves preference to the move in the text. It leads to a safe development, for after 4. $\frac{\text{B. to R.4.}}{\text{Kt. to K.B.3.}}$ 5. $\frac{\text{Castles.}}{\text{Kt. takes P.}}$, this Knight, on being attacked, can retire to B.4., attacking the Bishop, and thereby gaining time.

(b) This move must be condemned on principle; it blocks Black's game entirely. Kt. to K.B.3. is the proper move.

(c) This move, which to some might look rather strange, was played to prevent Black from Castling on the Queen's side, as that was his intention; it will be seen that this, though only a small amount of forethought on the part of White, opened the door to a large amount of luck.

(d) As pointed out by the *Sonntagsblatt*, it would not have been feasable for Black to play 9.P. to K.Kt.3 with the intention of preparing to Castle on the King's side, as White would have replied with 10. $\frac{\text{B. to K.Kt.5}}{\text{P. to K.B.3.}}$ 11. $\frac{\text{Kt. to Q 4}}{\text{P. takes B.}}$ 12. $\frac{\text{Kt. takes B.}}{}$ and the Queen cannot retake on account of R. to K.sq. If in reply to 10 B. to K Kt.5. Black should play Q. to Q.2, then White continues with B. to B.6. Black's best course would have been to play P. to Q.R.3. and then Castle.

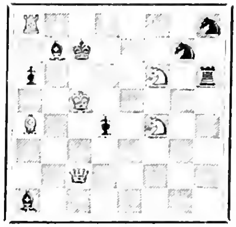
(e) Taking proper advantage of the position. The Knight back to Q 3. was about the only thing to save the Pawn, for if P. to Q.Kt.3., then the Pawns on the Queen's side would be weakened still more by Q. to R.4.

(f) The idea of Black in giving up the Rook's Pawn was to play Q. to Q.Kt.5. thinking that the Bishop would be compelled to retire, which Black would follow up by B. to Q.B.4. getting an attack. Checking with the Queen would obviously have been bad, *i.e.*, 11. $\frac{\text{Q takes R P.}}{\text{Q. to Q.Kt 5}}$ 12. $\frac{\text{Q to R.8.ch.}}{\text{K. to Q.2.}}$ 13. $\frac{\text{Kt. to K.5.ch.}}{\text{K. to K.sq.}}$ and White would lose a piece. Black, however, overlooked the force of 12. Kt to K 5. at once, which proved fatal. The whole idea of Black of exposing himself to the great danger of the position, for the purpose of extricating his game, was unsound. In chess, as in everything else, you must not indulge in such risky speculations; no move ought to be made but what a sound judgment would approve, as otherwise an unseen danger, or an overlooked resource, is almost sure to aid your adversary in demolishing your unsound speculation; though in many instances, it might be highly ingenious, and, in a few instances, might succeed, only as an exception to prove the rule.

(g) There is nothing to save Black's game. This is a remarkable collapse of a first-class player, of which there were a good many instances at Berlin. Dr. Schmid played Black, and we apprehend he must have also looked black, after White's 12th move—a rather remarkable instance of a Smith turning into a Blacksmith during a chess game of 12 moves.

Problem No. 11. By Herr Gunsberg.
(From the "Westminster Papers.")

BLACK.



WHITE.

White to play and mate in three moves.

Solutions of problems 6, 7, and 8, by G. W. Middleton, T. H. Symington, J. K. L., Gamma, Try Try Again, S.D.P., R.M., Afternoon, Etoniensis, correct. Of problem 8, by J. A. Miles, Arcadian, and See Saw, correct.

ALPHA BETA.—The solution you send of problem 5 is based on an incorrect idea as to the nature of chess problems. When mate is to be given in three moves, what is meant is, that against the best possible defence White is to give mate on his third move. You have shown how, after a certain move by White, and a certain reply by Black, White can mate. But after White, 1.B. to K.R.3., Black can play Knight to K.R.5.ch., and there is no mate either on the move, or in several more moves.

Contents of KNOWLEDGE *No.* 10.

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KNOWLEDGE
AN ILLUSTRATED
MAGAZINE OF SCIENCE
PLAINLY WORDED-EXACTLY DESCRIBED

LONDON: FRIDAY, JANUARY 20, 1882.

Contents of No. 12.

PROFESSOR YOUNG ON THE SUN.

PROFESSOR YOUNG, although no one would imagine it from the book before us, is one of those to whom we owe some of the most important of the discoveries which, during the last few years, have added so much to our knowledge of the sun. It is well that we should have treatises such as this from the workers themselves to whom our knowledge is due. For, though the most skilful observers are not always the ablest either in dealing with known facts and deducing sound theories from them, or in presenting them to the unscientific world, there is always in their writings a special value and something of the charm which we find in accounts of travel by those who have seen what they describe. Professor Young, however, possesses much more than mere observing skill. He is a sound and careful reasoner, and if there is a certain terseness and preciseness (more than mere precision) in his writing, which detracts a little from its literary charm, this has probably been rendered necessary by the limited nature of the space at his disposal, and in no sense deprives his work of its claim to be regarded as exceedingly well written. This treatise possesses also another quality, very important, we conceive, in astronomical writing.* Professor Young is a mathematician, and the formulas he gives, even when they are not of his own devising, are given with adequate understanding of their meaning and value, which, unfortunately, has not been the case with all the treatises on astronomy recently published.

Taking first Professor Young's treatment of the dimensions, mass, and power of the sun, we note that he adopts for the sun's distance an estimate very near that which the labours of his countryman, Professor Newcomb, seem to indicate as nearest to correctness, viz.: 92,885,000 miles, with a probable error of about a quarter per cent., or 225,000 miles. This would correspond with a mean equatorial horizontal solar parallax of 8″·80, or, in untechnical terms, the earth seen from the sun at his mean distance, would have an apparent maximum diameter of 17″·60 (about one 106th part of the apparent mean diameter of the moon or of the sun as we see these orbs from the earth.)* We may note in passing, that in comparing the methods of determining the sun's distance by observations of Mars on the stellar heavens and of Venus in transit, Professor Young omits to notice that whereas the full displacement of Mars, as seen from different parts of the earth, is available, the displacement of Venus on the sun's disc is only a part of her actual displacement, the sun himself being displaced (roughly, only about seven-tenths of the displacement of Venus is available). His account of the American photographic method of observing the transit of Venus is full of interest, though calculated to make Englishmen somewhat ashamed of the relatively unscientific method which (despite good advice to the contrary) was adopted at the English stations.

The mass of the sun, deduced from this estimate of the distance, is about 330,000 times that of the earth. Expressing this in tons, Professor Young uses what we take to be the erroneous American system of notation, saying that the earth's mass amounts to about two octillions of tons, where we should say two thousand trillions of tons. Respecting the sun's attractive power, as exerted on the earth, Professor Young quotes the following impressive illustration from a recent calculation by Mr. Warring:—"We may imagine gravitation to cease, and to be replaced by a material bond of some sort, holding the earth to the sun, and keeping her in her orbit. If, now, we suppose this connection to consist of a web of steel wires, each as large as the heaviest telegraph wires used (No. 1), then, to replace the sun's attraction, these wires would have to cover the whole sunward hemisphere of our globe about as thickly as blades of grass upon a lawn. It would require nine to each square inch."

It should be added, however, and still further enhances our conceptions of the sun's might, that were the connection between the sun and earth of this nature—that is, by steel wires—more than three hundred days would be required to make the pull of the sun felt at the earth. The action of gravity is exerted certainly in less than a second. In fact, the most careful observation of the planet's motions reveals no evidence that gravity takes even any appreciable time at all in traversing the spaces separating the various members of the solar system from each other. This apparent instantaneity of the action of gravity is one of the greatest mysteries known to science.

Sir John Herschel has well remarked that Giant Size and Giant Strength are little without Giant Benevolence. It is the light poured forth by the sun on the planets, the heat whereby he nourishes them, more than his vast bulk and his mighty mass which fit him to be the central ruler

"The Sun." By C. A. Young, Professor of Astronomy in the College of New Jersey. (Messrs. Appleton & Co., New York.)

* "To borrow the curious illustration of Professor Mendenhall," says Professor Young, "if we could imagine an infant with an arm long enough to enable him to touch the sun and burn himself, he would die of old age before the pain could reach him, since, according to the experiments of Helmholtz and others, a nervous shock is communicated only at the rate of about 100 feet per second, or 1,637 miles a day, and would need more than 150 years to make the journey. Sound would do it in about 14 years if it could be transmitted through celestial space, and a cannon-ball in about 9, if it were to move uniformly with the same speed as when it left the muzzle of the gun." These illustrations are striking—we have seldom used them in lecturing on the sun without noticing that they produce a strong impression on the audience. But we find another illustration in the book before us, still more impressive. While the earth is travelling 20 miles along her circular path, her path deviates from the tangent at the first point of the arc of 20 miles by only one-eighth of an inch. This is the distance which the sun, with all his tremendous attractive energy, draws the earth towards him in a second of time.

of a scheme of circling worlds. It is estimated that the intensity of sunlight at the sun's surface exceeds 19,000 times that of a candle flame; 5,300 times that of metal in a Bessemer converter, 146 times that of the calcium light; 3·4 times that of the electric arc at its greatest attainable brilliancy. The heat emitted by the sun in every second of time is as much as would be obtained by the consumption of 16,136 billions (millions of millions) of tons of coal per second.

Passing to the consideration of the aspect of the mighty globe, which thus rules, lights, and nourishes the earth and her fellow planets, we find in this treatise one of the most complete, though concise, accounts of telescopic study of the sun which has yet been published. The nature and appearance of the spots, the faculæ and the granules, the way in which the spots are formed, vary in structure and disappear, their motions in different solar latitudes, and other phenomena of interest, are described succinctly and reasoned upon with skill and caution. We must leave, however, our remarks on this important part of Professor Young's subject to another occasion.

PRECESSION OF THE EQUINOXES.

By the Editor.

Since the whole globe of the earth reels in the way described in No. 11, while the sphere of the fixed stars remains all the while unchanged in position, the poles of the heavens (the points towards which the axis of the earth is directed) must move in circles around the poles of the ecliptic (the points towards which a perpendicular to the plane of the earth's motion is directed). The inclination of the earth's axis being about $23\frac{1}{2}°$, the circles thus described by the poles have an arc diameter of about 47°. In our illustrative map we show a part of the circle described by the north pole of the heavens around the pole of the ecliptic. This part shows the course pursued by the pole from between five and six thousand years B.C. to about 8500 A.D.

We must, however, note two circumstances in the part of the North Pole's path thus given. There is a reeling motion due to what is called nutation, a sort of small reel executed in about eighteen and a half years, the effect of which is to make the course of the pole wavy, instead of the simple circular path we have shown; this does not much affect the accuracy of the picture. There is another and somewhat more important correction, which would not, indeed, make much difference in our map, but in a map of the pole's course, on a larger scale, would have to be taken into account. The pole of the ecliptic is itself moving. The plane of the earth's path is slightly inclined to the median plane—the so-called invariable plane—of the solar system. This invariable plane is nearly coincident with the plane of Jupiter's orbit, but not quite. It crosses the ecliptic ascendingly, in about longitude 103°, and is inclined to that plane at an angle of about 1° $35\frac{1}{2}'$ (the plane of Jupiter's orbit crosses the ecliptic ascendingly in longitude 99°, and is inclined to it at an angle of about 1° $18\frac{2}{3}'$). The pole of the ecliptic describes then a small circle about the pole of the invariable plane, this small circle having a diameter of about 3° 11′. The time of circuit is not as yet known, because of uncertainties which exist as to the exact masses of the members of the solar system. Suffice it here to notice that, owing to this change, it is calculated that, during the last 3,000 years, the pole of the ecliptic has diminished its distance from the pole of the equator by about 25′. That the physical theory is not far from correctness is shown by the circumstance that, from observations made by Tcheou King 1,100 years before the Christian era, the obliquity would seem to have been then about 26′ greater than at present.

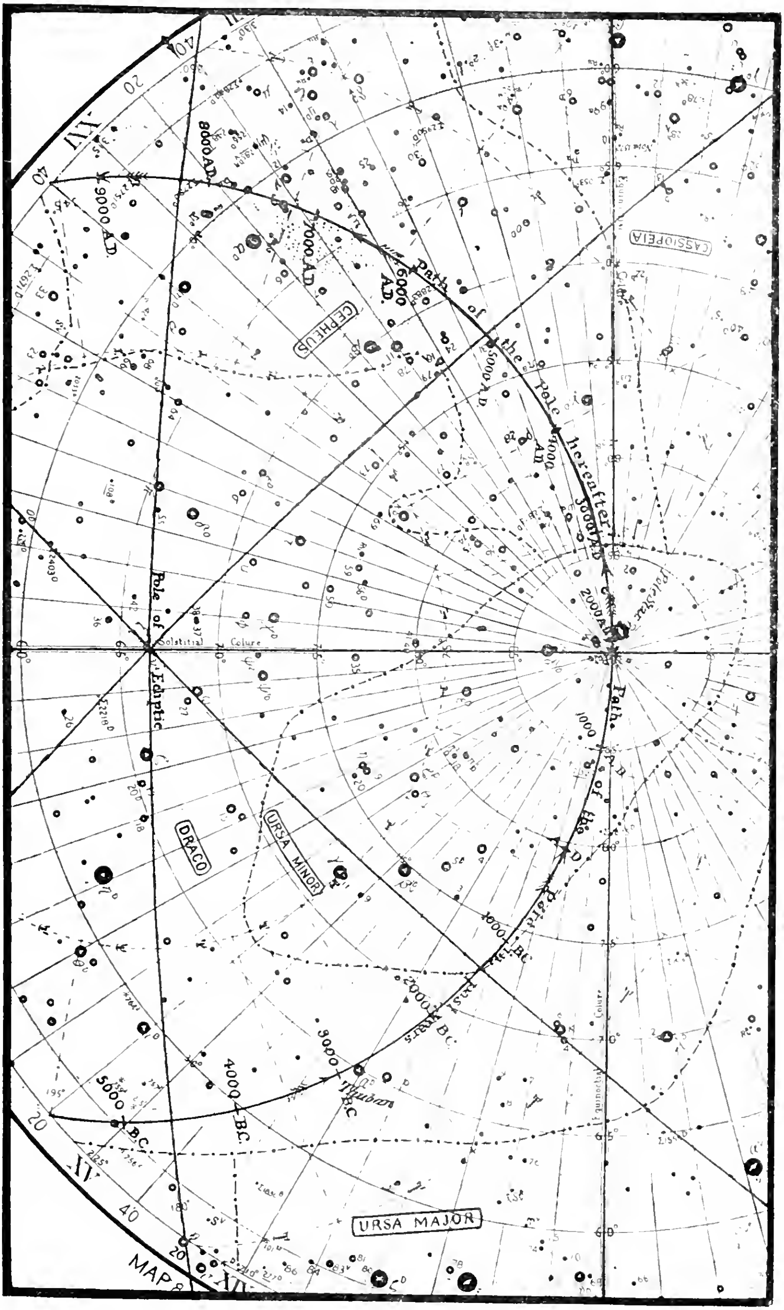
Apart from this slight change, the path shown for the pole in our illustrative map is sufficiently near to correctness. We note that the only conspicuous star which has been very near the pole during the last five or six thousand years is the star Alpha Draconis, or Thuban, yet this star must have been far more conspicuous in Bayer's time (still more, therefore, in all probability, in the time when it was the Pole Star) than it is in our own; for Bayer lettered the stars in each constellation in the order of their brightness, as nearly as he could estimate that order with the comparatively rough light-measuring methods available in his time; and the most cursory study of the stars of the Dragon shows that both Beta and Gamma are now much brighter than Alpha.

Thuban was nearest the pole about 2,700 years ago; but, of course, it was the Pole Star for a long time before and after the period when it was nearest the pole (just as Alpha of the Little Bear is now the Pole Star, though some 300 years will elapse before it is at its nearest to the pole). In dealing with the Great Pyramid, and trying to determine when it was built, it is not the time when Thuban was nearest the pole that we have to consider, but the time when it was at a certain definite distance (about 3° 10′) from the pole. Now, a study of the pole curve in our map shows that the star Thuban was at this distance from the pole (about $\frac{11}{15}$ths of the distance between the successive circular lines in the map) at two epochs. We open out a pair of compasses to the distance just named, and placing one point on α Draconis, describe with the other a circle; this will cut the path of the pole in two points, one corresponding to about 3350 B.C., the other to about 2170 B.C. Either of these would correspond with the position of the descending passage in the Great Pyramid; but Egyptologists tell us there can absolutely be no doubt that the later epoch is far too late. If, then, we regard the slant passage as intended to bear on the Pole Star at its sub-polar passage, we get the date of the Pyramid assigned as about 3350 years B.C., with a probable limit of error of not more than 200 years either way.

Be this as it may, we know that in the past the constellation of the Dragon was at the pole or boss of the celestial sphere. In stellar temples, like those of which Rawlinson gives examples, the Dragon would be the uppermost or ruling constellation. And here, in passing, it may interest the reader to note that some find evidence in this relation, that when writers of old spoke of the Old Dragon as having been cast from heaven, carrying two-thirds of the celestial

---

* Fellows of the Astronomical Society will remember, with some amusement, how, a few years ago, Lieut.-Col. Drayson (a mathematician of some skill) gravely told astronomers of the observed diminution of the ecliptic obliquity, as if it were some unexplained change, instead of being one of the most beautiful confirmations of the theory of gravity. On the one hand are the calculated effects of the perturbing action of the several planets; on the other, an observed change, precisely corresponding with calculation; yet, instead of "confirmation strong" of accepted views, Col. Drayson could only find a source of perplexity. On this imaginary difficulty he based an entirely impossible theory of the glacial period. Yet, when he described his views about the changing obliquity of the ecliptic, one of the greatest mathematicians living explained to him that the change he found so perplexing was a necessary consequence of the action of gravity. He had been too many years fostering his delusions to give them up. Such is the birth and growth of paradoxes: an error which could have been readily corrected at the outset, sets a man wasting years of labour in developing a false theory; and when, at last, his error is corrected, he is too much in love with his own work to give the error up.


CEPHEUS
CASSIOPEIA
DRACO
URSA MINOR
URSA MAJOR
Path of the Pole hereafter
Pole of Ecliptic
Solstitial Colure
Polar Star
9000 A.D.
8000 A.D.
7000 A.D.
6000 A.D.
5000 A.D.
4000 A.D.
3000 A.D.
2000 A.D.
1000 B.C.
2000 B.C.
3000 B.C.
4000 B.C.
5000 B.C.
MAP 8

beings with him, reference was made (unconsciously, perhaps, on the narrator's part) to some tradition of the passing away or fall of the Dragon from its former ruling position among the constellations. Those who thus interpret ancient records (much more ancient than Jewish history) find in Hercules, with his heel assailed by the serpent, as in our constellation figures, the first Adam; in Ophiuchus, the serpent holder, the second Adam. In Argo they find the Ark—in fact, in a whole series of constellations they find the story of the Flood. In Aquarius, with the streams pouring from his water jug, they find the beginning of the Flood. In the river Eridanus and the seas in which Pisces and the great sea monster, Cetus, seem to swim, they see pictured the prevalence of deep water over the whole earth. The Raven of the heavens is the Raven of the Flood-narrative. Argo is the Ark, shown as if only the stern half of a great ship lodged in the mountain. The Centaur, bearing sacrifice, as Aratus says, to Ara, the altar, is Noah offering sacrifice after he had left the Ark; and the Bow of Sagittarius in the smoke (the Milky Way) which seems to ascend from the altar,—

Ara ferens thuris, stellis imitantibus, ignem,—

is the Bow of the Promise.

These may, of course, only be fancies, but it is singular how closely these constellations, which are among the few really seeming to picture recognisable objects in the heavens, correspond in sequence and in range of right ascension with the events recorded respecting the Flood. For my own part, I am not of those who wonder that the ancients should have seen figures of the animals and other objects with which they were familiar in the heavens. From my boyhood upwards (and the boyhood of the individual is like the childhood of the race) I have seen figures among the stars, the figures being always such as I was familiar with. Even so late as my recent visit to the southern hemisphere, I found that almost despite myself the novel-looking star groups formed figures* with which I was ever after compelled to associate them; and I cannot doubt that it was the same with the childhood of the human race. There is certainly a well-shaped ship where Argo was seen by the ancients; the water streams of Aquarius and Eridanus are distinctly visible; Ara is a well-shaped altar; and though at present the figure of the Centaur (the man part) is not so upright as it was before precession had tilted it over (as it has the ship), we can still see there the figure of a portly man bearing something towards the altar. The bow is clearly seen, and nothing can be better in accidental picturing than the curling streams of smoke (figured in the stars of the Milky Way), which seem to ascend from the flat summit of the altar.

But precession has altered the configuration of all the star-groups as seen when most favourably situated for observation. Take a star globe, and, holding it with one forefinger near Thuban (Alpha Draconis), and the other at the opposite point of the sphere, notice the constellations as they slowly rotate. Note how steadily the ship sails past its highest point, on upright keel; how upright the Centaur and the Altar;* and how many groups, now almost unrecognisable, are seen in their new aspect to be fairly entitled to the names which the ancients bestowed upon them.

* I had always expected, from Sir John Herschel's description, to find Orion when inverted, as we see him in the southern skies, a very noble and impressive figure. But the very first time I so saw it, I immediately recognised in the figure, Clive Newcome's picture of Fred Bayham, and I never afterwards saw the constellation without at once seeing in it that ridiculous figure. When people tell me they cannot see a Bear in Ursa Major, I can only wonder at their blindness; the head of the bear being to me as obvious and as obviously ursine as a group of stars could well be.

* Only the modern figure of the Altar is absurdly drawn upside down. In old globes and charts we find it properly drawn.

SEEING THROUGH THE HAND.

AN OPTICAL ILLUSION.

By Thomas Foster.

There is a series of illusions affecting the apparent shapes and positions of solid figures—not only regular geometrical figures, as prisms, parallelopipeds, &c., but all solids whatever—drawn as if formed of a transparent material, so that their farther as well as their nearer outlines or edges can be seen. It can be readily shown that there is a law connecting in every case the false figure with the real figure. I have prepared a paper on illusions of this sort, with suitable illustrations, and another paper with illustrations on some curious cases of apparent motion in sets of concentric circles. But these and other papers on illusions are, it appears, kept over for the present by the pressure of other matter. [Circles next week.—Ed.]

In the meantime, I wish to submit to readers of Knowledge (as occupying less space) an illusion which seems to me exceedingly instructive, as bearing on the question how we see. Everyone knows that the eye itself is simply the organ by which the optical nerve is affected by light, and that it is by this nerve that the brain becomes cognisant of these light effects, the brain interpreting the messages brought along the optical nerve into information respecting the objects of sight.

Now the two eyes, and the optical nerves which extend to each, convey at all times different messages to the brain, which yet, as a rule, combines the two sets of messages into a consistent account (so to speak) of what is seen with both eyes. Even when the eyes differ in focal length, so that, as separately analysed, the views obtained by the two eyes are utterly unlike, the mind is very seldom perplexed by the two different accounts conveyed to it. But in the following experiment the eyes entirely deceive the mind, conveying to it the absurd impression that there is a hole right through the palm of the hand, or of a book or other opaque object which may replace the hand in the experiment.

Roll a sheet of card or paper (or the number of Knowledge now in your hand) into a tube nine or ten inches long, and about an inch in diameter. Holding this tube with the right hand, say, look through it with the right eye, while the left hand is held six or seven inches from the eye, the palm facing you, and touching the tube a little below the lowest joint of the little finger, that is, at about the level of the middle of the palm. Then, if both eyes are open, the tube being held touching or close to the right eye, while the left eye looks at the left palm (at about the nearest distance for distinct vision), the appearance presented is as though there were a circular hole about an inch in diameter through the palm of the left hand.

Now, in this case, the mind does not need to be told that it is deceived. The observer knows as well as possible that while he seems to be looking with the left eye through the palm of the hand at objects beyond, he is in reality looking at those objects with the right eye through the tube. Yet the mind does not correct the illusion, clearly though it recognises that there is illusion and its nature. The illusion is as preposterous as that experienced when

crossing the second finger over the first, we feel the tip of the nose with these fingers thus interchanged in relative position. In this case the observer seems to feel two nose tips, though he knows certainly that he has but one; he knows, too, just how the illusion is occasioned, but for all that the illusion remains.

The two most trustworthy senses, sight and feeling, being thus able to deceive—to palm off upon us, so to speak, what each of us knows to be false,—we see (and feel) how very far from the truth is the saying that seeing or feeling is believing. If these senses deceive us when we know precisely what they are doing (as when conjurors explain and illustrate in action the manner of their tricks), is it to be supposed that they do not often deceive us, and still more thoroughly when we have no means of testing what they tell us by what we already know?

INTELLIGENCE IN ANIMALS.

IN the case last considered, we see that a dog, belonging to a species not distinguished for keenness of scent, was not long deceived by a picture, even under circumstances favouring the deception—as his previous sleep, the position of rest from which he saw the figure, and the strong light shining upon it. As this was the only instance known to one who was familiar with the ways of dogs, the negative evidence respecting the recognition of pictures by animals is rather strong. However, there have been cases where animals, if not actually deceived by a picture, seem certainly to have understood what it was intended to represent. The following case seems to me full of interest. It is related by Mr. Chas. W. Peach, of Edinburgh. He remarks, first, that in certain publications dogs are said never to have recognised a painted likeness. "During my residence in Cornwall," he goes on to say, "I had a most intelligent and faithful dog for fifteen years. I had him when a month old. His mother was a beautiful liver-coloured spaniel, rather large; his father, a black Newfoundland; my dog took after him in colour and shape. In 1843, a young and self-taught artist asked me to allow him to paint my likeness in oil colours, and I consented. His studio was in the next town, three miles distant, and, as often as required, I went over. I, however, did not take my dog with me. It was done in "kitcat" size, and he succeeded so well in the likeness and artistic work, that, when exhibited at the annual meeting of the Polytechnic Society at Falmouth, a medal was awarded for it; and, as well, it was highly commended. The artist was so grateful that he presented me with the painting, and I still have it. When it was brought to my house, my old dog was present with the family at the "unveiling"; nothing was said to him nor invitation given him to notice it. We saw that his gaze was steadily fixed on it, and he soon became excited, and whined, and tried to lick and scratch it, and was so much taken up with it, that we, although so well knowing his intelligence—were all quite surprised; in fact, could scarcely believe that he should know it was my likeness. We, however, had sufficient proof after it was hung up in our parlour. The room was rather low, and under the picture stood a chair; the door was left open without any thought about the dog; he, however, soon found it out, when a low whining and scratching was heard by the family, and on search being made, he was [found to be] in the chair trying to get at the picture. After this I put it up higher, so as to prevent its being injured by him. This did not prevent him from paying attention to it, for whenever I was away from home, whether for a long or short time—sometimes for several days—he spent most of his time gazing on it, and as it appeared to give him comfort, the door was always left open for him. When I was long away he made a low whining, as if to draw attention to it. This lasted for years, in fact so long as he lived and was able to see it. I have never kept a dog since he died: I dare not, his loss so much affected me."

A similar anecdote is related of a painting by the elder Phillips. "Many years ago," says the lady who narrates the tale, "my husband had his portrait taken by T. Phillips, sen., R.A., and subsequently went to India, leaving the portrait in London to be finished and framed. When it was sent home, about two years after it was taken, it was placed on the floor against the sofa, preparatory to being hung on the wall. We had then a very handsome, large, black-and-tan setter, which was a great pet in the house. As soon as the dog came into the room, he recognised his master, though he had not seen him for two years, and went up to the picture and licked the face. When this anecdote was told to Phillips, he said it was the highest compliment that had ever been paid to him."

We have seen how a bull-dog, the least intelligent, perhaps, of all dogs, behaved in presence of a portrait. We have now to consider the behaviour, under similar circumstances, of the British mastiff, a more intelligent animal than the bull-dog, though not regarded as standing by any means first among dogs in this respect. The particular mastiff in question is one to whom I had the pleasure of being introduced some five years since, Dr. Huggins' dog Kepler. He is worthy of a brief biographical sketch. He was a son (that is, Kepler was) of the celebrated Turk, and was born about the year 1871. "He stands," wrote Mrs. Huggins of him, towards the close of 1876, a few months before his lamented decease, "thirty inches high, and is lion colour on the body; his face, the tips of his ears, and the tip of his tail, are marked with black. In disposition he is usually exceedingly affectionate and gentle, though he can be otherwise. Probably he thinks [though here I must confess that, strongly though Mrs. Huggins's opinion would support my case, I cannot altogether agree with her] that the words of George Herbert may apply to dogs as well as to men, and so reasons that—

He is a fool who cannot be angry,
But he is a wise *dog* who will not.

He has a clear idea of his duty in life. As Mr. Carlyle would say, 'he has found his work to do,' and considers it to be—to borrow the expression of an old writer—'to kepe his mastre and his maistirs hous.' To this end he is continually on the watch, barking in quite different ways as different comers approach. He has a bark of welcome for those he loves; of courtesy for mere acquaintances; of inquiry for strangers; of warning against enemies," an approach here, one may say, to language.

Kepler first attracted scientific attention by a peculiarity which, most probably, must be regarded as a result of instinct, or as, at any rate, inherited, since nothing in Kepler's own life explains it as the result of any process of reasoning. "When he," that is Kepler, "was very young," writes Mrs. Huggins, "his master discovered on taking him for a walk one day, that he" (Kepler, not Dr. Huggins) "was very much frightened at the sight of a butcher's shop, and some little time afterwards, when he was out with a servant, the feeling again showed itself, but in a much more marked manner. On this occasion Kepler threw himself upon the ground near the butcher's, exhibiting every appearance of terror, and as no amount of coaxing could induce him to pass the shop, the servant was at last obliged to bring him home again. His master, upon this, wrote to Mr. Nicholls, from whom he had purchased

Kepler, asking if he could throw any light upon this strange dislike. Mr. Nicholls replied that it had been strongly marked in Kepler's father and grandfather, and was unusually strong in one of his brothers, so much so, indeed, that he would fly at a butcher, even when dressed in plain clothes. These facts being very striking, Mr. Huggins Kepler's master wrote details of them to Mr. Darwin, who was so much interested, considering the circumstances a clear instance of inherited antipathy, that he sent an account of them to *Nature*. The facts attracted much attention at the time, and various theories were put forward to explain them. In connection with this dislike to butchers shown by Kepler and his relations, it is interesting to notice that a similar antipathy is noted by Jesse to dog killers, as mentioned both by Lord Bacon ('Sylva Sylvarum'), and Sir Kenelm Digby ('Treatise on the Nature of Bodies'), as having been common among dogs in their times. The passage from Sir Kenelm Digby's Treatise runs thus: 'We daily see that dogs will have an aversion from glovers, that make their ware of dogs' skin; they will bark at and be churlish to them, and not endure to come near them, though they never saw them before.' Dog-killing was an old custom in August." Perhaps, after all this intense aversion to butchers, dog killers, and others who may be supposed to bear about them some scent of blood, suggesting to the dog-mind the slaughter of his kindred, may be an effect of reasoning, not, as I have suggested above, of instinct only. A dog may argue that the scent can only be explained in one way, and that the explanation is such as to suggest danger to himself—"*hinc illæ lachrymæ*."

EXCAVATIONS AT THE PYRAMIDS.

(From *The Academy*.)

The Tombs, Gheezeh Pyramids. *Nov.* 26, 1881.

DURING the past six weeks excavations have been carried on by me here, under the authorisation of M. Maspero, not for obtaining portable antiquities, but for deciding questions of architecture and measurement. Many points of interest have been uncovered for the first time in modern history, though the work was not on a large scale, and the number of excavators never exceeded twenty. There have been over 280 holes sunk, varying from a foot deep to shafts twenty feet deep and trenches ninety feet long.

A brief notice of the work done may be worth giving at once, without waiting for the complete publication of it, along with my survey of the pyramids (made during five months of last season), to which it is a necessary sequel, for fixing the exact fiducial points of the ancient constructions.

At the Great Pyramid, the entrance passage has been cleared enough to examine it throughout, and to enter the subterranean chamber freely. Some of the loose gravel in the "grotto" of the well has been moved, showing that there is a natural vertical fissure filled with the gravel. The casing and pavement of the pyramid have been found *in situ*, at about the middle of the west, east, and south sides; it was already exposed on the north side, on which alone it has been hitherto known. The outer edge of the rock-cut bed of the pavement has been cleared in parts of the sides, and at the north-east and south-west corners. The great basalt pavement has been cleared in parts, and the edge of the rock-cut bed of it has been traced along the north-east and south sides; but its junction with the limestone pyramid paving (which is at the same level) could not be found, as both are destroyed at that part. The ends of the great trenches around the basalt pavement have been partly cleared. The bottom and sides of the east-north-east trench have been cleared in parts to show the form. No bottom was found under nine feet of sand in the north trench. The small north-north-east trench has been cleared in parts up to its inner end at the basalt paving, where it is much smaller, and forks into two. The various rock cuttings and trenches north-east of the pyramid have been cleared and surveyed, but refilled, as the road passes over them. A piece of the casing of the pyramid, found near the base on the west side, has Greek inscriptions, apparently Pto . . . . Sôt . . . . (perhaps Ptolemy VIII., as the s is round); and Markos K . . . . over which is hammered roughly . . . . in a p . . . . in Arabic. Nothing, besides a few fragments with single letters, had been previously discovered of the many inscriptions that existed on the casing.

At the second pyramid the corners have been all cleaned. The site of the edge of the casing has been found in six places near the corners, and the casing itself uncovered at the south-west. The edge of the bed of the pavement has been found on the north and west sides. The peribolus walls of the pyramid have been cleaned in many parts, showing that they are all carefully built, and not of "heaped stone rubbish," as had been hitherto supposed. Also, the so-called "lines of stone rubbish" on the west side of the pyramid prove to be all built walls, forming a series of long galleries about sixty in number, each about 100 ft. long, 9 ft. wide, and 7 ft. high, with ends and thresholds of hewn limestone. They would suffice to house two or three thousand men, and I can only suppose that they were the workmen's barracks. Fragments of fine statues in diorite and alabaster were found here, like those in the temple of this pyramid. The great bank of chips on the south side of the cyclopean wall north of the pyramid proves to have retaining walls built in it to hold up the stuff. The peribolus wall on the south-south-east of the pyramid is of fine limestone, of good workmanship, like most of the tombs of the period. The enormous heaps of rubbish south of this wall were slightly cut, and found to consist of tipped out, stratified, clean chips of limestone, like the rubbish banks of the Great Pyramid, but inferior stone.

At the third pyramid, the granite casing has been uncovered at its base in five places near the corners. The peribolus walls have been cleared in many parts all round, and found, in every case, not to consist of heaped stones, but to have carefully-built vertical faces, like the second pyramid peribolus, but of inferior work; and the wall on the south side is better built, and very wide.

The small pyramids have not been cleared for lack of time, as they are rather deeply buried; but a part of the rock-cut bed of pavement of the northern one near the Great Pyramid was accidentally uncovered close to the edge of the bed of the basalt pavement.

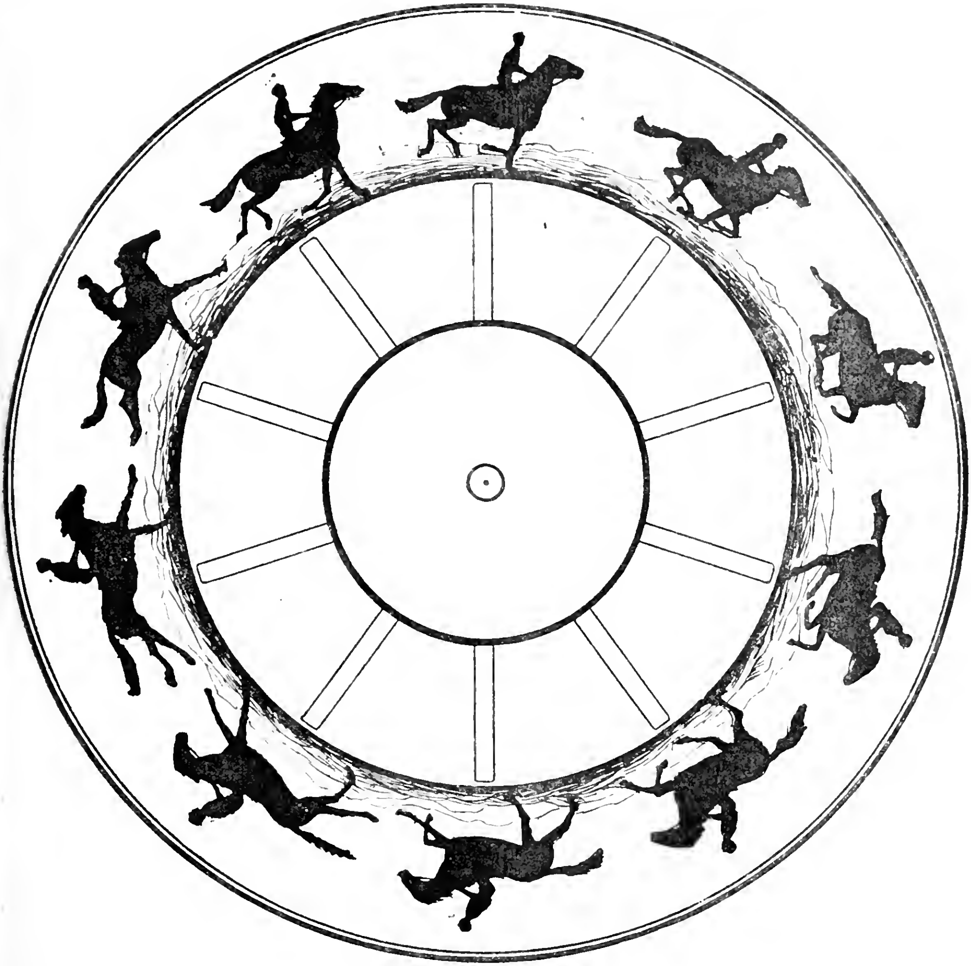
Though I am obliged to suspend work here at present, yet I shall be very glad to receive any suggestions of points needing examination (addressed to Poste Restante, Cairo); and, if they are practicable, I may find an opportunity for further work two or three months hence.

When all the paper work of this survey is finished, we shall know the sizes and distances of the pyramids within a quarter of an inch; and there will be fresh soil for the growth of theories, *as the Great Pyramid proves to be several feet smaller than hitherto supposed*, the sockets not defining the casing at the pavement level, though defining it, perhaps, at their own respective levels.

W. M. FLINDERS PETRIE.

[Let not pyramidalists despair. It will be just as easy to find all the features of the solar system in the Pyramid with the new measures, as it was before. The new coincidences will be worth just as much, too, as the old ones.—ED.]

MANUFACTURE OF GAS FROM WOOD.—When wood is burned, the chief product of the combustion is a gas called carbon dioxide or carbonic anhydryde (CO_2), *i.e.*, one of carbon united with two of oxygen. The gas is the same which gives the effervescence to soda-water, &c. This fact is made use of in New York, by a process called Wilkinson's, for the manufacture of wood-gas. CO_2 is itself a non-supporter of combustion, extinguishing a light when plunged into a jar of this gas, when it is inhaled destroys life, as in the fatal valley of Java (Johnston's "Chemistry of Common Life"), or anywhere else where persons have got into an atmosphere where it predominates. If we, however, get rid of one of oxygen in CO_2, *i.e.*, reduce CO_2 to CO, or carbonic oxide, we obtain the "Wood-gas" now used in New York. This is done "by forcing the gas through red-hot charcoal" (abs. *Chem. Soc. Jour.*). By doing this we get rid of the one of oxygen which was in excess, and so obtain a gas which has different properties—for it is now a "supporter" of combustion. The gas needs very little purification, the dangerous, obnoxious, and otherwise objectionable products being absent. The wood is only drawn from the retorts twice a day, "to prevent too great an accumulation of charcoal," whereas coke is withdrawn each time. "53,832 cub. feet of gas are produced from a cord of wood, costing 5·25 dollars (about 21s.) per cord of 3,300 lb. The quantity of gas alone is thus *four times as great* as that which can be obtained from coal, costing an equal sum per ton as wood does per cord." In New York, wood-gas is sometimes mixed with an inferior coal-gas, forming what is known there under the name of "commercial," which has a great candle power.—F.C.S.



THE MAGIC WHEEL.

We give, this week, the series of pictures of a galloping horse. We have to notice, however, that the instructions given in the *Scientific American* are erroneous. If a slit is cut exactly beneath each figure of a horse, we get a view of a horse galloping without advancing. Eleven slits should be cut (which the reader will find no difficulty in doing), at equal distances, when the horse will not only be found to move his legs, tail, &c., but to advance, as might reasonably be expected from a galloping horse. The same remarks apply, of course, to the trotting horse, in number 10, in fact, the trotting horse alone is taken from the *Scientific American*, the ten views of a galloping horse being from a series kindly supplied to the editor by Mr. Muybridge, of San Francisco, who photographed them. Twelve slits will produce the desired illusion even better than eleven, a correspondent notes, and their places are more easily measured.

THE MOON AND THE WEATHER.

It is held by a large number of educated people that a belief in the influence of the moon on the weather is a remnant of a past and now discredited system of divination by which all the events of life were referred to the influence of the heavenly bodies. It should, however, be borne in mind that the germs of truth may be found in every system of religion and philosophy, and that the interests of truth are served better by seeking for the truth underlying any particular theory, than by denouncing it as false because it lies beyond the range of superficial observers.

In the first place, the theory of lunar influence upon the atmosphere stands apart in a great degree from the old system of astro-meteorology; for, as may be seen in the text-books containing the

data on which predictions were founded, notably Ramesey's "Astrologia Mundi," it was the conjoined influence of the sun and planets, rather than the moon's position relative to the earth, which was mainly relied on as causing atmospheric changes, and whatever nucleus of truth the system contained was necessarily obscured and rendered almost valueless by the imperfect observation and boundless credulity of the times. And, in the second place, it must be clear to every tyro in natural science, that if it be rational and in accordance with the verifications of science, to assert that the moon's influence acts upon the waters of the ocean, it cannot be foolish and irrational to hold that the same influence affects the waters of the clouds and the air in which they float—both ponderable bodies equally subject to the laws of gravitation. So much may be taken for granted.

But the most tangible objection urged against the theory of lunar influence is that atmospheric changes are not coincident—or, if so, only accidentally—with changes of the moon, and, therefore, it is unreliable. This reasoning, however, is in itself fallacious. Those who predict changes in the weather from the moon's influence, allege that the disturbing force is felt when she is on the equator, crossing from north to south declination, or, *vice versâ*, arriving at one of the stitial colures at its extreme north or south declination. And, of course, she may or may not at such times be at the points called syzygies (the new or full moon), or at the quadratures. Thus, an atmospheric change is not necessarily to be expected at every change of the moon; but it may reasonably be when she arrives at one of the cardinal points, and it is found by experience almost invariably to occur.

But, without the knowledge of what the theory really is, the most acute critics must fall into error in judging a system whose principles they misapprehend. To give a striking instance—It is asserted by no less an authority than Professor Jevons, in his Science Primer, "Logic," that the belief in lunar influence on the weather is an illustration of "fallacy." He says:—"In one sense it is a fallacy that the moon governs the weather, because long and careful enquiries have shown that there is no correspondence between changes of the moon and changes of the weather." This reasoning clearly shows how the subject is misunderstood by those who accept the popular notion of the theory. Certainly, a change need not be expected if the syzygies and quadratures are alone regarded. The same writer says further:—"There are at least twelve new moons in each year, and changes in the weather take place in this country at least once a week on an average. It is, therefore, quite likely that a new moon and a change of weather will happen together now and then." To this the reply is that it is quite likely, for the moon usually, in the course of each week, either crosses the equator or arrives at one of the stitial colures.

And those who have made atmospheric changes a subject of special observation, are compelled to admit that the theory of lunar influence cannot be tabooed as unworthy of attention. In a number of the *Journal des Débats*, published last summer, M. de Parville, in an article on the temperature of the present year, says:—"A very long series of observations has also shown that the moon, which passes every month from one hemisphere to the other, influences the direction of the great atmospheric currents. The changes in those currents, in consequence of the prevailing moisture or dryness, are intimately connected with the relative position for the time being of the sun and moon."

Admitting that the time of atmospheric changes, taking local and climatic conditions into account, may be approximately known, it remains to be seen how far the relative positions of the other members of our solar system affect the character of the change; whether, for instance, the chemical rays of one planet, when stationary, or in conjunction or opposition of the sun, incline to heat, and those of another to cold, is a legitimate matter of investigation, always remembering that the wider the scope for surmise and assumption, the more carefully must hastily-propounded theories be tested by verified conclusions. To formulate into a system the effects of solar, lunar, and planetary aspects and motions, requires long-extended and careful observation, but, on the other hand, it must not be too hastily assumed that an error in detail proves the principle to be false—a sweeping condemnation which no logical mind should accept. When mathematical or scientific demonstration cannot be given off-hand, and when, consequently, so much depends on the even balance of the judicial faculties, scepticism is as dangerous to the interests of the truth as credulity. H. A. BULLEY.

THE PRINCIPLE OF THE VERNIER, *vide* last week's KNOWLEDGE, page 224, 11th line from bottom,

for "$x = \frac{7}{10}$ + a fraction," &c.,

read "$x = \frac{8}{10}$ + a fraction," &c. JOHN R. CAMPBELL.

SOMETHING ABOUT THE POTATO.

HOW few of us ever give any thought to the food we eat, beyond the fact of its being well or badly cooked. Very few know that the potato for years past has, amongst other articles of food, received the earnest attention of investigating chemists at home and abroad. A few of the facts resulting from their work during the last few years, I now propose to try without going into that part of the researches which can only be of interest to the chemist, such as giving the results of analyses, or the names of the various *alcohols* which we are able to extract from the potato—and put before you as an interesting article.

I daresay some, or rather most, of you have heard of potato spirit, to obtain which, potatoes may be mashed either with mull or sulphuric acid. Mull, if anything, gives a trifle better result. In manufacturing this spirit we obtain an oil, called fusel oil, from which we can extract *nine* different alcohols, as well as other organic substances, the names of which I will not trouble you with. Besides the spirit and ether we obtain from potatoes, we may soon advantageously obtain pressed yeast. At present there is a little difficulty to be overcome in the working of the process, the difficulty being that the yield of spirit is less in proportion to the yield of yeast; but in a short time the chemists, who are working the solution of this problem out, expect to overcome this obstacle.

Another important produce manufactured from the potato is potato starch, which, by the action of acid, is converted into glucose-sugar, or syrup, which is chiefly made in America and Germany, though we have, at the same time, some important works in this country, and one quality is, from the samples which I have seen, for brewing and sweet manufacturing purposes, the purest. Owing to the large amount of sulphuric acid present in some of the prepared syrup in America, it has been found to have had an injurious effect on the health of those people who have taken the syrup, but this acid should not be present in so large an amount as this. Some samples I have tasted were perfectly free from it, and you could only taste the very sweet taste of glucose. On the other hand, other samples have really been unbearable, owing to the large amount of acid present—in one case, in the form of sulphurous acid, which leaves the same taste in the mouth as a mouthful of burnt sulphur would do, if inhaled. As glucose is one of the chief ingredients required for brewing, it is easily seen what an important substitute for malt this must be to the brewers, though a few years ago brewers would not acknowledge using it! Its use has since then become more general, so brewers are not ashamed to own to using it. Without going into detail, I will here give a general average analysis of the potato:—

Water.	Nitrogenous substances.	Oil.	Non-Nitrogenous Substances	Cellulose.
75·77	1·79	·16	20·56	·75

A full analysis of the above, giving the names of all the non-nitrogenous substances, &c., with their percentages, would take up a good deal of space, without making the general reader any the wiser than he was at the beginning.

The next thing to be considered is the general cultivation of the potato, as made known to us by the chemists who have been studying this branch of the tuber. Generally speaking, large seed produces more large potatoes than small seed. Most people might take this for granted, without corroborating the supposition by numerous experiments, as the chemist does, for the chemist must bring forward positive results only to bear out his suppositions.

The influences of the blossoms makes a great deal of difference in the yield of the crop, as the following results show. 208 centrs. 19 lb. of tubers were obtained from plants from which the blossoms had been removed, and only 181 centrs. 48 lb. from plants not so treated. I do not think many readers of KNOWLEDGE would have thought that the difference could have been so great.

It has been found that the growth of sprouts on potatoes depends on their nearness to the apex of the tuber; if they are treated with water in sunlight, the growth increases forty or fifty times, although this not the case in the dark. Absence of light is necessary for the growth of young tubers.

Heat produces very beneficial results to the growing plant. On this account too early planting is injurious, for potatoes require the same total heat to bring out the leaf buds; so, should the plants be planted too soon, it would require, as experiment shows, perhaps forty days instead of fourteen to bring them to this stage, but it is well, at the same time, to bear in mind the fact that if they are left too late, the ground will become equally unsuitable.

Potato rot is the next point to be considered, of which there are three kinds—viz., dry, wet, and sweet rot. In the dry and wet rot it has been found that the potatoes with the maximum starch resist the disease most effectually. Potatoes grown on moist soils and soils containing much organic matter are most liable to the disease. The starch in diseased potatoes is yellow, but can be used for manu-

facturing a second quality of dextrin. In dry rot the tuber is loose and spongy, coated on the outside with mould. The disease is originally due to Bacteriæ, and another parasite is often present at the same time. Wet rot, I forgot to say, differs from dry rot in this respect, the interior of the tuber is partially liquid and the outside coated with mould. During rotting, the potato loses half its nitrogenous constituents and the whole of the sugar.

The Sweet wort is formed by a parasite called *Mucor mucedo*. The mycelium of the parasite travels to a certain distance inside of the potato, and then disappears. Bacteria rapidly destroys the plants. There is another parasite, called *Aspergillus niger*, which produces a similar result. It seems rather uncertain as to whether cane sugar or glucose is formed; some chemists say the one, others the latter.

We all are familiar with frozen potatoes, but not many of us know in what manner the frost affects them. Frost has the effect of doubling the amount of sugar in the tuber, the starch diminishing in proportion; part of the protein passes from the coagulable to the soluble form.

To prevent rot in potatoes, the tubers, whether sound or diseased when taken from the ground, are left in a weak solution of calcium chloride—one part to a thousand of water—for half-an-hour. They are then transferred to a soda solution of the same strength, after which they are washed with clean water and air dried. Half kilo of calcium chloride and the same amount of soda is sufficient for 250 kilos of potatoes. A kilo is a little over 2 lb. 3 oz.

Even though potatoes are badly diseased, they are still useful for cattle food. They are best cut up and boiled or merely scalded, mixed with chopped straw or chaff, and stored in narrow trenches covered up with clay. So stored, they will keep for many years. If steamed, they should be kept in casks, as there is more adhering moisture. If it is not convenient to heat them as above, they should at once be sent to the distillers.

With regard to the best kind of potato for yield and quality, there seems to be a good deal of difference in opinion, as the following examples will show. 1. For quality and quantity: Champion, Richter's Imperator, Eos, &c. 2. Richter's Imperator for yield is far the best; is of a hardy nature, smooth, and the stalk is tall and straight. Next in order are Violet, Victoria, Irlachin, Richter's Snowrose, &c. 3. For starch and yield: Eos, Aurora Alcohol; lower in yield but very high in starch, Achilles and Ceres. Richter's Imperator Early Rose, &c., very heavy yields. The above forms three opinions of three chemists; they all seem to agree that the Imperator is the largest yielder.

I think I have now put before the readers of KNOWLEDGE the most important points in the potato which have received the attention of chemists during the last few years. There is one more branch of the researches on which a few words may be said, and that is on a few of the results obtaining through *manuring*. At the same time it must be mentioned that experiments on large scales are still being carried out with respect to numerous manures on different kinds of ground.

The most successful way of manuring is to partially dig the manure into the ground. The application of nitrogenous and phosphate manures without farmyard manure is much less to be trusted on a poor soil than on a rich one. Manuring on fertile soil is not productive of any great gain. Sheep-dung produces a yield of 50 per cent. higher than any other artificial manure, and 60 per cent. higher than if no dung be applied at all. Extra supplies of ammoniacal and superphosphates produce no increase. Compost has the same effect as if the land were unmanured, except that there is an increase of starch. The unmanured plots are highest in disease. A good mixture is Chili saltpetre and Baker's superphosphate in the proportion of one of the former to two of the latter mixed with stable manure. Bone meal, one author says, increases the weight of product, and it is also very likely that the whole plant likewise increases in weight. There is another substance which has been tried, viz., peat, and good peat is found to give even better results than good stable manure.

If I have interested, and at the same time shown the readers of KNOWLEDGE that there is a good deal of matter for reflection, even in the use of every-day articles of common food, such as the potato, I am satisfied. F. C. S.

THE PURPLE OF THE ANCIENTS.

FROM the interesting "Notes on the Purple of the Ancients," by Edward Schunk, Ph.D., F.R.S., the following abstract, which may be interesting to the readers of KNOWLEDGE, is taken:—

After briefly referring to the works of Pliny and Aristotle as containing accounts of the shell-fish used in their days for the purpose of dyeing, and also of the processes incidental to same, the author proceeds to lay before the Fellows of the Chemical Society the history of this interesting dye as known to modern authors and investigators.

A shell-fish from which this dye can be extracted is found on our own coasts, Cole, in 1683, having discovered on the coasts of Somersetshire and South Wales a species of fish which, by proper treatment, could be made to dye linen and silk a *fine purple*. Reaumur, in 1710, discovered the same animal on the coast of Poitou.

The shell-fish is known to conchologists by the name of *Purpura capillus*.

The results of the various observers—Cole, Reaumur, Duhamel, (1736), and Bancroft (1803), may be summed up as follows:—

1. The colour-producing secretion, which resembles pus in appearance and consistence, is contained in a small whitish cyst or vein, placed transversely under, but in immediate contact with, the shell, and near the head of the animal.

2. This pus-like matter, either diluted with water or undiluted, on being applied to bits of white linen or calico, and exposed to sunlight, rapidly changes its colour, passing from yellow through light green, deep green, and "watchet-blue," to purplish, red or crimson. While these changes are in progress, a strong odour like garlic or asafœtida is given off.

3. To produce this change of colour, the light of the sun is essential. It is effected more rapidly by the direct action of the sun's rays than by that of diffused light, but it does not take place in moonlight, or in artificial light. If the linen or other fabric to which the secretion has been applied is kept in the dark, it remains unchanged, but when exposed to the sun it becomes purple, even after the lapse of years, though a little more slowly than at first. The metamorphosis which the change of colour indicates is not sensibly promoted by heat. It proceeds in a vacuum and in hydrogen or nitrogen gas as speedily as in air, on exposure to light. It seems to be hastened by the addition of certain chemical reagents, though these cannot replace sunlight, which is indispensable for initiating the change.

4. The colour produced is remarkably stable, resisting the action of soaps, alkalies, and most acids; being destroyed by nitric acid and chlorine.

Bizio, in 1855, experimented with the *Murex branduris*; the conclusions deduced were similar to previous observations. A. and G. de Negri, in 1875, experimented with the *Murex branduris* and *M. trunculus*.

They state that the purpurogenic secretions of these two species are not identical, the secretions of the former being photogenic, *i.e.*, not coloured when protected from the action of the light, whilst the latter becomes violet, even in the *dark*, by the sole action of the air.

Observers on the subject say that the action of light is indispensable in the action of colouring matter in mollusca.

The author conducts his experiments with the *Purpura capillus*, which he found on the rocks at low water near Hastings. He worked them up immediately after collecting, as it is necessary to use only live animals.

Breaking the shell, he easily found the pale yellow vein at the back of the animal, which may easily be cut out for examination. Putting some of the yellow viscid secretion on linen, the following changes occurred: the colour changed from yellow to green, and then to purple—emitting during the process the peculiar odour referred to in No. 2 of the summary of previous observations. The author finds that on linen, at least, the colour is not such a very brilliant purple, though it seems permanent, resisting the action of a strong soap liquor.

A temperature of 100° had no effect, either in retarding or hastening the formation of colouring matter.

Besides the above, the author found that sunlight was *not* the only agent which would bring out the deep colour, for hydrochloric acid likewise possesses this property on the veins of this remarkable shell-fish; but he thinks, at the same time, that it is open to doubt whether the action in this case is similar to that produced by insolation. It is interesting to watch the changes which occur in colour through a microscope, and if any of the readers of KNOWLEDGE desire to pursue the interesting investigation of the properties of this shell-fish, I shall be glad, at any time, to furnish them with particulars of the author's observations on this point.

Although we have, in the Old World, long since left off using the shell-fish as an article for dyeing, it is still used, to some extent, in America for this purpose, more especially on the Pacific coast of Nicaragua and Costa Rica.

The following is one of the interesting accounts, given in the paper, of the process as practised in Central America:—

"Some of the cotton fabrics manufactured by the Indians are very durable, and woven in tasteful figures of various colours. The colour most valued is the Syrian purple, obtained from the murex shell-fish, which is found on the Pacific Coast of Nicaragua. This colour is produced of any desirable depth and tone, and is perma-

nent, unaffected alike by exposure to the sun and to the action of alkalies. The process of dyeing the thread illustrates the patient assiduity of the Indians.

"It is taken to the seaside, when a sufficient number of shells are collected, which, being dried from the sea-water, the work is commenced. Each shell is taken out singly, and a slight pressure upon the valve which closes its mouth forces out a few drops of the colouring fluid, which is then almost destitute of colour. In this each thread is dipped singly, and after absorbing enough of the precious liquid, is carefully drawn out between the thumb and finger, and laid aside to dry. Whole days and nights are spent in this tedious process, until the work is completed. At first the thread is of a dull blue colour, but upon exposure to the atmosphere acquires the desired tint. The fish is not destroyed by the operation, but is returned to the sea, where it lays in a new stock of colouring matter for a future occasion."

The author had some of this fabric sent him, but the colour did not quite realise his expectations. The yarn and calico were of a *dull purple,* which is rather different from the general idea of the famous Syrian dye. They were harsh to the touch, and emitted a peculiar smell. A shell was sent with the articles; it resembled the *Purpura capillus* of our coast, but is much larger, belonging, the author says, to the *Purpura patula* of conchologists. The author informs us that in the process which the natives who dyed the goods used (samples of which he received), the whole animals were picked out with a pin. So we see there are various ways of making use of the animal. F.C.S.

METEORIC ORGANISMS.

By CARL VOGT.

THE organisms in meteorites (chondrites), announced by M. Hahn, have no existence; what has been described and drawn as such, results from crystalline conformations, which are absolutely inorganic. None of these imagined organisms has the microscopic structure belonging to the organisms with which they have been associated. In particular, the asserted sponges do not show the structure either of existing or fossil sponges; the so-called corals do not show that of polops or anthozoa; and the imagined crinoids do not show the structure of known crinoids. The observed structures are due to an opaque crust, or result from optical illusions, caused by an incomplete method of conducting microscopical researches. Apart from pulverulent masses, metallic substances, and non-crystalline encrusting matter, ordinary meteors are composed of crystalline elements collected into granules, as is shown by their disaggregation, either by wearing down, or by the use of acids. —*Les Mondes.*

INTELLIGENCE IN ANIMALS.

AN acquaintance of mine, Mr. H., is the possessor of a cat that is a great admirer of birds as an article of food, and he has more than once devoured a chicken, for which he received condign punishment. A short time ago Mr. H. placed a number of eggs under a duck, but the only result was a single, weakly duckling. This my friend laid on the fender before the kitchen fire, and tended the little creature for some time. Eventually he put it into the garden, in order that it might get its own living, and then resumed the sowing of some seeds. On looking round in a few minutes, he saw the cat seize the duckling and dart into the house. Mr. H. followed with the intention of punishing the thief, but he found that the cat had set the duckling in its accustomed place on the kitchen fender, and was caressing and carefully licking some dirt stains from the down of his new friend. Mr. H. frequently afterwards took the duckling into the garden in the presence of some of his neighbours, and the cat invariably carried its little favourite back to the kitchen fire. I think this is a remarkable instance of the suppression of a strong instinct on the part of the cat, and may it not also have learnt a lesson of kindness?

STUDENT.

THE EFFECT OF SEWAGE ON OYSTERS.—Many people have complained of feeling ill after eating what they have afterwards thought were stale oysters. But the microscope has now shown that this has been caused by "germs" present in the liquid of the oyster. These "germs" are similar to those found in sewage hence the deduction that the presence of sewage pipes near oyster beds has a poisonous effect upon the oysters "especially the northern side of Dublin Bay" (Abs. *Chem. Soc. Jour.*—C. A. Cameron). When the oysters were submitted to analysis it was found that the fact asserted by the microscopist was fully corroborated. This now accounts for the reason of people complaining that the oysters were stale.—F. C. S.

Letters to the Editor.

[*The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with their writers. All communications should be as short as possible, consistently with full and clear statements of the writer's meaning.*]

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⁂ All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.

All Letters or Queries to the Editor which require attention in the current issue of KNOWLEDGE, *should reach the Publishing Office not later than the Saturday preceding the day of publication.*

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

Our Correspondence Columns.

VEGETARIANISM.—COMMUNICATION WITH THE MOON.—LIGHT AND LANTERN.—CHOANITES.—THE NAUTILUS.—CELESTIAL MAPS.

[207] Those amiable fanatics, the vegetarians, seem determined to force their way into the columns of KNOWLEDGE; and there, as elsewhere, to insist upon every one, high and low, rich and poor, young and old, adopting their panacea on pain of being branded as idiots for rejecting it. Now, I have no particular faith in the "whole hog" system, and believe that I may eat a cutlet without tomato sauce; and even drink a glass of claret or sherry without imperilling either my longevity, my digestion, or my intellectual powers. May I express a hope, then, that some of those who would send all mankind back (like Nebuchadnezzar) to graze, will listen to what Dr. Wald says, in Carper's *Quarterly Journal*, as quoted by Dr. Drysdale in the *Echo* newspaper. "Some prisoners in a castle at Waltenburg, who had a mortality from 1846 to 1851 of 18 per 1,000, in 1852 had a mortality of 380 per 1,000, the causes of death being diarrhœa, scurvy, and dropsy. Dr. Wald, on investigation, found that, as the potato crop had failed, these prisoners, on theoretical chemical grounds, had been fed principally on white and grey peas and lentils with bread. The mortality increased terribly on this diet, and more leguminous food was supplied, so that in 1854, beans were given five times a week, when the mortality still increased whole troops of the prisoners becoming blind and dropsical. Wald at once dropped this theoretically nutritious food, and gave milk, bread, rice, meat, and coffee, and the normal mortality soon reappeared." I have eaten meat for upwards of forty years, and certainly have not spent twopence in medicine during the last ten of them. Moreover, I will do the hardest day's walking, shooting, rowing, riding, or thinking, against any man whatever of my age who has lived upon potatoes and turnip-tops for an equal time; with the moral certainty that, in sporting slang, I should "beat him off his head."

I have no book of reference at hand, but in connection with letter 206 (p. 233), would say that, unless my memory is more than usually at fault, it was Gruithuisen, of Munich, who proposed to communicate with the Selenites by the erection of the familiar diagram to the 17th proposition of the First Book of Euclid, on some extensive plain or desert on the earth's surface.

Mr. J. W. Stow (query 169, p. 234) should obtain vols. xxxii. and xxxiii. of your contemporary, the *English Mechanic*, running through which he will find a series of articles by Mr. Lewis Wright, under the title of "Optics with the Lantern," which supply the very thing he requires.

Query 171 (p. 234) may be answered by saying that the fossil choanite must have been nearly related to the modern family of Alcyonidæ, to which the common organisms, called "Dead Men's Fingers" by the fishermen, belong. It has no affinity with the sea anemones at all. It is the polypidom of a zoophyte. I have seen a large number of choanites in my time, but "the spiral worm round the body" is something new to me.

Assuming that by "gut" in Query 177 (p. 231) Mr. Webb means the siphuncle of the nautilus, the most plausible explanation yet given of its use is that by Mr. Searles Wood- that, passing right through all the closed and isolated chambers of the shell, it serves to maintain the vitality of the entire organism during the animal's certainly long life.

In answer to Query 181 (p. 234), Middleton's "Celestial Atlas" is, or was, published by Jarrold & Son, of Norwich, and costs, with the "Companion," about 15s. Gall's is, I fancy, published by Gall & Inglis, who are Scottish publishers, I forget whether in Edinburgh or Glasgow. The price of this I have forgotten too.

In "Nights with a three-inch Telescope," in Fig. 5 the smaller star should be at the bottom. In Figs. 6 and 7 the rings of Saturn and the disc of Jupiter are *grey* in the engraving. The rings and all Jupiter's disc not covered by belts should be *white*. The small star beneath ε Persei, also in Fig. 8, is to the *left* of a perpendicular let fall from its primary, instead of slightly to the right of it.

A Fellow of the Royal Astronomical Society.

CHEAP TELESCOPES SUNLIGHT ON FIRES—TEMPERATURE OF INTERSTELLAR SPACE—ICE—TIME OF THE GLACIAL EPOCH HEAT FROM THE STARS.

[208]—If "R. I. P." (query 135, p. 188) will take my advice, he will make no attempt to construct any telescope, cheap or dear, himself. At any rate, five guineas would be the lowest price at which he could obtain anything whatever worth looking through, and that would not be a weariness to the flesh. I presume that by 2½" and 3" he means 2½ and 3 *inches* respectively; but " really signifies seconds of arc, and, legitimately, nothing else whatever.

"X." (query 136, p. 188) may rest assured that it is a mere vulgar superstition that bright sunlight interferes with combustion. It renders flame almost invisible, and so gives an ordinary fire the aspect of going out; but anyone who will take the trouble to shut the shutters will see at once that the seemingly decaying fire is burning as brightly as ever. As for cigars, I can not only always keep one alight, but never, to my recollection, has one gone out, in brilliant sunshine.

In answer to query 139 (p. 188) estimates of the temperature of interstellar space not unnaturally differ considerably. Hopkins's was − 39·5 deg. centigrade (No. 15, "Monthly Notices," Vol. XVII., p. 192, Fownes estimated it at −50 deg. cent., and Pouillet as low as −142 deg. cent. These are all mere deductions from theoretical considerations, as of course the temperature of space has never been actually measured.

One single fact will suffice to show that ice (Query 140) does not vary in volume, as other solids do, with variation of temperature; since it *contracts* during liquefaction.

There can be very little doubt that Dr. Croll's Theory (Query 141) is substantially correct. No other theories exist which are worthy of scientific attention. Adhémar's is worthy of Jules Verne; while Colonel Drayson's well, his is worthy of Adhémar.

It may interest "Student" (Query 161, p. 211) to hear that Mr. E. J. Stone, the present Radcliffe Observer, in 1869 determined the heating effect of Arcturus to amount to 0.0000137 deg. Fahr., about equivalent to that radiated by a 3-inch Leslie's cube of boiling water 400 yards off! Curiously the heat from α Lyræ is *less* than this; in fact, it is only equal to that emitted by a 3-inch cube of boiling water at a distance of 600 yards.

A Fellow of the Royal Astronomical Society.

INTELLIGENCE IN ANIMALS.

[209]—A pony belonging to a gentleman in this neighbourhood showed great unwillingness to carry any one on his back. One day he was being ridden by a young lady; he seemed to be very spirited, and galloped along the drive at a good pace for some distance, when he suddenly stopped, stretched out his neck and threw the lady over his head.

For some time after this he was not ridden, but the fright gradually wore off, and his back was once more called upon to bear the burden, until the following incident occurred, after which I don't know what became of him. He was trotting briskly along the road with his young master on his back, when, on coming to a shallow pond on the right-hand side of the road, he abruptly *turned at right angles*, took a step or two into the pond, then came to a stand, throwing his rider into the centre of it. He stood for a moment and looked at his unseated rider, then trotted quietly home.

I don't know whether this will be considered *reasoning* or not. The pony seems to have thought that if he got off for so long by throwing his rider on the ground, he might get off for a longer time if he threw him in the pond; certainly he must have considered it more disagreeable to be thrown into the pond than on to the ground, seeing he turned into the pond to accomplish it.

Edinburgh. J. B.

ASTRONOMICAL CURIOSITY:—EVOLUTION.

[210]—I have seen somewhere the following paradox, as I suppose I must call it. Granting (1) that the stars are on the average as bright as our sun, (2) that their number is practically infinite, and (3) that light travels without loss (all of which premises appear to me perfectly reasonable); it follows that the sky should *always* be as bright as if full of suns, and square suns, so as to fit closely. For a star's greater distance is compensated by the greater number that can shine near together, and the theory supposes that in any direction we should reach stars sooner or later. This result is, however, so different from reality, that I should be glad to be shown where the fallacy lies. [In our next.—Ed.]

I venture to point out to Mr. Donbavand (Letter 183), first, that biologists should no more be expected to account for the origin of any "first-born animal," than that astronomers should be called upon to point to the origin of the nebulous globe, from which our solar system is derived, or theologians to explain whence came the material from which it was made (since we are told that "created" means "set in order"). Secondly, Pasteur, or any one else, has never disproved the possibility of "spontaneous generation;" all he proved related to his particular experiments, and similar ones. How do we know, for instance, that, *even now*, the process is not going on in our ponds and ditches, side by side, if you like, with the known modes? It is not possible to *disprove* a thing like this, and the *probability* of it must remain a matter of opinion. Thirdly, is it more astonishing that a fish should develope into a reptile by continuous slight modifications, in course of ages, than that a tadpole should become a frog, in the same individual, in a few hours? I think Mr. Donbavand should read Haeckel's "History of Creation," if he still feels interest in the matter.

Lewis Arundel.

LIQUIDS AND THEIR VAPOURS.

[211]—At what Mr. M. Williams has described (see Knowledge, No. 8) as the "critical temperature" of liquids, the mysterious disappearance of all visible distinction between the liquid and its vapour, is much misunderstood by many persons, who imagine that, when this temperature is arrived at, "the whole of the remaining liquid is suddenly converted into vapour."

We know that liquids expand (becoming lighter) as their temperature is raised, and, at the same time, the density (and elastic force) of their vapours increases at a very rapid rate, so that we might reasonably expect that, after a certain increase of temperature, the densities of the liquid and of its vapour would become equalised, and at this point, of course, the eye could no longer perceive any distinction between them. This, then, is "the critical state," which thus loses its "mystery." [Not quite.—Ed.]

No experiments, so far as I am aware, have been instituted with the object of testing this view. But some of those by Cagniard de la Tour (as in the following table) plainly point to the result I have mentioned above as regards "ether" (See Miller's "Chemical Physics," 1867, p. 366).

Temperature Fahrenheit.	Pressure in atmospheres of the vapour of ether. Volume of liquid = 7; " of vapour = 20.	Volume of liquid = 3½. " of vapour = 20.
257°	10·6 atmospheres.	14 atmospheres.
302°	18 "	22·5 "
324°·5	22·2 "	28·5 "
369°·5	37·5 "	42 "

On increase of temperature, as above, = 67°·5 (from 257° to 324°·5), the pressure (in *each* column) is *more* than doubled; whereas, with same increase of temperature = 67°·5 (from 302° to 369°·5), the pressure is *more* than doubled (by 7½° atmosphere) in the first column, while in the second column it is *less* than double pressure by three atmospheres. Hence, the conclusion that in the latter case, *all* the liquid having passed into vapour, the full pressure was not attained; whereas, in the former case, where some of the liquid still remained (though imperceptible), the tension continued to increase from addition of vapour, density of *both* liquid and vapour increasing *equally* with increase of temperature.

It would be desirable, if possible, to ascertain (by spectroscope or transmitted polarised light) if any "molecular change of arrangement or of motion" could be detected in these different states of liquid and gas co-existing at the same density.

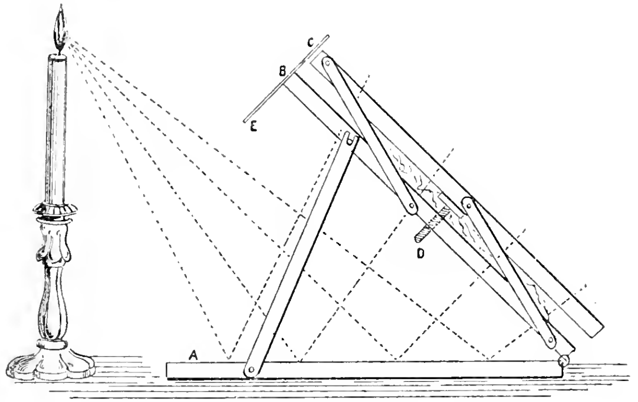
Henry Hudson, M.D.

TATTOO MARKS.

[212]—I have asked myself the question which your correspondent now asks (166), and I have answered myself in this manner:—The atoms inserted by tattooing are quite foreign to those constituting the human body; they do not form and disperse with them, but must be removed as mechanically as they were inserted. They were dead and foreign atoms when introduced, and so remain, uninfluenced by the living electrical changes that surround them. Might I compare them to a boulder thrown into a running stream? If the stream were strong enough, it would carry away the boulder. If an abscess were to occur where the tattooing took place, the powder would be carried away.—J. J. A.

BOTANICAL CONTRIVANCE.

[213]—Those of your readers who are students of botany may find the contrivance, of which I send you a diagram, useful for obtaining a correct outline of the specimens which they wish to draw. It was made for a friend, who wanted a means of copying flowers, &c., the exact size of nature. A is a looking-glass laid flat upon the table, B and C are wooden frames, each holding a square of plain glass. The flower to be drawn is laid between the glasses, which can be kept at any distance apart by means of the parallel links on each side, and the screws at D. A piece of paper is laid upon the upper glass, and by the light of a candle reflected from the mirror, the shadow of the flower is projected through the paper, and its outline can be easily traced. The paper can then be removed, and the shading and colouring copied from the object, which is held in the same position between the glasses. A skilful draughtsman may despise such aid, but it has been found useful for drawings aiming rather at correctness of shape and size than at artistic effect. The shadows will, of course, be very slightly larger than the object. The machine might also be useful to designers of Christmas cards, or floral patterns of any kind. It can easily be made with a common looking-glass and two picture frames, and a few pieces of brass wire. A cardboard screen should be placed at E to prevent the light from falling directly upon either side of the paper. Everything must have a Greek name nowadays, so we call it the SKIAGRAPH.



BOTANICAL CONTRIVANCE.

COLOURS OF STAMENS.

[214]—I have read Mr. Grant Allen's interesting and clever paper in the *Cornhill Magazine*, in which he considers yellow to be the original colour of flowers, founding his theory on the fact that most flowers have yellow stamens. This is scarcely borne out by an examination I have made of twenty-three flowers in a conservatory, of which I found eleven had yellow, seven white, and five red or pinkish stamens. I presume Mr. Allen in speaking of stamens means the filaments and not the anthers only, which are certainly almost always yellow. As Mr. Allen is a contributor to KNOWLEDGE, he may be disposed to give further explanations, and to furnish more details in that periodical. Some orders of plants appear to have nearly always yellow stamens. In others they appear to come near the colour of the throat of the corolla. It is, however, too early in the year to investigate the subject thoroughly.

T. HOWSE.

MARINE BOILERS.

[215]—With reference to "Crankshaft's" letter, 114, p. 167, I can tell him that in marine boilers it is not merely the incrustation of salt which prevents heat passing from the furnace or tubes to the water. As a rule, marine engines are surface condensing, and the grease used for lubricating the cylinders passes with the steam into the condenser, and thence into the boiler. It here, apparently, enters into chemical combination with solids mechanically held in suspension in the water, and deposits this upon the fire-box and tubes in a thin, hard scale. This is a most extraordinary non-conductor of heat—so much so, that I have known boiler-plates almost heated to redness without maintaining the steam higher than 100 lb., the scale being only as thick as one's thumb-nail. If this scale be taken out, pulverised, and rubbed on the hand, and water poured on it, the water will assume a spheroidal shape, and run off without wetting the dust, showing the presence of grease in large quantities.

Another experiment is—spread some of the scale, when moist, upon a tin plate, and place a drop of water on same, and hold the same over a lamp. A very long time will elapse before evaporation takes place. Tilt the plate so that the water runs on to a portion which has not been covered by the scale, and the water will at once evaporate.

The above will perhaps be more readily noticed as occurring with water containing silicate of lime, or lime in some shape.

Letter 115, p. 167.—Undoubtedly there is a tendency to vacuum on the following side of a screw-propeller when revolving; this, however, does not affect the *power* of the steam-engine, but the *speed* of the vessel. The former depends upon the pressure of the steam and the speed of the piston. The latter depends upon the slip of the screw-propeller, *i.e.*, as to whether the propeller is passing a body of water equal to its own pitch. The freer the access for the water to approach the propeller, the greater the quantity of water passed, the less the amount of slip, and the greater the speed of the vessel. Hence, although in designing vessels for screw propulsion it is, of course, necessary to study to a certain extent the form of the bow, it is far more important to have a clean run aft.

Letters 114 and 115 contain questions of deep import to engineers, and you would do great service to the profession if you could publish what information you can obtain upon these points.

Thanks for answers to my former queries on "Gravity." Am not quite sure I agree with one small part, but have not yet had time to go fully into the matter.—Yours truly,

W/g.

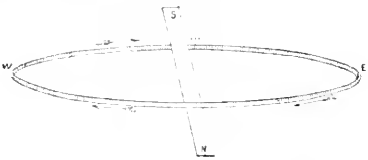
MAGNETIC NEEDLE.

[216]—Referring to query 90, p. 123, to say that a magnetic needle turns to the north, owing to the northern terrestrial mag-

netism being of an opposite name to that magnetism which resides at the north-pointing end of the needle, and, therefore, attraction ensues, would be little to W. H. P.'s purpose. He desires, most probably, to know how the terrestrial magnetism got there.

It seems to me the following view (touched upon in some text-books) is the most satisfactory:—Whenever heat flows in a closed circuit, there is a flow of positive electricity with it. If, therefore, a positive current passes in the direction indicated by the arrows in Fig. 1, the piece of soft iron would become—so long as the heat or electricity circulated—an electro-magnet. If to the upper end, marked S, of the soft iron, we were to present that end of a magnetic needle which pointed southward (geographically speaking), we should find that would be repelled, while the north-pointing end of the needle would be quite violently attracted.

The earth, during her daily rotation, offers successively parts of her surface to the warming action of the sun, and a slight consideration will show that this is practically the same as heat continually passing round the earth in a westerly direction, therefore well representing a closed circuit. We



have then all that is necessary for the state of things above noticed. The wire (Fig.) E.W.w. may well show or represent the surface of the earth exposed to the warming action of the sun (for the current will be in the same direction, viz., E.W.w.), the bar, SN, the iron contained in the earth. This iron becomes converted into a huge electro-magnet. We should then have towards the northern part of the earth the same-named magnetism as at position S in the figure. The behaviour of a needle under these circumstances we have shown; so a needle on the earth will act in precisely the same manner—that is to say, its N-pointing pole turns to the north, because the different-named magnetisms (if I may use the term) attract. Strictly speaking, there is a fourfold force acting, two on either pole of the needle, one pulling, the other pushing, not in the sense of moving merely directive; but, in order to simplify, we have neglected the other three. Their action needs no explanation, for they are all additive to the result.—G. F. J.

THE DESCENT OF MAN.

[217]—Mr. Donbavand's method of criticism, regularly adopted, would cripple scientific inquiry. He asserts the first difficulty of the evolution theory to be its foundation, complaining that it cannot give an exact definition of the period and the form in which the first living organism appeared upon our planet. Now I submit that a similar argument could be advanced, and with equal force, against the fundamental facts and theories of almost every branch of knowledge. The chemist bases most intricate calculations upon a hypothesis of atoms and molecules, of which not one has ever been seen isolated, weighed, or defined. The electrician speaks habitually of magnetic fluids which have never tangibly revealed themselves. No definite origin can be assigned either to molecules or to magnetism. Instances of this kind might be multiplied *ad infinitum.*

Science cannot and does not, as yet, pretend to any knowledge of the beginnings of law, matter, or life. Its present task is to procure and to sift evidence, to arrange and to explain known facts, and thereupon to formulate certain theories which shall enable us, reasoning by analogy, to acquire a more correct and comprehensive view of the subject of inquiry.

Darwin and his followers are stated by the highest authorities to have effected much in this direction. But, in any event, I contend that there is no scientific reproach to their theory in the circumstance that it fails to demonstrate the absolute origin of life. It is no evidence against the continued straightness of a line that our vision cannot extend to its extremities. E. BURKE.

"A GLIMPSE THROUGH THE CORRIDORS OF TIME—LIGHT AND HEAT WAVES."

[218]—A writer in *Nature* calls attention to a paper published by Kant, when he was thirty years of age, in which he states, "If the earth were a perfectly solid mass, without any liquid, the attractions of the sun and moon would not alter the rate of rotation round the axis . . . . If, however, the mass of a planet includes a considerable amount of liquid, the attraction of the sun and moon, by moving this liquid, impress upon the earth a part of the vibrations thus produced. The earth is in this condition." He then goes on (says the writer) to state that the moon produces the greatest effect and the tide running round the earth in a direction opposed to that of rotation. "We have here a cause on which we can count with certainty, incessantly reducing this rotation by as much as it may be capable of." . . . "When the earth steadily draws nearer and nearer to the end of its rotation, this period of change will be completed when its surface is, relatively to the moon, at rest, *i.e.*, when it rotates round its axis in the same time in which the moon revolves round it, and will, consequently, always show the same face to the moon. . . . If the earth were entirely fluid, the attraction of the moon would very soon reduce its rotation to this minimum. Herein we at once see a cause why the moon always shows the same face to the earth. . . . From this we may conclude with certainty that when the moon was originally formed and still fluid, the attraction of the earth must in the manner above described have reduced the speed of rotation, which then, in all probability was greater, to the present measured limit."

The writer considers that Kant had given a glimpse through the corridor of time a century earlier than any of the authorities mentioned by Professor Ball.

Do light and heat travel at the same pace? Do the waves of light and heat coincide? which, I suppose, may be only another way of asking the same question.—A. T. C.

[Light waves are, for the most part, heat waves, and *vice versâ,* though the luminous effect of different ether waves is not proportional to the heating effect. Of such waves we may say that they do not coincide, being identical. The same things cannot coincide, any more than a horse can run a dead heat with himself.—ED.]

PLATING ALKALOIDS.

[219]—Mr. Lewis Arandel has failed to answer the point in query 152 that I wanted elucidating, viz., how to plate on iron. Having tried silvering on iron in the wet way, first coating with copper, and failed in this, I am anxious to try nickel. In Supplement II., Watts' "Dic. Chem.," the neatest method of nickel-plating on copper and brass is given. With regard to the letter on alkaloids, Watson Smith (Bericht's "Deut. Chem. Gesellschaft," xii., 1,420) gives antimony or bismuth trichloride as a test for several alkaloids, including aconitine, which gives a bronze-brown colour. Again, last year (Thresh gives in "Pharm. J. Trans." [3], X., 809, a method of estimating the quantity of this, and of other alkaloids present, by precipitation with iodide of potassium and bismuth.—C. T. B.

THE HEALTH OF NAVVIES.

[220]—After reading your abstract from the *Times* on the above, it occurred to me that M. Colin might now very advantageously try what the effect the betel nut would have in warding off the malarias, &c., contracted in marshy grounds. I think Johnson, in his "Chemistry of Common Life," informs us that the betel chewer, whether native or stranger, has an invaluable treasure in the use of this narcotic, which is a perfect safeguard against fevers, agues, and all other maladies incidental to marsh life. If this is the case, is it not worth a trial during the extensive canalisation soon to be done in France? M. Colin, should he try it, would be able to see if the effect of betel by itself is the same as when chewed with the betel pepper leaf, and quicklime. F. C. S.

SCIENTIFIC PARADOX.

[221]—The difficulty of "Eclecticus" (p. 202) may possibly arise from his conception of the manner in which the pressure of a gas or vapour arises. Liquids have a tendency to give off vapour to a greater or less extent. This tendency varies with different liquids, and in the case of the same liquid, varies with differences of temperature. But vapours have a tendency to condense into the liquid form dependent upon their nature and the conditions to which they are exposed. When these two antagonistic influences are in equilibrium, a state of stability arises. Now, although there may be an atmosphere of ether-vapour present, water will continue to evaporate either until the tendency of the water to vaporise is counterbalanced by the tendency of the water-vapour to liquefy, or until no liquid water remains. It is evident the ether-vapour takes no part in producing this equilibrium, merely retarding the formation of vapour, and going to increase the total pressure.

It may be added that the total pressure exerted in the case of the vapours of ether, alcohol, and water is very much less than the sum of their individual tensions. This only holds good for liquids which do not dissolve in one another. W. S. C.

ALUMINIUM.

[222]—With reference to query 157, I give the following method of extracting aluminium, as it may be of general interest to your readers of KNOWLEDGE :—

A mixture of ground aluminous clay (ordinary clay, but of a good quality, and soda ash (carbonate of sodium) are heated in a furnace, aluminate of soda and silico-aluminate of soda being formed. The fused mass is then broken into pieces and thrown into an iron tank containing water; the mass is frequently stirred, and finally allowed to settle.

The aluminate of soda (being soluble in water) is dissolved, while the silico-aluminate of soda (being insoluble in water, sinks to the bottom of the tank, with any peroxide of iron that may be present in the clay.

The liquid is then drawn off, and carbonic acid gas passed through the solution. This decomposes the aluminate of soda, forming carbonate of soda and pure alumina hydrate. Thus :—

$$\underbrace{2\,AlNaO_2}_{\text{Aluminate of Soda.}} + \underbrace{CO_2}_{\text{Carbonic Acid Gas.}} + \underbrace{3\,H_2O}_{\text{Water.}} = \underbrace{Na_2CO_3}_{\text{Carbonate of Soda.}} + \underbrace{2\,Al\,(OH)_3}_{\text{Alumina Hydrate.}}$$

The alumina hydrate is then dried, mixed with chloride of sodium (common salt) and charcoal or coke, and formed into balls about the size of an orange. These balls are then taken and put into a vertical earthen retort, and heated to redness; then a stream of chlorine gas is passed through them. The chlorine combines with the alumina (being greatly helped by the charcoal), and forms chloride of aluminium, which unites with the sodium chloride (common salt), and distils over or sublimes as double chloride of aluminium and sodium thus :—

$$\underbrace{Al_2O_3}_{\text{Alumina}} + \underbrace{2\,NaCl}_{\text{Common Salt.}} + \underbrace{3\,C}_{\text{Coke.}} + \underbrace{3\,Cl_2}_{\text{Chlorine}} = \underbrace{3\,CO}_{\text{Carbonic Oxide Gas.}} + \underbrace{2\,AlNaCl_4}_{\text{Double Chloride of Aluminium and Sodium.}}$$

Ten parts of the double chloride of aluminium and sodium thus formed is mixed with 5 parts of kryolite (a double fluoride of aluminium and sodium, found in Greenland), which serve as a flux, both in a state of fine powder, and to this mixture is added 2 parts of (metallic) sodium in small pieces.

The whole is now introduced on to the hearth of a reverberatory furnace, previously heated to the required degree, when a violent reaction ensues. The dampers are then closed, and all parts of the furnace kept as close as possible, to prevent access of air. This causes the mass to completely fuse. When the action has subsided and the decomposition is completed, the furnace is tapped, and the metal and slag are run into suitable moulds. Most of the aluminium collects in the bottom of the mould. Above this are two layers of slag, the top layer being sodium chloride (common salt), the middle layer being less fusible than the top, and consisting chiefly of fluoride of aluminium, in which small globules of aluminium are mechanically held, which are recovered by pulverisation and sifting of the slag.

The following equation will show the reaction that takes place in the above reduction. Thus :—

$$\underbrace{AlNaCl_4}_{\text{Double Chloride of Aluminium and Sodium.}} + \underbrace{AlNaF_3}_{\text{Kryolite, or Cryolite.}} + \underbrace{6\,Na}_{\text{Sodium.}} = \underbrace{2\,Al}_{\text{Aluminium.}} + \underbrace{4\,NaCl}_{\text{Common Salt.}} + \underbrace{4\,NaF}_{\text{Fluoride of Aluminium.}}$$

There are other methods, but this gives the purest metal, and is one generally used in England and France. Messrs. J. Lowthian Bell & Co. (or Messrs. Bell Bros.), at Washington, near Newcastle-on-Tyne, manufactured aluminium on a large scale for several years, but gave it up a few years since, owing to it not paying so well as was first anticipated, and also on account of the limited demand for the metal. I think they used the above method.

Gateshead. G. W. GRAY.

DESCENT AND DARWINISM.—VEGETARIANISM.

[223]—I wish to protest against the large amount of space M. Doubavand has taken up in stating his ideas. He says that he sometimes ventures on the labour of thinking. May I venture to suggest that before he again trespasses on your space, he should think over what he means to say, and the shortest and clearest method of saying it ?

E. M. (letter 170) argues that because uncooked diseased meat is apt to produce tapeworm and trichinosis, therefore we should abstain from animal food altogether. Does he not see that his own argument might be turned with equal force against himself ? Because uncooked unripe fruit, if eaten at all, and all uncooked fruit, if eaten in quantity, is likely to produce diarrhœa, and so forth, therefore abstain from vegetable food altogether. EXON.

Queries.

[185]—COLD SATURATED SOLUTIONS.—Can you inform us as to the best method of making and of ascertaining the quantity of salt present in a saturated solution at 60° F ? We have tried a number of experiments, none of which are satisfactory. F. GAUBERT and C. A. SEYLER.

[186]—THE CALCULUS. Is a knowledge of the differential and integral calculus gained from, say, Boucharlat's or Hall's treatises sufficient to begin studying the Mécanique Céleste ?—TRIA. [It would be far from sufficient. A complete course of study of the higher mathematics must intervene, unless you are a born analyst. ED.]

[187]—FLAME. What are the modifications of the ordinary theory of a gas or candle flame introduced by (I think) Frankland ? Perhaps you could sometimes give us an article or two on flame, explaining why one kind of burner is better than another. I take it that for a given expenditure of gas, heat + light is constant. Is there a theoretical maximum to light, and what principles require attention in our attempts to attain it ? —L.

[188]—INTRA-MERCURIAL PLANET.—Leverrier formed a theory of an intra-mercurial planet, for which he surmised some three or four orbital periods. Oblige by informing me exactly what these periods were. I do not refer to Lescarbault's Vulcan, but to the prior hypotheses and periods of theory.—JOHN JONES. [The only periods that Leverrier dealt with, to the best of my belief, were calculated after Lescarbault's Vulcan was announced, though Leverrier attempted to reconcile prior supposed observations of planets in transit across the sun's face. —ED.]

[189]—EYE-PIECE.—Can you please give me an easy way to find the power of an eye-piece of 2 plano-convex lenses, both ½ inch focus, and sliding one into the other, at a distance from one another, say ¼ inch and ½ inch ? I want this information, so I may know the power of my telescope.—J. W. C. [The lenses should be set ½ inch apart. The power is then four times that of either lens alone.—ED.]

[190]—THE PRESERVATION OF SERTULARIAS, ZOOPHYTES, SEAWEEDS, &c.—Can the Editor of KNOWLEDGE, or a correspondent, inform me of a chemical preparation to preserve zoophytes, sertularias, and seaweeds, &c. ? and is there a book on the subject, and on British and foreign seaweeds, &c. ?—M. A. S.

[191]—ANIMAL LANGUAGE.—Do not the habits of swallows, prairie-dogs, &c., seem to prove that animals have some sort of language by which they can make themselves understood at least to members of their own family ? ARACHNIDA.

[192] THE ICE-AGE IN BRITAIN.—What proofs are there at the present time that there ever existed an ice-age in Britain ?—ARACHNIDA.

[193]—ACTION OF THUNDER-STORMS.—Why are beer and milk so frequently turned sour during a thunder-storm, although they will often stand as high a temperature without being affected when there is no thunder in the air?—F. A. S.

[194]—GRAVITY.—The editorial reply to query 114 is incomprehensible, if correct. If any force, however small, will overcome the inertia of any mass, however great, what then becomes of the law, that inertia is proportional to mass? I wish to know what force is necessary to overcome the inertia of one ton weight in a direction at right angles to the force of gravity ?—ZARES.—[We repeat, any force, however small. If a ton of matter were suspended as you described, or placed on a perfectly smooth table, the breath of a child would overcome its inertia. The velocity communicated to it would be very small, but you did not say how much velocity you wanted. ED.]

Replies to Queries.

[61]—STAR NUMBERS AND LETTERS.—"A Fellow of the Royal Astronomical Society" says (p. 164, No. 8): "Stars . . . . . are both lettered and numbered in their order of Right Ascension." There should be a comma in this reply, after the word "lettered." The stars are numbered in the order of their right ascension, and Bayer lettered them roughly in order of their brightness. This answers a query by "Winter."—ED.

[71]—NAMES OF FLOWERS. I would advise Querist 71, in sixth KNOWLEDGE, to get Bentham's "British Flora." He will find it everything he desires. Should he fail to get at the right name of a flower by means of this excellent work, I should then recommend him to send a perfect specimen of the "stranger" to the editor of

KNOWLEDGE, who, I am sure, will not consider it too much trouble to assist amateur botanists by giving names of flowers when requested to do so. J. E. GEORGE. We are not personally botanical, worth mentioning - better trust in the book. Though we would forward specimen to botanist. ED.

[130] KNOWN LANGUAGES. Horalacque calculates 199 language - how many more have been made *known* since he wrote I do now know. G. T. HARRAP.

[133] In addition to "Pangul's" references, "Querist" will find several of Mr. Dollinger's papers in the *Monthly Microscopical Journal*, vols. X., XI., XII., XIII., XIV., XVI., XVIII. B. J. AUSTIN.

146 CHEMICAL.- The reason why the O. of the air is utilised in the body, while the N. is rejected, is that the former is essential to those transformations which convert the potential energy of certain constituents of food into actual energy, while the latter is useless for that purpose. Fats and other carbohydrates, when taken into the body, furnish, by their oxidation, the actual energy which is in part transmuted into muscular power; and it is to this process of oxidation that animal heat is due. There being no other available source of O., the amount necessary for the said oxidation of the carbohydrates used as food must come from the inhaled air. It is only the heat and muscular-power-producing constituents of food - the carbohydrates - that get oxidised in the body, the nitrogenous or flesh-forming constituents, together with the phosphates and carbonates, being capable of performing their functions without undergoing the process of oxidation. Now, as the products of oxidation, CO_2 and OH_2, are exhaled, there cannot remain in the body anything that can be termed "food already oxidised." In fact, the utility of carbohydrates does not consist in their adding anything to the structure of the body, but in their producing muscular power and heat when being transformed into the gases exhaled in the breath. In starch and sugar, the O. and H. exist in the same proportions as they do in OH_2, and, as the whole of the O. and H. of those carbohydrates leave the body as OH_2, all the carbon is in a position to be oxidised by the inhaled O. of the air, and is, consequently, available to produce its full amount of force and heat.— E. M. D.

[157]—ALUMINIUM, page 211.—Cryolite (a fluoride of sodium and aluminium, which occurs in great abundance in Greenland) is powdered, mixed with half its weight of common salt, and either placed in alternate layers with two parts of sodium in a crucible, or roasted with the same quantity of sodium in a furnace. The chief advantage in using cryolite is that the costly and troublesome process of preparing the double chloride of aluminium and sodium is thus dispensed with. There was a process patented in Germany a short time ago, the main point in which consisted in fusing the sulphide of aluminium with iron; but surely other chemists thought of this process before: I did three years ago.—A GREENOCK STUDENT.

Answers to Correspondents.

**** All communications for the Editor requiring early attention should reach the Office on or before the Saturday preceding the current issue of* KNOWLEDGE, *the increasing circulation of which compels us to go to press early in the week.*

HINTS TO CORRESPONDENTS. 1. *No questions asking for scientific information can be answered through the post.* 2. *Letters sent to the Editor for correspondents cannot be forwarded; nor can the names or addresses of correspondents be given in answer to private inquiries.* 3. *No queries or replies savouring of the nature of advertisements can be inserted.* 4. *Letters, queries, and replies are inserted, unless contrary to Rule 3, free of charge.* 5. *Correspondents should write on one side only of the paper, and put drawings on a separate leaf.* 6. *Each letter, query, or reply should have a title, and in replying to letters or queries, reference should be made to the number of letter or query, the page on which it appears, and its title.*

J. CARGILL. Thanks for encouraging words. Your student friend who thinks we are too abstruse for the masses and too popular for scientific students is right enough; that is what we have aimed at. But he is unwise in calling it a fault. We do not write for science students, though among those who write for us are some who can teach the teachers of science students. Nor do we write for those who neither think nor care to think. J. D. M. I have never seen unusual feats of strength done under mesmeric influence, but Dr. Carpenter gives instances of the kind. They illustrate the influence of imagination, but they have not yet been explained.—J. WILLSON. Lardner's works are not absolutely out of date, but they would suit better one who was well up in all the modern views, so as to be able to distinguish at once what is obsolete.—J. PRESTON BATTERSBY. Many thanks. Your notes are very interesting. I return them as you desire. I think you are mistaken in imagining there is any prejudice about so-called mesmeric phenomena. They are treated very fairly by some of our most eminent physiologists.—M. If, knowing what the planets are, their distances, &c., the slightest reason for conceiving that they can influence our weather could be shown, it might be worth while to make such observations as you suggest; but none can be imagined. BRENTON. You are quite right; T. R.'s solution may be made by embodying his demonstration of the property on which it depends to all intents and purposes a solution within the limits of Books I. and II.; but with the extra matter you show to be necessary to complete it, it contrasts very unfavourably with either of the other proofs depending on books III. and VI.—ZARES. *What* series 0 to 1 do you mean? M. J. HARDING. Orion, on the cover of my "Easy Star Lessons," is "quite too utterly awful." I had nothing to do with it. It is the binder's conceit. Trying to correct it would be useless. In the present position of science, a work "containing a most exhaustive refutation of the Darwinian theory" must either be a very wonderful performance (as you say), or very bad. Science has not been profoundly stirred by the book. The sub-title seems open to objection, to say the least, as suggesting that the Newton of Biology is a sort of "devil's advocate."— A. G. P. We misread your letter; but our reply was not meant as you suppose. The absence of centrifugal force would produce a very slight but measurable increase of weight. W. A. C. We have no objection; you may prefer a dog to your nearest and dearest; you must allow us, though we like Rover very much, to prefer our dear ones of human sort. To be consistent, you ought not to breathe, for, at every breath, you destroy multitudes of "creatures belonging to races below us." —WINTER. Yes; he probably smiled: but "he laughs longest who laughs last."—VEGA. Yes; but the point really is whether the classification adopted by Owen is essentially wrong. For convenience, the other may be better; but it is a merely arbitrary distinction.- H. D. P. Not about Sorghum just now; so many things wanted and promised. Sorry am unable.—GRADATIM. The loxodromic curve approaches the pole by an infinite series of circlings, though the total distance is finite, each whorl bearing to the previous one a ratio less than unity. Thus your whole question involves a paradox, as no ship could follow such a course; no wonder no ship could leave the Pole on the loxodromic spiral. Remembering that the stereographic projection of the loxodromic curve on the equatorial plane with South Pole as pole of projection is an equi-angular spiral, and that this curve approaches the pole asymptotically, you will find no difficulty in interpreting your paradox.—B. DONBAVAND. Your letters at least five times too long for admission.—J. P. SANDLANDS. South cone is up, means that this weather signal is hoisted—viz., a cone with its point downwards. Mrs. Kingsford says the teeth of man indicate that he is a frugivorous animal; others regard them as showing him to be omnivorous. As a matter of fact, he is omnivorous, whatever his dental formula may be. The other query seems answered by what is stated in our review of Mrs. Kingsford's book. —ONE INTERESTED IN "KNOWLEDGE." No one asks you to believe that man is descended from monkeys—only that he is related to them; so that what you believe is closely akin to the scientific faith, for you think monkeys descended from men. The tone of your remark that "no theory will make you believe" so and so, is unscientific in the extreme. The student of science is prepared to believe whatever facts may prove.- J. R. See p. 170, where precisely such a problem is solved.—PETER KNOWLES. I return the twelve stamps. If I "published" the work you name I would send it, but I do not publish that or any work. I did not even write it, I only revised it. If I remember rightly the publisher is Baillière, King William-street, Strand. URSA MAJOR. A lunar rainbow is caused precisely as a solar rainbow is caused, only that the light comes from the moon instead of the sun. V. C. C. D. I do not see how reasoning so sound and moderate as that you quote can be answered. I should imagine few evolutionists would be so extreme as those who seem to be disposed of by this reasoning.— G. T. HARRAP. You "disagree with vaccination," but your letter would disagree with our readers. SALUTO. Yes, microscopy is as desirable in our pages as telescopy.- F. C. S. Many thanks: marked for insertion.- J. B. More rapid respiration, increasing the combustion, seems to account for the difference.—G. S. BODKIN. We point out mistakes (where we see them) frankly and without apology, because that is doing to others what we wish them to do to us.—J. MCNEILL. In issuing monthly parts, the publishers are endeavouring to meet the wishes of the public; when I note that reprints have been necessary to make up the first two monthly numbers, you will understand their price. The reprint of a newspaper always means loss of money, and the publishers have reprinted (setting up every line of contents and advertisements afresh) in some cases twice; and made up sets out of the back numbers which had been intended for volumes. Many thanks for good wishes.—C. J. BROWN. Gravity vanishes at the earth's centre, but pressure is there at its maximum. Apart from a very slight diminution, due to the greater or less rigidity at

different parts of the earth's interior, the pressure at any point within the earth is proportional to the total quantity of matter above that point, just as in the sea, where, though gravity is appreciably constant, pressure is proportional to the depth. If you ask where the force of gravity is at a maximum, I cannot answer, because I do not know according to what law density increases towards the centre. As to the other point, our circulation increased so that it became quite impossible to continue cutting the edges without unduly delaying publication. But we more than make up for that in other ways. In binding there would in any case have had to be another cutting, and most of our readers, we believe, intend to bind each volume as it is completed.—H. Fear cannot find space for paper on colours. Readers complain unless a certain proportion of space is given to original matter, and we cannot increase the total space beyond a certain range. Correspondents must not suppose we do not value letters which we are obliged to omit.—J. E. SHEPHARD. The writer of the article in question was not likely to consider the use of aconitine in homœopathic practice. It is seldom used by allopathic physicians, except as an external application. A GREENOCK STUDENT. Clouds are formed by the condensation of aqueous vapour into small water drops, which, however, do not fall rapidly like rain drops. It can be shown that the smaller a water drop, the slower its descent through still air. However, a cloud is also constantly changing, as you suggest. The light, feathery clouds consist of particles of ice or fine snow. Colonel Ross promised other papers on blowpipe analysis, but has apparently not yet been able to find time to write them. The blowpipes made on his plan will be rusting for want of use. TREBOR ROLAT. Short whist, played without counting honours, is, to all intents and purposes, the ordinary game with the element of chance as far as possible eliminated. It is in my opinion a much better game even than that which Clay thought would be perfect, in which the honours are counted at half the usual rate, one point for two by honours, two for all four honours. After playing the game for any length of time without counting honours, a real lover of whist feels something like shame in counting honours, or even half honours. At present we fear the star chart cannot be issued with the last week of each month, as we want one chart to appear each month. As soon as we can, the change shall be thought of. Have no space, for some time, for article on relation between mind and physical force.—DOUG. BRODIE. Your 2½ stamps received, but no paper on the Tides has yet appeared; we did not promise it for an early date, and if we had, we could hardly have kept our promise.—JAMES DEAS. Never heard before that the Star in the East was thought by any one to be the variable Mira. That star has been said to be a comet, a conjunction of planets, and a temporary meteor. Astronomy has nothing to say to any of these ideas.—S. DE MERVIGNE. The passages you quote show that where the conditions remain the same, races change little or not at all. What is there in the theory of evolution to suggest that they should change a great deal?—F. F. Your suggestion about the use of Jacob's ladder at fires seems excellent, but space should be found for it elsewhere. It does not belong so much to our subjects, as to those treated of in weekly newspapers or mechanical papers.—VIGNOLES. Many thanks; but your square has already been given.—ROBERTUS. We do not know the name of the secretary of the Society for Promoting the Abolition of Capital Punishment.—E. M. Not knowing the tables that you refer to, cannot tell precisely how the discrepancies arise. The date of your tables is so remote you could scarcely expect calculations made by them to correspond with the recent ones of the Nautical Almanack. We shall give such a map as you mention, relating to the transit of Venus. You must not expect to find in the "Nautical Almanack" all that is necessary to calculate phenomena. I had to work many hours and many days to get the results on which I based my investigations of the transits of 1871 and 1882.—J. N. DEVINE. Of course you are right. Our mistake was a natural one; but it was unquestionably a mistake. We have altered it, noting the error.—MEMBER OF THE ARCHÆOLOGICAL SOCIETY. Thanks; but it would be an obligation if you could briefly sketch for readers of KNOWLEDGE the essential ideas of Naville's book. The subject is a curious one, a little out of my own personal line of studies, but full of interest, and I am sure readers would be interested.

MR. W. H. H. HUDSON, late Fellow and Lecturer of St. John's College, Cambridge (Third Wrangler in 1861), succeeds Mr. Drew as Professor of Mathematics at King's College, London.

Notes on Art and Science.

FERMENTATION IN BEER.—"In Re" is evidently unaware of the lengthy explanation which his question involves. In the first place, different brewers have different ways of starting fermentations. The Germans, and a few English, mix their yeast with a little of the "wort" first, and then add the rest. The Japanese use rice must for starting. Then the "pitching" variations during process of specific gravities and temperature must be considered, also germs generated in fermenting healthy and unhealthy the preparation of the wort and yeast; in fact, I should advise "In Re" to get a book on brewing and read it through, for it will *all* be interesting, and will lead him up to his subject. If Dr. Graham, Professor at University College, London, has published his lectures delivered on the subject, I can only say that "In Re" will have a book written by one who has mastered thoroughly the whole subject of brewing. Mr. T. A. Tooley, B.Sc., F.C.S., editor *Brewers' Guardian*, has his articles on Brewing and Distilling, written for the "British Manufacturing Industries," published. His name needs no comment.—F.C.S.

THE EXPANSION OF WATER BY HEAT.—Herr P. Volkmann has in the *Annalen für Physik und Chemie* compiled the results of Hagen, Matthiessen, Pierre, Kopp, and Jolly, on the expansion of water, and has obtained the following mean results for the volume and density of water at various temperatures:—

Temp.	Volume.	Density.	Temp.	Volume.
0 degr. C.	1·000122	0·999878	15 degr. C.	1·000847
1 ,,	1·000067	0·999933	20 ,,	1·001731
2 ,,	1·000028	0·999972	25 ,,	1·002868
3 ,,	1·000007	0·999993	30 ,,	1·004250
4 ,,	1·000000	1·000000	40 ,,	1·007700
5 ,,	1·000008	0·999992	50 ,,	1·011970
6 ,,	1·000031	0·999969	60 ,,	1·016910
7 ,,	1·000067	0·999933	70 ,,	1·022610
8 ,,	1·000118	0·999882	80 ,,	1·028910
9 ,,	1·000181	0·999819	90 ,,	1·035710
10 ,,	1·000261	0·999739	100 ,,	1·043230

—*Scientific American.*

SUNDAY LECTURE SOCIETY.—In the course of his lecture on "The Heart and its Work," on a recent Sunday, at St. George's Hall, Langham-place, Dr. Andrew Wilson gave some interesting details regarding the action of the central engine of the circulation. The heart, he showed, was merely "a hollow muscle," and the force which drives blood through our bodies is therefore similar to that wherewith we move our fingers in writing, or our legs in walking. Dr. Wilson showed also that the work of a man's heart in 24 hours amounted to 124 ft. tons; *i.e.*, if the heart's force for 24 hours were gathered into one stroke or lift, it would suffice to lift 124 tons 1 ft. high. The heart is ruled by at least three sets of nerves. In its own substance, there are masses of nerve-matter, or *ganglia*, which carry on the normal work of the organ. Then, secondly, there exists an important *vagus* nerve, which checks or slows the heart's action, as under the influence of fear or other mental emotion; and there is, thirdly, a *sympathetic nerve*, which causes the heart's action to increase in rapidity. The varying emotions of the mind thus influence the heart for good or for evil by disturbing its normal action.

NO ORGANIC MATTER IN METEORS.—A Louisville (Ky.) paper reports an interview with Prof. J. Lawrence Smith, of that city, in the course of which Mr. Smith gave reasons for discrediting the discovery of organic substances in meteors, as claimed by Prof. Hahn, of Berlin. Mr. Smith said:—"Although I have probably examined more microscopic plates of fragments of meteorites than any other person, still I have never discovered anything like organic remains in any of them. Besides, the well-known chemical composition of these bodies is averse to the existence of any such remains as spoken of by Prof. Hahn. Were these remains present, we should discern carbonate of lime in their interior. The two or three that have any carbonate of lime were discovered and analysed by myself, and in these cases the carbonate of lime was an accidental constituent of incrustation deposited on the surface after their fall. In the microscopic examination of these polished plates of meteorites, the two predominating minerals, enstatite and bronzite, will, by their fissures and forms, sometimes remind one of vegetable and other organic forms, but the merest tyro of an observer will trace here nothing but a rare resemblance. And furthermore, the nature of these minerals precludes the possibility of organic remains even in terrestrial minerals of similar kind. Not knowing of any eminent German geologist named Prof. Hahn, I thought it but reasonable and logical that I should inquire something about him from my friend Prof. Hawes, now in the employ of

the Smithsonian Institution, and the best lithological microscopist in this country, and who recently returned to this country after ten years' study with Prof. Rosenbaum and others into the microscopic character of rock. In answer to my inquiries, Prof. Hawes wrote me this letter:—"I read that paper of Prof. Hahn's. He is a kind of half insane man, whose imagination has run wild with him. These forms which he so accurately describes and figures have long been known to exist in meteorites, and have been frequently described by mineralogists and microscopists. They are mainly composed of enstatite or bronzite in radial forms, and fractured in such a peculiar manner as to give them the appearance of structure. Some of the American meteorites which I have examined show these forms in great beauty, but Prof. Hahn is the only man who has seen anything organic in them, and his paper has excited nothing but ridicule. It reminds one of the long and laborious research of a German professor who found a whole flora and fauna which he named with double Latin names, and which he found in his microscopic examination of basalt. It is very clear to my mind," continued the Professor, "that these cranky observations, viewed with the spectacles of the imagination of Prof. Hahn, have obtained more publicity than they merit."—*Scientific American.*

Our Mathematical Column.

MATHEMATICAL QUERIES.

[15]—ARITHMETICAL PROBLEM.—A and B have to build a wall one hundred yards long; A is to have five shillings a yard more than B. When the wall is finished A and B each receive £50. How many yards did each do, and at what rate per yard?—HAMILTON STUART.

[This leads to an indeterminate equation. Thus, if x be number of yards built by A, y the number of shillings received per yard by B, we have $x(y+5)+y(100-x)=2{,}000$; or $x+20y=100$.—ED.]

[16] In an old volume of problems, I came across the following: A messenger, M, starts from A to B (a distance a miles), at a rate of v miles per hour, but before he arrives at B, a shower of rain commences at A, and at all places occupying a certain distance, z, towards, but not reaching beyond, B, and moves at the rate of u miles an hour towards A. If M be caught in the shower, he will have to wait until it is over. He is also to receive for his errand a number of shillings inversely proportional to the time occupied in it, at the rate of n shillings for one hour. Supposing the distance, z, to be unknown, as also the time at which the shower commenced, but all events to be equally probable, show that the value of M's expectations, in shillings, is—

$$\frac{nv}{u}\left[\frac{1}{2}-\frac{u}{v}+\frac{u(u+v)}{v^2}\log.\frac{u+v}{u}\right]$$

Can you or one of your readers kindly solve this problem for me without using the differential calculus?—NO ANALYST. [We will leave this problem for a week. "No Analyst" has set us a hard one.—ED.]

[17]—TORTOISE PROBLEM. A hare and tortoise have a race, the hare gives the tortoise 100 yards start and runs ten times as fast; they start together, but while the hare runs the 100 yards, the tortoise has crawled ten yards; while the hare is running the ten yards the tortoise crawls one yard; while the hare runs that one yard the tortoise has advanced $\frac{1}{10}$ of a yard; and so on, *ad infinitum*, the hare, mathematically, never overtaking the tortoise.—TORTOISE. [The paradox was known to the Greeks as the problem of Achilles and the Tortoise. The explanation is that the distance run by either animal is divided into an infinite number of parts, forming a geometrical series, having a finite sum. Thus the hare runs 100 yards $+10+1+\&c. = 100\div(1-\frac{1}{10}) = 100\left(\frac{10}{9}\right) = 111\frac{1}{9}$ yards. The tortoise crawls $11\frac{1}{9}$ yards. Dividing up a finite distance into an infinite number of parts does not make it infinite.—ED.]

[18]—ARITHMETICAL QUESTION.—A man having a cask containing 300 gallons of wine takes out one gallon per day, putting in a gallon of water to refill the cask each day. After how many days will the mixture in the cask be half water and half wine.—YOUNG BEGINNER. —[Y. B. asks whether this problem can be solved without the aid of logarithms. Not readily. At end of one day the mixture is $\frac{299}{300}$ths wine, at end of second it is $\frac{299}{300}$ths of $\frac{299}{300}$ths wine or $\left(\frac{299}{300}\right)^2$ wine, and so on. If in x days the mixture in the cask is half water and half wine, we have $\left(\frac{299}{300}\right)^x=\frac{1}{2}$; or taking logarithms of reciprocals of each side, we have $x(\log.300-\log.299)=\log.2$, or $14501x=3010300$, $x=\frac{3010300}{14501}=207.59$; or up to the 208th day the mixture would contain more wine than water, after the 208th day it would contain more water than wine.]

A. M. R. points out a mistake on p. 191, where we put

$$1=(\sqrt{1+x}+x)(\sqrt{1+x}-x)$$

Of course, this relation does not hold; and the solution fails. We had found 0 to be the only root which could be obtained without a solution for cubic or biquadratic, and as the solution, after above mistake, led to root 0, we did not (as we should have done), run through the steps. The equation obtained just before the mistake occurs is

$$1=\sqrt{1-x}\,(\sqrt{1+x}+x)$$

this will be found to give $x=0$ and the biquadratic

$$x^4-4x^3+4x^2+4x+4=0.$$

"T. R." considers that the proposition $DM^2=AM.MB$, first figure, p. 211 (ADB a right-angled triangle, and DM perp. to AB), may be established without the use of books of Euclid beyond I. and II. Thus (we rather abridge his proof):

$$AM^2+MB^2+2AM.MB=AB^2=AD^2+DB^2=AM^2+MB^2+2MD^2$$
$$\therefore\ AM.MB=MD^2$$

We did not say otherwise. We said it neither is in those books, nor can be given as a *corollary* on any propositions they contain. Several of the propositions in Book III. can be as easily proved from Book I., but they are thrown, for convenience, into a separate book.

[19] SUM OF SQUARES. I do not know if the following simple method of getting at the formula for $1^2+2^2+3^2+4^2 \ldots\ldots + n^2$, is new or not, but I believe it is not the usual one, and so send it on the chance of your thinking it worth publishing:—

$$\begin{array}{ccccccccccccc}
 & 1 & 2 & 3 & 4 & 5 & . & . & . & . & . & n & A\\
 & 1 & 2 & 3 & 4 & 5 & . & . & . & . & . & n & \\
 & 1 & 2 & 3 & 4 & 5 & . & . & . & . & . & n & \\
C & 1 & 2 & 3 & 4 & 5 & . & . & . & . & . & n & B\\
 & 1 & 2 & 3 & 4 & 5 & . & . & . & . & . & n & \\
 & . & . & . & . & . & . & . & . & . & . & &
\end{array}$$

Here the horizontal line cuts off the series $\frac{n}{2}(n+1)$, and the zigzag line divides the figures forming the squares from the unnecessary ones below. The sum of the squares equals $A+B$.

Now, $B=2C$, as is readily seen from the above diagram. [Every vertical column in B sums up to twice the sum of a horizontal row in C.—ED.] And

$$B+C=(n-1)\times\frac{n}{2}(n+1)$$
$$\therefore\quad B=\frac{2}{3}(n-1)\times\frac{n}{2}(n+1)$$
$$=\frac{n}{3}(n-1)(n+1)$$
$$\therefore\quad A+B=\left(\frac{n}{2}+\frac{n}{3}(n-1)\right)(n+1)$$
$$=\frac{n(n+1)(2n+1)}{6}$$

FLORENCE E. BOYCE.

[9]—Several correspondents consider this problem incorrect, incomplete, or absurd. Others give incorrect solutions. It is, however, quite correctly stated. It is correctly solved thus, or to the same effect, by T. R., Charles Hammond, D. Kelley, Jas. Frobisham, K. Carlson, and others. We give Mr. Hammond's solution as one of the neatest in form:—

"Since the grass grows uniformly, its growth may be kept down by a certain number, independent of time, but varying directly as the area of the field.

"The remaining horses are required to consume the original stock of grass. Their number varies directly as the area, and inversely as the time.

"From this it is evident that in the first case 6 horses take 16 weeks to consume the original stock of 10 acres, and 6 keep down the growth.

"Now, by proportion, the number of horses taken to consume the original stock in a 40-acre field in 6 weeks is 64, and the number required to keep down the growth is 24.

$\therefore$ Answer, 88 horses.

N.B. For the general case of this, see Newton's "Arithmetica Universalis," page 90, 2nd edition, London, 1722.

Our Whist Column.

By "Five of Clubs."

THE LEAD (*Continued*).

Addendum to Plain Suit Leads.

The leads considered in the last two numbers were supposed to be either original leads from long or strong suits, or forced leads with no knowledge of your partner's strength in the suit led. Note that in every case of a forced lead from a short suit, where you have reason to believe that your partner has strength in the suit, the highest is played, so that from Ace two others you lead Ace in this case, following with the next highest. Similarly, from King two others you play King, then next highest; from Queen two others, Queen, then next highest: from Knave two others, Knave, then next highest. This last is the constant lead from Knave two others (as from ten two others, you play ten, then next highest): when your partner has not indicated strength, there is some use in leading lowest from Ace two others, King two others, or Queen two others; but manifestly keeping back the Knave or ten (with two others) can be of no use to you, while playing it may help your partner.

The Lead in Trumps.

We do not propose to consider here the important question (important in whist-play, at any rate) when to lead trumps, but what card is to be led when it has been decided to open the trump suit.

The lead in trumps differs in one important respect from the lead in plain suits,—there is no fear that a good card will be lost if kept back, by being trumped. We can, therefore, play with safety a waiting game; indeed, it is often advantageous to do so, because so much often depends on winning the last round in trumps.* Another difference between trump cards and others arises from the circumstance that you need not so carefully indicate your strength by playing an obviously winning card; for there is no possibility of your partner hurting you by trumping a card which he may mistake for a losing one. The chief variations of the trump lead from a lead in plain suits depend on these considerations—principally on the former.

Thus, in trumps, from Ace, King, and not more than five others, the smallest should be played; this ensures the numerical command in trumps, if you have five others, and is the best way towards obtaining it if you have less than five. Besides, by this course you give your partner a good chance of winning the first trick. Of course, if you have six small ones besides Ace and King, you have the numerical command, even if all the remaining trumps are in one hand; you therefore play King, then Ace.

Again, from Ace, King Queen alone in plain suits, you lead King, then Queen. In trumps, having no fear that Queen will be lost, you lead Queen first, then King. So with Ace, King, Queen, Knave, you lead King first in plain suits; in trumps you lead the lowest of the sequence, the Knave.

Again, in plain suits from Ace and four or more others, not including King, you lead Ace, because of the risk that in the second round it might be trumped. Having no such fear in the trump suit, you lead the smallest, unless you have more than six small ones, in which case, being sure of the numerical command, you lead Ace.

In trumps from King, Queen, and two or more small ones, the lowest is led, instead of the King, as in plain suits; but with more than five small ones, begin with King.

Some trump leads differ from plain suit leads and ordinary trump leads, on account of the trump card being of a particular value. Thus, from Ace, Knave, ten and nine, the nine would be led in plain suits: so, also, nine would be led in trumps, unless Queen is turned up on the left, when Ace should be played. Again, from King, Knave, two or more small ones, the lowest is led in plain suits, and ordinarily in trumps; but if ten is turned up to the right, the Knave is led. In trumps, from Knave, ten, eight, with one or more small ones, lead the lowest, as in plain suits, unless nine is turned up on your right, when lead Knave.

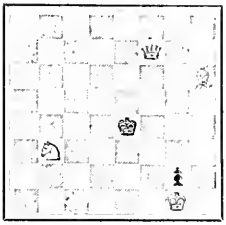
* We do not mean simply having the last winning trump, for this may be left in and the opponents' strong suits yet played out to the bitter end; but winning the last round of trumps, so as to be able to bring in a strong suit either of your own or your partner's.

Our Chess Column.

Problem No. 11 (From *American Chess Nuts*).

By E. B. Cook.

Black.



White.

White to play and mate in three moves.

TWO KNIGHTS' DEFENCE (*Continued*).

Another interesting form of this opening is where Black, on his fourth move, plays Kt. takes K.P. In most cases strong players will adopt this move with good effect against a weaker opponent. As it is chiefly our wish to instruct the student, we shall show him in full all the variations resulting disadvantageously, so that he might avoid them, at the same time also giving him the good replies for adoption.

1. P. to K.4. P. to K.4.	2. Kt. to K.B.3. Kt. to Q.B.3.	3. B. to B.4. Kt. to K.B.3.
4. Kt. to Kt.5. Kt. takes K.P.		

White has three modes of continuing, viz., Kt. takes K.B.P. or Kt. takes Kt., or B. takes P. ch. (best). Of these only the last move can be recommended, for if

5. Kt. takes K.B.P.
Q. to R.5.

6. Q. to K.2. Kt. to Q.5.	or 6. Castles. B. to B.4.	or 6. P. to K.Kt. 3. Kt. takes Kt.P.
7. P. to K.Kt.3. Kt. takes Q.,	7. Kt. takes R. Kt. takes B.P.	7. B.P. takes Kt. Q. to K.5.ch.
8. P. takes Q. Kt. to Q.5.	8. B. to B.7.ch. K. to K.2.	8. Q. to K.2. Q. takes R.ch.
9. P. to Q.3. Kt. to Q 3.	9. R takes Kt.ch. Q. takes R.ch.	
10. Kt. takes Kt.ch. B. takes Kt.	10. K to R.sq. B. to Q. 3.	

In all these cases Black obtains a decisive superiority. 5. ——— Q. to R.5 wins in any case, as the attack obtained thereby is very strong.

If 5. Kt. takes Kt. P. to Q.4.	6. B takes P. Q. takes B.	7. Q. Kt. to B.3. Q. to Q.sq.

and the game is even

If 5. B. takes P.ch. (best)
K. to K.2.

6. P. to Q.3. Kt. to K.B.3.	or 6. Kt. takes Kt. K. takes B.	or 4.
7. B. to Q Kt.3. P. to Q.4.	7. Q. to B.3.ch. K. to K.sq.	or 7. Q.Kt. to B.3. P. to K.Kt.3.
8. P. to K.B 4. B. to K.Kt.5.	8. P. to Q.4. P. to Q.4.	8. Castles. P. to Q.4.
9. Q to Q.2. K. to Q.2.	9. Kt. to Kt.5. Q. to B.3.	9. Kt. to Kt.3. B. to K.3.
10. Castles.	10. Q. takes P. Kt. takes P.	10. P. to Q.3. B. to Q.B.4.

It will be recognised that Black incurs the disadvantage of playing his K. to K.2., on account of the strong centre which he afterwards threatens to obtain, which, as second player, he could not get in the ordinary course. In all the three variations given above, White has no advantage, but Black has a great many chances of getting the better game. In the first variation the game is about even, although Black will have to take some care. We can best show the very attacking nature of the opening in spite of the unfavourable position of Black's King (which, as we have before said, is compensated by his

strong counter by giving the following additional variations, namely, if in the first variation, instead of 10 Castles, White play the tempting move 10. Kt. to B.7. Black gets the advantage as follows:—

10. Kt. to B.7. / Q. to K.2. 11. Kt. takes R. / P. takes P.dis.ch. 12. K. to B.sq. / Kt. to Q.5.

13. Kt. to B.3. / B. to Q.B.4. and Black has the better game, as he has an exceedingly strong attack. He chiefly threatens Kt. to B.6, which White cannot retake with the Pawn, on account of B. to R.6.ch. The Queen has no place to go to if attacked by the Kt., as Q. to K.8.ch. is thereby threatened. White would lose his Queen if, in reply to Kt. to B.6, he should play Q. to Q.sq., by Kt. takes P.ch.

Again, if in the first variation White does not play 8. P. to K.B.4. he will speedily be strongly attacked by Black. If, for instance, 8. Castles. / P. to K.R.3. 9. Kt. to K.B.3. / K. to B.2., with a good game, as Black has two good plans to pursue, firstly B. to Q.3., R. to K.sq., and K. to Kt.sq.; or, secondly, he might play B. to Q.3, B. to K.3., Q. to Q.2., and Q.R. to K.Kt.sq., so as to proceed with an attack on the King's side by P. to K.Kt.4.

Of course, statements of these kind are only general, and meant to show the nature of the position, but real play has, of course, to be modified, even according to weak replies of an opponent. But, whatever White should do, Black should obtain the better game, owing to his good position.

(4) 1. P. to K.4. / P. to K.4. 2. Kt. to K.B.3. / Kt. to Q.B.3. 3. B. to B.4. / Kt. to K.B.3.

4. Kt. to Kt.5. / Kt. takes K.P. 5. B. takes P.ch. / K. to K.2. 6. P. to Q.4.

This is the very best reply, we may say the only move which, amongst the numerous possible moves, gives White the advantage, which fact shows the dangerous nature of this opening. 6. P. to Q.4. breaks up Black's centre, and thereby obtains for White the better game.

In reply to 6. P. to Q.4. Black can play

6. — / P. to Q.R.3.	or 6. — / P. to Q.4.	or 6. — / Q.Kt. takes P.
7. Kt. takes Kt. / K. takes B.	7. P. takes P. / Q. Kt. takes P.	7. P. to Q.B.3. / Kt. to Q.B.3.
8. P. to Q.5. / Kt. to K.2.	8. Q. takes P. (best) / Q. takes Q.	8. B. to Q.5. / Kt. to K.B.3.
9. Q. to R.5 ch. / P. to Kt.3.	9. B. takes Q. / Kt. to K.B.3.	9. Kt. to B.7. / Q. to K.sq.
10. Q. takes K.P. / R. to Kt.2.	10. Kt. to Q.B.3. / P. to B.3.	10. Kt. takes R. / Kt. takes B.
11. Q. to B.4.ch. / K. to Kt.sq.	11. B. to Kt.3. / B. to B.4.	11. Q. takes Kt. / P. to K.R.3.
12. Q. Kt. to B.3.	12. B. to K.3.	12. Q. to K.4.
winning.	and White has a good game.	winning.

Besides these moves Black can also play

6. — / P. to Q.3.	6. — / P. takes P.	6. — / Kt. to K.B.3.
7. B. to Q.5. / Kt. to B.3.	7. Q. to K.2. / P. to Q.4.	7. P. takes P. / Kt. takes P.
8. B. takes Kt. / P. takes B.	8. B. to R.5. / etc.	8. Q. to K.2. / etc.
9. P. takes P. / etc.		

If 6. — / Kt. to Q.3. White wins the Queen by 7. Kt. to K.6.

Therefore, whatever Black may do, White, by 6. P. to Q.4., will invariably get the better game, which move, therefore, is the proper defence against this strong counter-attack.*

* To Mr. Gossip is due the credit of advocating this strong move and the ensuing variations.

SALFORD.—You have not transgressed the rules of chess, but your solution of Problem 6 is unsound. After Q. to Q.3 ch., B. takes Q., P. takes B., why should not Black take B. with P. in his turn? How would White mate then on the move?

JAMES MACALPINE.—If (1) Kt. to Kt.5. in Problem 6, 1. P. to Kt.4., 2. P. takes K.P., Black can play Q. to Q.'s 5th ch., and there is no mate.

CRYPTOTYPE.—If Q. takes K.B.P., in Problem 6, Black moves Q. to Q.R. square; then, if Q. takes Q., P. to Q.B.3., and there is no mate on the move.

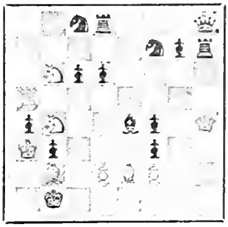
H. FITZ HART. Your letter forwarded to Hades, "Mephisto," being the author of the analysis in question. You should have addressed Chess Editor.

The *Chess Player's Chronicle* calls our attention to the following beautiful problem by Mr. Grimshaw, in which the idea underlying the second solution of his problem, No. 6, p. 190, is embodied.

PROBLEM, No. 12.

By W. Grimshaw.

BLACK.



WHITE.

White to play and mate in four moves.

Unfortunately there are two solutions, one beginning Q. takes P., the other (the author's) beginning Q. takes R. (at R.'s 2.). The *Chronicle* believes that the solution given in the *Illustrated London News* was not the author's; how, then, was it that no reference was made in the *Illustrated* to any other solution? The *Chronicle* appends a note of interrogation to our remark that if, in Problem 6, Queen goes at once to K.'s 6th, mate is only threatened in one way. How is mate threatened, except by B. takes P.? After 1. Kt. to K.B.5., 1. B. takes Kt. 2. Q. to K.'s 6th, mate is threatened also by Q. takes B.,—that is, as we said, in two ways.

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CONTENTS OF No. 13.

HYACINTH BULBS.

BY GRANT ALLEN.

IF we were not so familiar with the fact, we would think there were few queerer things in nature than the mode of growth followed by this sprouting hyacinth bulb on my mantelpiece here. It is simply stuck in a glass stand, filled with water, and there, with little aid from light or sunshine, it goes through its whole development like a piece of organic clockwork, as it is, running down slowly in its own appointed course. For a bulb does not grow as an ordinary plant grows, solely by means of carbon derived from the air under the influence of sunlight. What we call its growth we ought rather to call its unfolding. It contains within itself everything that is necessary for its own vital processes. Even if I were to cover it up entirely, or put it in a warm, dark room, it would sprout and unfold itself in exactly the same way as it does here in the diffused light of my study. The leaves, it is true, would be blanched and almost colourless, but the flowers would be just as brilliantly blue as these which are now scenting the whole room with their delicious fragrance. The question is, then, how can the hyacinth thus live and grow without the apparent aid of sunlight, on which all vegetation is ultimately based?

Of course, an ordinary plant, as everybody knows, derives all its energy or motive-power from the sun. The green leaf is the organ upon which the rays act. In its cells the waves of light propagated from the sun fall upon the carbonic acid which the leaves drink in from the air, and by their disintegrating power, liberate the oxygen while setting free the carbon, to form the fuel and food stuff of the plant. Side by side with this operation the plant performs another, by building up the carbon thus obtained into new combinations with the hydrogen obtained from its watery sap. From these two elements the chief constituents of the vegetable tissues are made up. Now the fact that they have been freed from the oxygen with which they are generally combined gives them energy, as the physicists call it, and, when they re-combine with oxygen, this energy is again given out as heat, or motion. In burning a piece of wood or a lump of coal, we are simply causing the oxygen to re-combine with these energetic vegetable substances, and the result is that we get once more the carbonic acid and water with which we started. But we all know that such burning yields not only heat, but also visible motion. This motion is clearly seen even in the draught of an ordinary chimney, and may be much more distinctly recognised in such a machine as the steam engine.

At first sight, all this seems to have very little connection with hyacinth bulbs. Yet, if we look a little deeper into the question, we shall see that a bulb and an engine have really a great many points in common. Let us glance first at a somewhat simpler case, that of a seed, such as a pea or a grain of wheat. Here we have a little sack of starches and albumen laid up as nutriment for a sprouting plantlet. These rich food stuffs were elaborated in the leaves of the parent pea, or in the tall haulms of the growing corn. They were carried by the sap into the ripening fruit, and there, through one of those bits of vital mechanism which we do not yet completely understand, they were selected and laid by in the young seed. When the pea or the grain of wheat begins to germinate, under the influence of warmth and moisture, a very slow combustion really takes place. Oxygen from the air combines gradually with the food stuffs or fuels—call them which you will—contained in the seed. Thus heat is evolved, which in some cases can be easily measured with the thermometer, and felt by the naked hand—as, for example, in the malting of barley. At the same time motion is produced; and this motion, taking place in certain regular directions, results in what we call the growth of a young plant. In different seeds this growth takes different forms, but in all alike the central mechanical principle is the same:—certain cells are raised visibly above the surface of the earth, and the motive power which so raised them is the energy set free by the combination of oxygen with their starches and albumens. Of course, here, too, carbonic acid and water are the final products of the slow combustion. The whole process is closely akin to the hatching of an egg into a living chicken. But, as soon as the young plant has used up all the material laid by for it by its mother, it is compelled to feed itself just as much as the chicken when it emerges from the shell. The plant does this by unfolding its leaves to the sunlight, and so begins to assimilate fresh compounds of hydrogen and carbon on its own account.

Now it makes a great deal of difference to a sprouting seed whether it is well or ill provided with such stored-up food-stuffs. Some very small seeds have hardly any provisions to go on upon; and the seedlings of these, of course, must wither up and die if they do not catch the sunlight as soon as they have first unfolded their tiny leaflets; but other wiser plants have learnt by experience to lay by plenty of starches, oils, or other useful materials in their seeds: and wherever such a tendency has once faintly appeared, it has given such an advantage to the species where it occurred, that it has been increased and developed from generation to generation through natural selection. Now what such plants do for their offspring, the hyacinth, and many others like it, do for themselves. The lily family, at least in the temperate regions, seldom grows into a tree-like form; but many of them have acquired a habit which enables them to live on almost as well as trees from season to season, though their leaves die down completely with each recurring winter. If you cut open a hyacinth bulb, or, what is simpler to experiment upon, an onion, you will find that it consists of several short abortive leaves, or thick fleshy scales. In these sub-

terranean leaves the plant stores up the food stuffs elaborated by its green portions during the summer; and there they lie the whole winter through, ready to send up a flowering stem early in the succeeding spring. The material in the old bulb is used in thus producing leaves and blossoms at the beginning of the second or third season; but fresh bulbs grow out anew from its side, and in these the plant once more stores up fresh material for the succeeding year's growth.

The hyacinths which we keep in glasses on our mantel pieces represent such a reserve of three or four years' accumulation. They have purposely been prevented from flowering, in order to make them produce finer trusses of bloom when they are at length permitted to follow their own free will. Thus the bulb contains material enough to send up leaves and blossoms from its own resources; and it will do so even if grown entirely in the dark. In that case the leaves will be pale yellow or faintly greenish, because the true green pigment, which is the active agent of digestion, can only be produced under the influence of light; whereas the flowers will retain their proper colour, because their pigment is always due to oxidation alone, and is but little dependent upon the rays of sunshine. Even if grown in an ordinary room, away from the window, the leaves seldom assume their proper deep tone of full green: they are mainly dependent on the food-stuffs laid by in the bulb, and do but little active work on their own account. After the hyacinth has flowered, the bulb is reduced to an empty and flaccid mass of watery brown scales.

Among all the lily kind, such devices for storing up useful material, either in bulbs or in the very similar organs known as corms, are extremely common. As a consequence, many of them produce unusually large and showy flowers. Even among our native English lilies we can boast of such beautiful blossoms as the fritillary, the wild hyacinth, the meadow-saffron, and the two pretty squills: while in our gardens the tiger lilies, tulips, tuberoses, and many others belong to the same handsome bulbous group. Closely-allied families give us the bulb-bearing narcissus, daffodil, snowdrop, amaryllis, and Guernsey lily; the crocus, gladiolus, iris, and corn-flag; while the neighbouring tribe of orchids, most of which have tubers, probably produce more ornamental flowers than any other family of plants in the whole world. Among a widely-different group we get other herbs which lay by rich stores of starch, or similar nutritious substances, in thickened underground branches, known as tubers; such, for example, are the potato and the Jerusalem artichoke. Sometimes the root itself is the storehouse for the accumulated food-stuffs, as in the dahlia, the carrot, the radish, and the turnip. In all these cases, the plant obviously derives benefit from the habit which it has acquired of hiding away its reserve fund beneath the ground, where it is much less likely to be discovered and eaten by its animal foes. For it is obvious that these special reservoirs of energetic material, which the plant intends as food for its own flower or for its future offspring, are exactly those parts which animals will be likely unfairly to appropriate to their personal use. What feeds a plant will feed a squirrel, a mouse, a pig, or a man, just as well. Each requires just the same free elements, whose combination with oxygen may yield it heat and movement. Thus it happens that the parts of plants which we human beings mainly use as food-stuffs are just the organs where starch has been laid by for the plant's own domestic economy—seeds, as in the pea, bean, wheat, maise, barley, rice, or millet; tubers, as in the potato and Jerusalem artichoke; corns, as in the yam or tare; and roots, as in arrowroot, turnip, parsnip and carrot. In all these, and in many other cases, the habit first set up by nature has been sedulously encouraged and increased by man's deliberate selection. What man thus consciously effects in a few generations, the survival of the fittest has unconsciously effected through many long previous ages of native development.

BRAIN TROUBLES.

Partial Loss of Speech.

Let us consider next a case where the almost complete loss of the power of fixing the attention was followed by the partial loss of the power of expression,—a sequence which would, we believe, be far more commonly noticed than usual were all the circumstances of each case carefully noted. The case also illustrates the danger resulting from the endeavour to over-tax the powers of nature:—"I was engaged this morning," says Dr. Alexander Crichton, "with a great number of people, who followed each other quickly, and to each of whom I was obliged to give my attention. I was also under the necessity of writing much, but the subjects were various, and of a trivial and uninteresting nature, and had no connection the one with the other; my attention, therefore, was constantly kept on the stretch, and it was continually shifting from one subject to another. At last it became necessary that I should write a receipt for some money I had received on account of the poor. I seated myself, and wrote the two first words, but in a moment found that I was incapable of proceeding, for I could not recollect the words which belonged to the ideas that were present in my mind. I strained my attention as much as possible, and tried to write one letter slowly after the other, always having an eye in order to observe whether they had the usual relationship to each other; but I remarked, and said to myself at the time, that the characters I was writing were not those which I wished to write, and yet I could not discover where the fault lay. I therefore desisted, and partly by words and syllables, and partly by gestures, I made the person who waited for the receipt understand that he should leave me. For about half-an-hour there reigned a kind of tumultuous disorder of my senses, in which I was incapable of remarking anything very particular, except that one series of ideas forced themselves involuntarily into my mind." The patient goes on to describe the various thoughts which occurred to him at this time, and how he tested his mental condition by thinking of the principles of religion, conscience, and the future life, finding to his relief that these principles he found "equally correct and fixed as before" (a degree of assurance which some do not possess who are quite free from mental disorder). Passing over these matters, as not bearing specially on our subject, we find that so soon as he tested his power of expressing his ideas, either by spoken or by written words, he found that for the time being the power was lost. "I endeavoured to speak, in order to discover whether I was capable of saying anything that was connected; but, although I made the greatest efforts of attention, and proceeded with the utmost caution, I perceived that I uniformly spoke other words than I intended. My soul was at present as little master of the organs of speech as it had been before of my hand in writing. Thank God, this state did not continue very long, for in about half-an-hour my head began to grow clearer, the strange and tiresome ideas became less vivid and turbulent, and I could command my own thoughts with less interruption." It is interesting to notice how the loss of the power of expression was associated thus with con-

fusion of thought and inability to fix the attention. "I now wished," proceeds the patient, "to ring for my servant, and desired him to inform my wife to come to me." (The power of correctly expressing his ideas does not seem to have been possessed in any very remarkable degree by this gentleman, even when his mind had fully recovered its usual health). "But I found it still necessary to wait a little longer, to exercise myself in the right pronunciation of the few words I had to say, and the first half-hour's conversation I had with her was, on my part, preserved with a slow and anxious circumspection, until at last I gradually found myself as clear and serene as in the beginning of the day. All that now remained was a slight headache. I recollected the receipt I had begun to write, and in which I knew I had blundered, and upon examining it, I observed, to my great astonishment, that instead of the words *fifty dollars, being one half-year's rate*, which I ought to have written, the words were, *fifty dollars, through the salvation of Bra*——, with a break after it, for the word '*Bra*' was at the end of the line. I cannot recollect any business I had to transact that could by means of an obscure influence have produced this phenomenon."

In this case it is obvious that the temporary loss of the power of verbal expression was occasioned by overwork; but it is noteworthy that the work was of a special character, involving the special exercise of the power which failed first (that of fixing the attention). It may be worth while to inquire whether that kind of mental confusion, which, when it has passed beyond a certain point is followed by impairment of the power of speech, is generally or often a consequence of distracting occupations. The following case seems to some degree to bear on this question. It is related by Dr. Watson. A patient who had had an attack of apoplexy seemed to be recovering under the influence of perfect quiet. But, "after a long and imprudent conversation with a friend, he suddenly lost the thread of his discourse, and could not recover it." Memory was affected first, be it observed: next went the power of attention. "Then he became confused." Thirdly, the power of speech was affected. "He misapplied words. I asked him how he felt. He answered, 'Not quite right,' and this he repeated very many times, abbreviating it at first into 'not right,' and and at length into 'n' ight.' Wishing to mention '*camphor*,' he called it '*pamphlet*.' I mention these as specimens." Afterwards, signs of bodily weakness, indicating paralysis, were observed. The weakness degenerated gradually into complete palsy, and before long the case ended fatally. In this case the patient had not suffered originally from undue mental work, the mental trouble being caused by an abscess. But the case seems to illustrate well the trying effect of distracting conversation on a wearied, weakened, or (as in this case) diseased brain.

The tendency to use one word for another, where, so far as meaning is concerned, there is no connection whatever, though there is some resemblance of sound, is one which probably most literary men have noticed at times, when they have been wearied or their attention has been much distracted. It is not by any means so alarming a symptom as temporary failure of the power of articulating words, or actual inability to write the desired words; but it is a circumstance which should not be overlooked. A little rest, or the substitution for awhile of some light reading for hard brain-work, will generally set matters right. If not, a longer rest or open-air exercise should be taken. Time will be gained by waiting till the brain is fitter for work. The present writer has repeatedly had occasion to time himself over certain forms of literary work, and his experience has been this, that where four or five hours are to be occupied in steady work, a good half-hour will often be saved by taking half-an-hour's sleep, when such signs of mental weariness are noticed as have been described above. There is, however, one point to be observed. Rest must be taken as soon as such signs are recognised, for if an effort is made to struggle against the occasion for rest, the power of resting may be lost. Precisely as an over tired pedestrian often tries in vain to sleep, when he has but a short time for rest, so the overwearied brain may be kept by confusing thoughts from obtaining rest.

DR. J. W. DRAPER.

BY THE EDITOR. (*With a Portrait.*)

AMONG the distinguished men whose acquaintance I have made during my lecture tours in this country America, and Australasia, few occupy a higher place in my recollection than Dr. J. W. Draper, whose death, at New York, has just been announced. His scientific researches and literary work are justly regarded by those whom we must consider as his fellow-countrymen (though he was an Englishman by birth) as most important and valuable, and his name is not honoured in America only, but throughout the world.

John William Draper was born at St. Helen's, near Liverpool, in 1811, and educated there and at the London University (to which he was sent to study chemistry when the University was first opened). He left England soon after for America, and completed his medical education at the University of Pennsylvania, graduating in 1836. He was soon after appointed Professor of Chemistry in Hampden Sydney College, Virginia, and in 1839 in the University of New York.

His earliest contributions to Science were on the chemical action of light, on which he published nearly forty memoirs. The following summary of his work on this subject is from a biographical sketch which appeared in the *Popular Science Monthly*, in January, 1874 (about the time when I first made Dr. Draper's acquaintance). I believe that though they may have been editorially revised by my esteemed friend Professor Youmans, they were in substance communicated by Dr. Draper himself, thus possessing a special value at the present time, when some of his most important researches are bearing fruit, which others are claiming as their own:—

"Of all the chemical actions of light, by far the most important is that of the decomposition of carbonic acid by the leaves of plants, under the influence of sunshine. On this the whole vegetable world depends for its growth, and the whole animal world, directly or indirectly, for its food. The decomposition in question is essentially a deoxidation, and up to about 1840 it was generally supposed to be due to the violet rays of the spectrum, which, in accordance with the views held at that time, were regarded as producing deoxidising actions, and were consequently known as deoxidising rays. But this was altogether an assumption unsupported by experimental proof. Professor Draper saw that there was but one method for the absolute solution of the problem, and that was by causing the decompositi tion to take place in the spectrum itself. In this delicate and beautiful experiment he succeeded, and found that the decomposition was brought about by the yellow rays, at a maximum by those in the vicinity of the Fraunhofer fixed line D, and that the violet rays might be considered as altogether inoperative. The memoir containing this result was first read before the American Philosophical Society, in Philadelphia, and immediately republished in London, Paris, and Berlin. It excited general interest among

chemists. Even so late as 1874 it furnished to the German experimenters the basis of a very interesting discussion in photo-chemistry.

"In 1842 Dr. Draper discovered that not only might the Fraunhofer fixed lines in the spectrum be photographed, but that there exists a vast number of others beyond the violet, which up to that time had been unknown. He also found three great lines less refrangible than the red, in a region altogether invisible to the eye. Of these new lines, which more than doubled in number those of Fraunhofer, he published engravings. He also invented an instrument for measuring the chemical force of light—the chlor-hydrogen photometer. This was subsequently extensively used by Bunsen and Roscoe in their photo-chemical researches. In their paper, read before the Royal Society in 1856, they say, 'With this instrument Draper succeeded in establishing experimentally some of the most important relations of the chemical action of light.'

"His memoir 'On the Production of Light by Heat,' published in 1847, was an important contribution to spectrum analysis. Among other things it gave the means for determining the solid or gaseous condition of the sun, the stars, and the nebulæ. In this paper he established experimentally that all solid substances, and probably liquids, become incandescent at the same temperature; that the thermometric point at which such substances are red-hot is about 977° Fahr.; that the spectrum of an incandescent solid is continuous, it contains neither bright nor dark fixed lines; and from common temperatures up to 977° Fahr. the rays emitted by a solid are invisible, but at that temperature they impress the eye with the sensation of red; that the heat of the incandescing body being made continuously to rise, other rays are added,

DR. J. W. DRAPER.

increasing in refrangibility as the temperature ascends; and that, while the addition of rays so much the more refrangible as the temperature is higher is taking place, there is an augmentation in the intensity of those already existing. This memoir was published in both American and European journals. An analysis of it was read in Italian before the Royal Academy at Naples, July, 1847, by Melloni, which was also translated into French and English. But, thirteen years subsequently, M. Kirchhoff published, in a very celebrated memoir, considered by many as the origin of spectrum analysis, and of which an English translation may be

found in the *London and Edinburgh Philosophical Magazine*, July, 1860, the same facts under the guise of mathematical deductions, with so meagre a reference to what Draper had done, that he secured the entire credit of these discoveries. In an historical sketch of spectrum analysis, subsequently published, Kirchhoff avoided all mention of his American predecessor.

"Dr. Draper was the first person who succeeded in taking portraits of the human face by photography. This was in 1839. He published a minute account of the process at a time when in Europe it was regarded as altogether impracticable. He also was the first to take photographs of the moon, and presented specimens of them to the New York Lyceum of Natural History, in 1840.

"'A Treatise on Human Physiology, Statical and Dynamical,' became a standard text-book in American colleges. It has passed through a great many editions, and was translated into several foreign languages. The Russian edition is used in the higher schools of that country. A yet more important work is his 'A History of the Intellectual Development of Europe,' thus described in the *Westminster Review*:—'It is one of the not least remarkable achievements in the progress of positive philosophy that have yet been made in the English tongue: a noble and even magnificent attempt to frame an induction from all the recorded phenomena of European, Asiatic, and North-African history.'" [Of this treatise, Dr. Draper's later work, "The Conflict of Science and Religion," may be regarded as in some sense an abstract. It is severe in its treatment of religious intolerance and dogmatism, and does not seem to do full justice to the motives which in many cases have actuated religious persecutions. But the book is the product of a healthy and vigorous mind, and, setting aside the undue hardness of its tone in certain places, it must be regarded as a work which has done, and is calculated to do, an immense deal of good.]

"Though in his earlier years Dr. Draper was a skilful mathematical analyst, he has published but few mathematical papers, the most important being an investigation of the electrical conducting power of wires. This was undertaken at the request of Prof. Morse, at the time he was inventing his telegraph. The use made by Morse of this investigation is related by him in 'Silliman's American Journal of Science and Arts,' December, 1843. The paper shows that an electrical current may be transmitted through a wire, no matter what the length may be, and that, generally, the conducting effect of wires may be represented by a logarithmic curve. Among electrical memoirs there is one on the tidal motions exhibited by liquid conductors, and one on the electro-motive power of heat, explaining the construction of some new and improved forms of thermo-electric batteries. An abstract of these improvements is given in the last edition of the 'Encyclopædia Britannica' (Art. Voltaic Electricity).

"Dr. Draper was the first person to obtain photographs of the diffraction spectrum given by a grating, and to show the singular advantages which that spectrum possesses over the prismatic investigations on radiations. In a memoir on the production of light by chemical action (1848), he gave the spectrum analyses of many different flames, and devised the arrangement of charts of their fixed lines in the manner now universally adopted. A memoir on phosphorescence contains the experimental determination of many important facts in relation to that property. Among purely chemical topics he has furnished a method for the qualitative determination of urea by nitrous acid."

[From 1860 to 1870 Dr. Draper did but little in scientific research, devoting himself mostly to historical works. During this time he published his "History of the American Civil War," in three volumes.]

"In the summer of 1870, Dr. Draper suffered a severe bereavement in the loss of his wife. Of Brazilian birth, she was connected with an ancient and noble Portuguese family. She had rendered his domestic life a course of unbroken happiness, and doubtless she was the exemplar before his eyes when he wrote that oft-quoted passage in his 'Physiology,' in which, after depicting the physical and intellectual peculiarities of woman, he says: 'But it is in the family and social relations that her beautiful qualities shine forth. At the close of a long life, checkered with pleasures and misfortunes, how often does the aged man with emotion confess that, though all the ephemeral acquaintances and attachments of his career have ended in disappointment and alienation, the wife of his youth is still his friend? In a world from which everything else seems to be passing away, her affection alone is unchanged: true to him in sickness as in health, in adversity as in prosperity, true to the hour of death.'"

Of their six children, one died in infancy; the survivors are three sons and two daughters. Of the former, the eldest, Dr. Henry Draper, Professor of Natural History in the College of the City of New York, is eminent as a physicist and astronomer; the second, Dr. John C. Draper, is Professor of Physiology in the University of New York; the third, Dr. Daniel Draper, is Director of the Meteorological Observatory in the New York Central Park, where he has exercised an important influence in developing the meteorological system of the United States. In recent years, Dr. Draper has published two short memoirs: one, on the "Distribution of Heat in the Spectrum," showing that the predominance of heat in the less refrangible regions is due to the action of the prism, and would not be observed in a normal spectrum, such as is formed by a grating; and that all the rays of light have intrinsically equal heating power; the second an investigation of the distribution of chemical force in the spectrum. The *Popular Science Monthly* notes to his credit that "these scientific researches, to which so many years of his life have been devoted, *have been at his own expense; he has never received any extraneous aid, though many of them have been very costly; he has never taken out any patent, but has given the fruits of his investigations and inventions freely to the public.*"

THE GREAT PYRAMID.

By the Editor.

IN No. 10 we showed how the builders of the Great Pyramid, in carrying out what obviously was their purpose, the exact orientation of the building, would have been led to construct those passages, descending and ascending, which actually exist in the building, with precisely the slopes we should expect to find; but we did not pass beyond the smaller of the ascending passages; and, indeed, it is to be noticed that in passing upwards from the upper end of this passage we recognise another plan. All the features thus far have been such as we should expect to find in a massive structure such as this, intended—for whatever reason—to be very carefully oriented. They are such, in fact, as could not but exist in a building oriented so successfully as the Great Pyramid unquestionably is, unless some utterly incredible chance had enabled the builders, by an imperfect method, to hit accidentally on so perfect an orientation. Even then, in passing from the ground level

to higher levels, they must inevitably have lost the perfection of their orientation, unless they had had such means of keeping their work correct as we find they had. This being so, the chances being practically infinite against their first obtaining, and afterwards retaining, such accuracy of orientation, without long, slant passages, such as we find within the Pyramid, we are logically justified in saying it is *certain* that the passages were used in that way, and were intended originally to subserve that purpose.

The case is somewhat altered when we reach the point C, where the ascending passage ceases to be of the same small square section as the descending one. Up to this point its purpose is obvious. But so far as *mere* orientation was concerned, there seems no reason why it should not have retained the same section to a higher level. It is true that the nearer it approached to the central line, LF,* the less effective its directive value; but certainly this value would not be increased by increasing the size of the they had considered this plane for the same reason that the modern astronomer considers it—viz., because this is the plane in which all the heavenly bodies culminate, or attain the middle and highest point of their passage from the eastern to the western horizon. They might have had only a fancy for exact orientation, though one can hardly tell why they should. Still, men of different races have taken strange fancies, and, unlikely though it seems, this might have been such an one, just as the building of colossal tombs seems to have been.

At the point C, however, all doubt ceases. The astronomical nature of the builders' purpose becomes here as clear and certain as already the astronomical nature of their methods has been. For from here upwards the small ascending passage is changed to one of great height, so as to command a long vertical space of the heavens, precisely as a modern astronomer sets his transit circle to sweep the vertical meridian. The floor, however, of the ascending

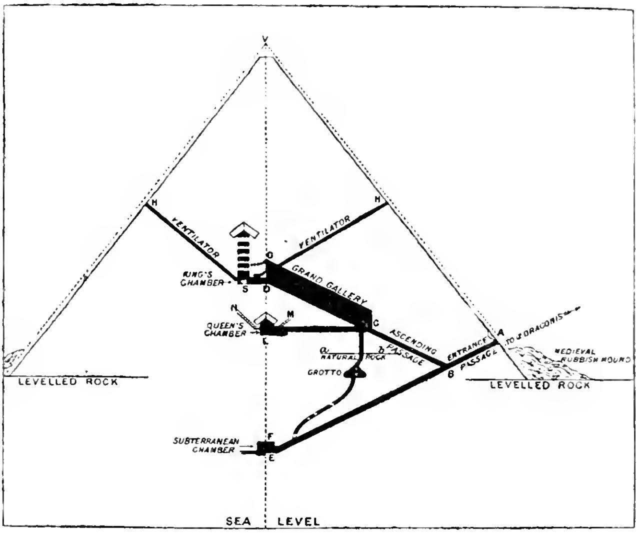


Fig. 1.

passage, whether in a vertical or a horizontal direction; and from and after the point C it is increased in both directions.

Now, we are certain that the builders of the Pyramid wanted to orient it very carefully, simply because we find that they did so. We do not know *why* they did. But it seems antecedently unlikely that *all* they wanted was to get the Pyramid perfectly four-square to the cardinal points. The natural idea is, that being, as we see by their work they were, astronomers of great skill, they had an astronomical purpose of some sort. They had thus far been working with manifest reference to the meridional plane, just as an astronomer of our own time would; and it looks very much, even from what we have already seen, as though

* This line is not vertically below the vertex, V, but central, in the sense of being the vertical line where the horizontal north and south line from the ascending and descending passages crosses the east and west plane through the vertex.

passage, and even its sides, are carried on unchanged in direction, right up to D, where the central vertical (see preceding note) meets the ascending gallery. So that from B to D, except where the horizontal passage CL to the so-called Queen's Chamber is carried off, the floor of ascending passage and gallery formed a perfectly uniform slant plane.

And here let us pause to inquire—seeing that the astronomical purpose of the passages is made manifest—what shape an astronomer, who was also an architect, would give to the great ascending slit, as it were, through which the transits of the heavenly bodies were to be watched. As an astronomer, he would like it to be very high and relatively narrow; but as an architect, he would see that the vertical section could not have such a shape as *ABCD* in Fig. 2; for then, not only would the side walls, *AB*, *BC*, be unstable, but the observer would not be comfortably situated. Yet, as an astronomer, he would know that such a shape as is

shown in Fig. 3 would be unsuitable. To mention only one case out of many, supposing he wanted not only to observe a transit of a heavenly body along such a course as p_1, p_2, or q_1, q_2, which during the short time the body was visible would be practically a horizontal line, but also by observations on successive nights to determine the course of a heavenly body on the star sphere along a path as P_1, P_2, which might be inclined: then, the slant of the walls would entirely defeat his purpose. He would require, as an astronomer, that the walls should be absolutely vertical

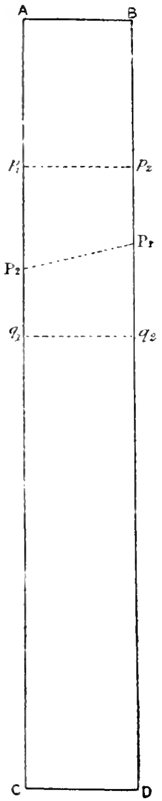


Fig. 2.

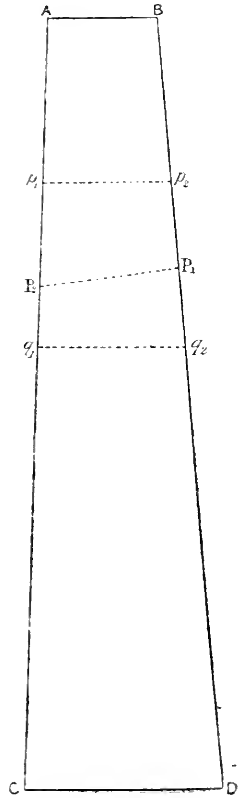


Fig. 3.

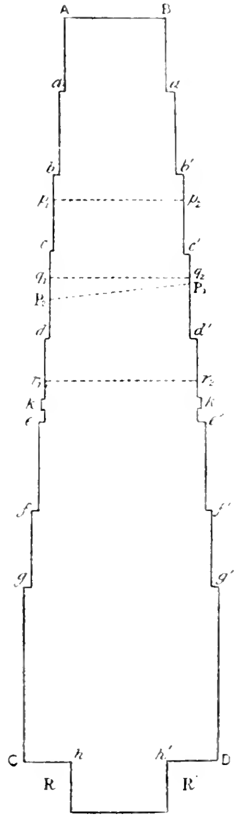


Fig. 4.

(note the difference between the paths p_1, p_2, q_1, q_2, P_1, P_2, in Fig. 2, and the similarly-lettered paths in Fig. 3), while as an architect he would know that they must be closer at the top than at the bottom of a passage so lofty as the great ascending gallery. Fig. 4, giving the actual shape of the vertical section of the great gallery, shows how the astronomical architects of the Great Pyramid combined both qualities. Every part of the walls is absolutely vertical, and yet the walls, regarded as wholes, are aslant.

If we had not seen from the beginning the astronomical plan of the Great Pyramid, and that such a plan indicated an astronomical purpose, we should find, I take it, in this double character of the ascending gallery, proof positive that it was intended for astronomical observations. Only an astronomer would have set the architect such a problem.

But it may be said, How are observers to be stationed along a slant gallery such as this, with smooth and much-inclined floor? Is not the idea that such an unstable place was intended for exact astronomical observation almost as absurd as the notion that the top of the Pyramid was meant for that purpose!

Certainly, if a modern astronomer were planning a slant gallery for transit work he would arrange for comfortable observation (the only observation which can be trustworthy).

Now the ramps, as Prof. Piazzi Smyth calls them—the long slant stone banks, shown in section at R and R' in Fig. 4—seem as if they had some reference to such a purpose. They are at a convenient height above the level of the slant floor, insomuch that Smyth pictures his Arabs leaning on them, stepping on to them, and so forth. But

they would not serve of themselves to make observations easy. The observer has to be set in the middle of the gallery (at whatever point of its length he may be), and he ought to be comfortably seated. I think, if I were planning for his comfort (which means fitness to make good observations), I should have seats set across from ramp to ramp. They must be movable, of course. And if there were not something along the ramps' upper surface to hold them, they would slide down, carrying the observer most uncomfortably with them. I should, therefore, have holes cut out along the tops of the ramps at convenient distances; the holes on one side being exactly opposite those on the other. A set of cross benches should then be made, with projections corresponding to these holes. Then a bench could be set wherever it was wanted, or several at a time, so that different observers might watch the same transit across different parts of the field of view, as along p_1 p_2, q_1, q_2, and r_1, r_2. For some observations, indeed, such holes would serve yet another purpose. By means of them, screens could be set up by which to diminish the field of view and make the observations more exact. Or on such screens, images of the sun (showing the sun spots, be it remarked) could be thrown through a small opening on a screen, covering for the time the mouth of the gallery. For such observations the holes would be convenient, for the seats they would be absolutely essential.

Now no traces of the seats themselves, with their projections, cushions, &c., &c., have been found or were likely to be found. But holes in the ramps are there still; twenty-eight of them there were originally in each ramp, though now only twenty-six remain, owing to the destruction of a ramp stone. They are situated just as they should be to subserve the purposes I have mentioned—that is, at equal distances (of about 5½ feet), and each hole on the east side of the gallery is exactly opposite the corresponding hole on the left side.

THE TRUE STORY OF THE MOON.*

GALILEO and Kepler, Huyghens and Hevelius, Cassini, the Herschels, and a host of other astronomers have tried in vain to interpret aright the telescopic aspect of the moon, during a period of about 270 years. Tired at length of being "une femme incomprise," the moon obtruded herself, Mr. Jones tells us, on his sight, "in so prominent a manner that she seemed to say, 'take a look at me,' and the night was so favourable, that" Mr. Jones "felt bound to accept the invitation." "Round went the tube, down went the eye, and instantly I telescopically gazed for the first time upon a scene of indescribable beauty; gazed and wondered, wondered and gazed, and for a time could do no other." Strange to say, the true meaning of the lunar scenery did not at once present itself to Mr. Jones's lively imagination. A second evening he observed her, and still he "felt that the lunar rings and plain walls were monuments of a departed greatness." But on the third occasion when the opportunity of observing the moon was embraced, "the moon was young, in crescent phase, and the structural character of the rings about the terminator came out beautifully. And now, O, volcanic theory, as commonly understood, thou art doomed. . . . Yes, there can be no mistake, *the ring mountains of the moon are the atolls of an ancient ocean which once overspread that luminary*, and the lunar walls, both of the ring mountains and walled plains, are organic structures, strictly analogous with the coral reefs of the terrestrial oceans, while the whole aspect of the lunar structures indicates that they are principally of similar carbonate of lime texture."

Mr. Jones is not only quick in forming novel views, but expects his readers to be equally quick in grasping them. He gives half a page more to a comparison between the lunar features and those described in Darwin's and Dana's books on Coral Reefs and Coral Islands, and, forthwith, "thinks that enough has been said to enable the reader to find out for himself that the craterology of the moon, and the theory which regards the lunar walls and reefs as direct products of eruption, is a *pure myth*."

Probably *we* have said enough to enable the reader to find out for himself that Mr. Jones's theory is pure nonsense. There is not the slightest resemblance between the lunar craters and coralline structures, whereas there is the closest possible resemblance between the lunar features—craters, mountain ranges, high tablelands, and level plains—and those terrestrial features which result from subterranean forces, or what Humboldt calls the reaction of the crust against the interior. Mr. Jones says there are some astronomers who have ventured to doubt the volcanic character of the lunar asperities, citing, as an instance, the Editor of KNOWLEDGE. He is as much mistaken in this as in his coralline theory (he could hardly be more). The remarks of the Editor, that some of the regions in which small craters are exceedingly numerous look as though they bore the marks of former meteoric showers, or, as Professor Newcomb puts it, "that the figures of these inequalities can be closely imitated by throwing pebbles upon the surface of some smooth plastic mass," bears no such interpretation. Neither Mr. Proctor nor Professor Newcomb has the least doubt that the volcanic theory of the lunar surface features is essentially true.

* "First Steps to Selenography." By John Jones. (John Leng & Co., Dundee.)

BABYLONIAN DISCOVERIES.

FOR some time past rumours have been current of great discoveries by an agent of the French Government in the Mesopotamian valley, and in the latter part of last year they took definite shape; the antiquities excavated were announced to be on their way to the Louvre, and the successful investigator proved to be M. de Sarzec, French Consul at Bussorah. The cases have recently been unpacked, and their contents so far fulfil the expectations created, that M. Oppert, at the "Academy des Inscriptions," has pronounced them to be the most priceless treasures of ancient art contributed to Europe since the great explorations of Layard and Botta.

In the *Gazette des Beaux Arts*, a preliminary account of some of the statues which form part of the "find" is given by M. Menant, an Assyriologist, accompanied by some exceedingly beautiful heliotypes of the objects themselves. This, and several other short notices in the French press, furnish these intensely interesting facts. The antiquities, which all come from one magnificent palace, are of all kinds—sculptured slabs, bas-reliefs, statues, fragments of terra-cotta, and numerous inscribed bricks, some of them with more than one hundred lines of cuneiform writing. Great honour is due to M. de Sarzec for rescuing these valuable remains, for they were buried beneath a part of Mesopotamia, close to the junction of its two great rivers, deep down in alluvial deposits, and their recovery required much greater exertions than that of relics in Assyria.

Had this collection only contained further additions to the fast-growing remains of Babylonia and Assyria, it would have been received with delight by archaeologists, but it fortunately presents us vestiges of another primitive people of Chaldea, the riches and importance of whom are probably at present quite unappreciated. The inscriptions (any full interpretation of which is as yet unattempted) are in very archaic forms of cuneiform characters, and embody a dialect quite distinct from the Semitic Assyrian, but whether closely allied to, or identical with, the so-called Accadian, cannot be pronounced from the fragments published.

Two statues of diorite, of which admirable facsimiles are given, so far from being inferior precursors of Assyrian sculptures, are, if anything, superior to the work of that people which they certainly preceded, proving a far advanced stage of art for the nation by whom they are executed. One is of a person seated, the other an upright figure, both, unfortunately, decapitated, a condition which seems to be that of all the statues exhumed. Each figure is clothed in a long robe reaching to the ankles, but the correct outline of the body is distinctly visible below the folds of raiment, as in the best periods of sculpture, and the delicate arrangement of the drapery is most pleasing. The feet, which are quite naked, are carefully executed. The whole lower front of the dress of the seated statue is covered with cuneiform writing of very old type, apparently closely allied to the extremely ancient texts, from which the Rev. W. Houghton proves the hieroglyphical origin of the cuneiform characters.

This seated figure appears to be that of an architect, for, on his lap, is a tablet inscribed with a plan of a building, and some instrument connected with architecture. The erect figure, if anything more correctly carved, has a few lines of writing on the right front of the robe and the right arm. The attitude of the arms is in

both precisely similar, and so, probably, conventional, but well suited to the character of repose given to the statues, they being crossed before the body, the right hand lower, and holding the left. The loss of the heads of these figures is greatly mitigated by the possession of the exquisite head (belonging to a statue not yet found), a copy of which is given. It bears an embroidered head-dress similar in shape to the old Cossack shako. This, and a fragment of ornament from a marble slab, completes the series of illustrations at present published.

As might be expected from a "savant," M. Menant concludes his monograph by a theory, and as it is one which, if correct, tends greatly to support his previous arguments, it need not be said he urges it with great confidence. For many years the advanced school of cuneiform decipherers had, without exciting much opposition, decisively declared the old Accadian tablets to be in a Turanian tongue, a language allied to that of the Tartar and Finnic families, but lately this has been called in question most determinedly by M. Halévy, one of his arguments being, that if this Accadian dialect were (instead of being, as he declared, merely an esoteric priestly writing, intended by the initiated to be unintelligible to ordinary readers) a language complete in itself, inherited from a prior civilisation, where are the remains of this primitive people? Especially were he and his followers dubious as to a Turanian civilisation, Turanians being celebrated in history rather by the destruction than evolution of culture.

Now, here are the complete remains of an advanced state of art and consequent wealth associated with numerous inscriptions of great difficulty in a writing allied to the alleged Turanian Accadian, or, at least, certainly separate from the Semitic Assyrian. To M. Menant, an advocate of pre-Turanian culture, the opportunity is too good to resist, and he triumphantly avers that here the "blow of a pickaxe" has presented the missing proof.

Do the monuments themselves offer support to either side? None; their evidence is only negative; still, it is decisive as far as it goes. The physical characteristics of the head are certainly non-Turanian, and as decidedly non-Semitic. The features are of a high type of beauty, allied to the Greek or Caucasian. Again, the statues have no analogy either with Assyrian, Egyptian, or Hittite art. If a resemblance must be sought, it would be found nearest in the figures from Branchidæ, obviously only a resemblance, not a real connection. To impartial observers this result is not a surprise, for whilst the Turanianists and Semiticists have been refuting each other, they remember that in the old ethnological list, never yet contradicted by research, Cush is said not to be a son either of them or of Japhet, but of Ham. These wonderful discoveries furnish one certain lesson, taught before in Egypt, that it is not an invariable rule that the greater the antiquity of relics of the past, the greater the inferiority of execution they present. For the last thousand years of its history, the architecture of the Nile valley presents only a decline; here, again, by the Euphrates and Tigris, the earliest appears in some respects to have been the better. Was there, then, a still higher art before this again; who shall say? Only a few years ago, Egypt and Chaldea were accounted the first of nations, and the existence of a great Hittite people only to be inferred from a casual statement by Masoudi, an Arab historian.

All that can be said is, that whatever wonders are still buried beneath the soil of Western Asia can only be revealed by the spade. M. de Sarzec has wielded it lavishly and to good purpose, and deserves the gratitude of scholars and all who desire to know the history of the human race, the world over.

A Member of the Society of Biblical Archæology.

INTELLIGENCE OF THE HOUSE MARTIN.

It is a common delusion, founded upon imperfect information, that animals guided by instinct do not modify their proceedings by reason, but persevere in a mechanical repetition of the same acts. Probably no creature with a complex nervous system that was observed with sufficient attention, under a variety of conditions, would be found so deficient in intelligence as this theory imagines. At any rate, it completely breaks down when applied to our common birds, and quite fails to explain the kind of facts to be narrated concerning the house martin. A cottage of many gables, situate on the slope of a wide heath, was for many years a favourite resort of this sociable bird, and in one season as many as thirteen nests were established. Now, according to the instinct theory, they ought to have been all alike, but in eleven cases there were obvious differences, some slight in appearance, but probably all-important for the stability of the erection or the comfort of its inhabitants. The simplest nest was quite open at the top, sheltered by projecting eaves, and very roughly finished at the margin. Another variety was built quite up to the woodwork, and had a side entrance left in the rough. Others had similar side entrances, neatly finished with a rounded border. On a north-western gable, quite on its top corner, repeated efforts had been made to construct a nest which would brave the storm winds, and after several failures and mendings, a sort of buttress was stuck on below, evidently a new idea. On the southern side, a favourite locality was under a projecting window, sufficiently high above the sill of a lower window that no cat could reach it by a jump. In this situation the birds built twin nests—semi-attached houses, and they placed their doorways close to the wall on opposite sides, so that when looking at them, the left abode had its entrance on the extreme left, and the right one on the extreme right. If the entrances had been in any other position, the birds might have jostled in going in and out. The walls of the cottage being rough cast, offered a good foundation, but there is no tenacious clay near, and the martin architects were never quite successful with two of the highest gables, possibly on that account. Mischievous sparrows occasionally stole a nest, but the right birds were generally very comfortable, and reared their broods prosperously. It was, therefore, a matter of surprise that, after coming for many successive years, repairing old nests, and making new ones, they merely looked at the place, and did nothing in the summer of 1881. The weather was unfavourable, the birds arrived late, and prepared houses some way off, perhaps from their offering more sheltered situations. Towards the close of the martin season, the custom of the old birds for many years was to give the young ones some building lessons, and lines of foundation, several feet long, were usually attached to the cottage walls. Some of them served for the commencement of nests in the following season, but most of them seemed merely school exercises. If these acts were all done under blind instinct, there is a kind of blindness much like seeing, and it may be doubted whether the mud huts of the poor Irishmen exhibit much more intelligence than the martin's homes.

Henry J. Slack.

INTELLIGENCE IN ANIMALS.

On reading the article "Intelligence in Animals," page 177, and also the previous one on "Brain Troubles," page 175, it struck me that human beings might, perhaps, lessen their "brain troubles" and improve their "intelligence" by trying to acquire a curious habit possessed by some animals, especially the dog. I allude to the way they have of saving themselves up, so to speak, when not on duty, which nearly everyone must have noticed, and which the following instance will illustrate: They have, at my father's house, a small black and tan terrier, Toby III., who has taken upon himself the duty of escorting all strangers to the door on their leaving the house. On the slightest sign of a departure, Toby, although lying on the sofa snoring and apparently fast asleep, instantly starts up in a fearful state of excitement, and with every appearance of undying fury and hatred, fairly screams the visitor out. In less than a quarter of a minute he is once more comfortably asleep. It would be interesting to know how dogs have acquired this enviable knack of disengaging their attention when not required; perhaps it is partly because they are, unlike "the literary gentleman," not "exposed to much anxiety respecting family matters."

Some time ago a friend brought us a small terrier, under the impression that it was our Toby that had got lost. It was an amiable little creature, and, unlike Toby, willing to make friends with anyone. On being noticed, it would look up, shake its head, and actually *laugh* with satisfaction. If laughter be a sign of intelligence—and it is an attribute generally supposed to be confined to the most intelligent of all animals, man—our little friend must have been quite an "infant phenomenon." This is the first instance I know about of a dog laughing; but my wife assures me they had a dog which, although bold and courageous, would, on being left in the house alone, cry "real tears," just like a child.

J. H.

GHOSTS.

I am asked by "T. D." [204 and 205] to explain the "War-Office Ghost," in which, he says, three friends, in different parts of England, saw a fourth friend at the corrected date of his death abroad. And Mr. Ebenezer Kelby cites the case of Lord Brougham (mentioned, if I mistake not, in his Autobiography), in which that illustrious statesman is said to have beheld a friend's "ghost;" the "ghost" appearing to him, by mutual pre-mortem agreement, as the spectre of the first deceased of the two. In reply, permit me briefly to say, that before one can form an opinion upon any such

apparently mysterious cases, one must have full and complete evidence, not only of times and seasons, but of all other circumstances connected with each case. There must be no shadow of discrepancy—no lack of complete and full agreement in every jot and tittle of evidence, before a scientist can take the case under his consideration.

To lay the explanation of such cases within the domain of the supernatural, because we may not be able exactly to satisfy others or ourselves of a rational cause for them, is, of course, but a sorry way of escape from our difficulties. But I have, at least, one idea to fall back upon in treating of so-called "warning dreams," and the appearance of apparitions at expected and unexpected times, and that is, the idea of *coincidences*. If a person tells me he dreamt of a person's death, and that the person can be proved to have died at the moment he dreamed of the event, or even if he tells me he saw the apparition of his deceased friend, I reply that he may be indulging in the fallacy of *post hoc ergo propter hoc*. I would further reply—being a disbeliever in "ghosts" of all kinds as visitations from the nether world, at least—that the explanation of these events does not lie outside the doctrine of coincidences. It is, in other words, a mere fortuitous circumstance that the dream or the "ghost" (*i.e.*, the *subjective* image in one's own brain) has appeared at the time (not always exact) of the person's death. If I can show that as startling coincidences occur in our waking life, I may claim to have, at least, shown the possibility and probability of their occurring in the case of dreams and ghosts. Here is one well-known coincidence, as startling to my mind as any ghost story I have ever heard. I quote from my recently published "Naturalist's Note-Book" (Chatto & Windus), page 39:—"The well-known case of Joseph Lesurques, whose misfortune forms the incident on which more than one melodrama and novel has been founded, has recently been brought anew under public notice through Mr. Henry Irving's performance in the 'Lyons Mail,' and by his assumption of the dual *rôle* of Lesurques and his villainous double. The case actually occurred in France in 1794, and its details are sufficiently well known to obviate the necessity for their repetition here. Charged with robbery and murder, the innocent Lesurques was recognised, identified, and sworn to as the real culprit by various disinterested witnesses. Notwithstanding strong exertions which were made to save his life, and, despite his previous high moral character and probity of conduct, Lesurques was sentenced to death, and executed. Soon afterwards, the real culprit, a man who bore the closest possible likeness to Lesurques, was brought to justice. It was then seen that the similarity in features, stature, build, and manner was so close as to have deceived the witnesses who gave evidence at the trial. On these grounds alone, and as a matter of common recognition and identification, the unfortunate resemblance of Lesurques to the real culprit had unwittingly led them into a 'Comedy of Errors,' which resulted in a legal tragedy as its *dénouement*. But more extraordinary to relate still is the incident, well-nigh unparalleled in the annals of coincidences, that Lesurques was marked by a scar on the forehead, and by another on the hand, whilst the real criminal likewise possessed similar markings. Surely "the grim irony of Fate" could no further go than this, in causing chance likeness to assume a form and to entail consequences so fatal and sad as in the case of Joseph Lesurques."

And, lastly, as to a "ghost" being seen by more than one person, or regarding the possibility of several persons being collectively deceived, let me relate the famous case of the Crystal Palace fire. When the wing of the Palace was burnt the animals in the menagerie were believed to have perished in the flames. But, as the flames progressed, the eager and excited crowd of spectators were horrified to behold the chimpanzee struggling to escape from a horrible death on one of the pinnacles of the building. With eager eyes the crowd followed every movement of the ape, and loud was the sympathy for the unfortunate animal. Imagine the sequel. When the chimpanzee was more nearly approached, the object which, to the eyes of thousands, had presented the exact reproduction of an ape, was discovered to be merely a fluttering rag of canvas, the movements of which had simulated in their eyes the attitudes of the monkey. After the occurrence of such an incident, I must be pardoned if I feel somewhat sceptical, even when the united testimony of two or three persons is hurled at my head by way of convincing me that a ghost was thereby necessarily proved to be no mere figment of the brain.

Andrew Wilson.

ARTIFICIAL INDIGO.

It may be interesting to many readers of this magazine to know something about the production of the dyestuff indigo by artificial means, for it is now to some extent obtained practically by the chemical process known as synthesis, from one of the constituents of the tar obtained from the distillation of coal for the production of gas.

Previous to the introduction of this colouring matter into Europe, about the middle of the sixteenth century, the indigo contained in woad (*Isatis tinctoria*) was employed for dyeing, although the Romans and Greeks used indigo for painting; up to the present time the name "wood" is still in vogue in many dyeworks, the vats in which the dyeing is carried on being called "wood vats" and the dye-house "wood-house."

The following is intended to give a short account of the manner in which this valuable colouring matter, so largely produced in India from the cultivation of the indigo plant, is built up by chemical processes. Indigo is composed of carbon, oxygen, hydrogen, and nitrogen, and is represented by the formula C_8H_5NO, showing the number of atoms of each of its elementary constituents. The naphtha obtained when coal tar is distilled, contains a number of chemical compounds—benzol, toluol, cumol, &c.; it is from the second of these (toluol, C_7H_8) that indigo is obtained. By adding an atom of oxygen to indigo, Erdmann and Laurent formed a compound named isatin ($C_8H_5NO_2$); and afterwards Baeyer succeeded in converting this compound back again into indigo by the reduction of its chloride; and from a knowledge of the constitution of this isatin, the two chemists, Claissen and Shadwell, have succeeded in building it up from a nitro compound of benzoic acid (called ortho-nitro-benzoic acid). It is pretty well known that this acid is contained in gum benzoin, from which it was exclusively obtained until within the last few years. Now, however, it is produced from toluol by simple but interesting chemical processes. The nitro-compound of benzoic acid has one of its hydrogen atoms displaced by chlorine, and on bringing this chloride in contact with silver cyanide, the chlorine and cyanide exchange places, forming the insoluble compound silver chloride and the nitrite. This nitrite, heated with potassium hydrate, exchanges the cyanogen for an atom of oxygen and hydrogen, producing ortho-nitro-phenyl-glyoxalic acid and potassium cyanide. The oxygen associated with the nitrogen forming the nitro group, is displaced by a similar process to one already largely employed for converting the nitro compounds of benzol, toluol, &c., into their corresponding amines or amido compounds (aniline toluidine); that is, by contact with nascent hydrogen by the abstraction of water from the amido compound just formed from ortho-nitro-phenyl-glyoxalic acid isatin is obtained, which, as we have already seen, can be converted into indigo by the process discovered by Baeyer.

This chemist has also succeeded in producing isatin by another process from toluol, first producing phenyl-acetic acid.

The colouring matter is obtained practically by Baeyer from a compound found in gum, benzoin, and other natural products, known as cinnamic-acid. This is the process employed:—The acid produced by natural means being too expensive, its synthesis is resorted to; toluol is also, in this instance, the starting-point; it is by the substitution of one of its atoms of hydrogen by chlorine, converted into a compound named benzyl chloride, which, by a proper treatment with nitric acid, is converted into oil of bitter almonds by the displacement of one atom each of the monatomic elements, hydrogen and chlorine, by the diatomic element oxygen. This oil is also called benzaldehyd. It was discovered in 1856 by Bertagnini that this compound, on treatment with acetyl-chloride, is converted into cinnamic-acid. W. H. Perkin, F.R.S., has, however, discovered a more practical and cheaper method for the synthesis of this acid. Two atoms of hydrogen in the toluol are substituted by two of chlorine, forming benzyl-dichloride, and this compound, heated with acetate of soda, yields cinnamic acid. Acting on this acid with nitric acid there is formed a nitro-compound—the ortho-nitro-cinnamic acid. This is caused to combine with two atoms of bromine, by which the dibrom-nitro-phenyl-propionic acid is obtained. By the treatment of this last-named compound with caustic soda or potash, the two atoms of bromine are removed, forming sodium bromide, whilst two atoms of hydrogen are also removed, forming a molecule of water, thus producing a new compound having two atoms of bromine and two of hydrogen less than the last-mentioned compound, the new product being ortho-nitro-phenyl-propiolic-acid, and this, by reduction with hydrogen, forms indigo, carbonic acid, and water. It is the ortho-nitro-phenyl-propiolic-acid which is supplied to the calico printers, who, on printing its alkaline solution with a reducing agent, form an indigo white on the fabric, and on steaming it is oxydised into the pure indigo blue. M. Rosentiehl says, "a mixture of gum water containing the above-

mentioned acid, along with carbonate of soda and glucose" is printed. "The design is scarcely visible at first, all the substances being colourless. But if the cloth is exposed for two minutes to a temperature bordering on 100° C the design appears, and the formation of indigotine (*i.e.* pure indigo) is so plentiful that the colour appears black. Washing with water removes the soluble matter, and indigo blue becomes visible with all its characters, and is intimately fixed upon the fibre." Although the manufacture of this interesting compound has been, according to the *Textile Manufacturer*, given up by one Continental factory, it undoubtedly only remains a matter of time for the further and more practical development of this new industry, and most probably it will become as successful a competitor with the natural product as alizarine is with madder, the cultivation of which has almost ceased, the beet being cultivated in its place for the manufacture of sugar.

SCIENCE AND RELIGION.

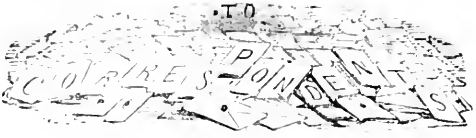
A CORRESPONDENT writes:—"I find fault with the *seeming* tendency of science to account for all 'physical' and 'chemical' laws as being merely due to certain fixed laws of nature. Scientific men may say that by nature they really mean God, or that nature was made by God; but the fact remains, that those who learn may or will say Nature does everything herself, there's no need for a God. Lecturers are too fond of such words as the following: 'These are the means by which Nature works. In this case Nature adopts such and such a course.' When some weak-minded people (and there are many such) are constantly hearing the one refrain of the whole law of existence, being merely a question of 'chemical' decay and 'chemical reproduction,' they say there is no God but these: they will perchance ask the question "Was there ever any beginning? Will there ever be any end?' One of our great men made use of the words that man was nothing but a 'shovelful of phosphates.' Such words spread quickly, not for good but for much evil. These, sir, are the points to which I would draw attention. I, for one, would gladly see a really good correspondence anent this matter. Yet even in this case, I would fear that some of the arguments advanced on the side of science would cause much mischief amongst those of weak minds or weak faith."

[We receive so many letters of this kind, that we think it well to admit so much as we have quoted of our correspondent's letter (all that is essential to the argument has been left): but it is only allowed to appear as the Helots were allowed by the Spartans to show the bad effects of indulgence. To the kind of correspondence which our correspondent invited (yet deprecates, though seeking to initiate it) our columns are emphatically not open. We can neither suffer scientific facts to be advanced as oppugning nor as supporting specific religious doctrines. If scientific statements were made here which seem, whether to "those of weak mind or of weak faith," or to able reasoners, to be opposed to religious doctrines which they hold, our correspondence columns would be open to scientific objections to such statements. They would be open to letters showing how such statements may be reconciled with the religious doctrines apparently oppugned, if they were open to suggestions on the other side. But the balance will be held fairly so far as lies in our power. We regard the wider questions of natural religion as within our scope, but those who wish to attack specific religious opinions from the side of science must seek some other arena; and so also must those who wish to attack science from the side of religion. Our purpose here is to seek for scientific truth. We are in no way concerned with the religious tenets of our contributors or correspondents. Those, on the one hand, who are unsatisfied with science unless used as a weapon wherewith to attack religious opponents; and those, on the other, who ask, first, not whether a scientific statement is true, but whether it can be reconciled with their religious views, will find science, as treated in these pages, altogether unsatisfactory to them. If there are few who do not belong either to one category or to the other, we shall have to admit that KNOWLEDGE is a mistake. But we should not change our plan; we should simply abandon our purpose.—ED.]

SEPARATE SOUNDS ON ONE WIRE.—M. Maiche has found by experiment that sounds of different characters produced from two separate sources can be sent simultaneously on one wire and received separately. He used at the receiving station two telephones of different resistances, and at the transmitting station caused a musical box to be set going on a microphone of small resistance, while an induction telephone transmitter was spoken into at the same time. The musical sounds were reproduced in the telephone which had the least resistance, and the vocal sounds in the other, so that with the two telephones to the ears, the music could be heard by one ear and the speech by the other.—*Scientific American*.

THE FRENCH SOCIAL PROBLEM.—At the beginning of the present century, with a population of not more than twenty-seven millions, there were actually more births in France than took place in the year 1880. M. Legrand, in his well-known essay on "Le Mariage et les Mœurs en France," states that between 1800 and 1815 the number of children born per marriage averaged 4·24; since then it had sunk gradually, and in 1860 averaged only 3·03 for the five preceding years. It rose again until 1865, but has since declined; and in the year 1871, the date of the Franco-German War, reached its lowest depth of 2·26. In 1872 the average rose to its highest for the last few years, namely 2·67, and in 1877 it was 2·55. M. Legrand asserts, on the strongest possible grounds, that this decrease in the birth-rate of his country continues, and is becoming more marked as the years go by. It is a noteworthy and, perhaps, ominous fact, that lately the number of marriages have not decreased. Indeed, there are actually more marriages per cent. in France than in England, the average per hundred being in the former ·88, and in the latter ·86.

ASBESTOS FIRE-PROOF PAINT.—A series of interesting experiments on a practical scale were recently carried out in the grounds of the Crystal Palace with asbestos paint, in order to test its qualities as a protective covering against fire. This paint is a new and special preparation of asbestos, and is being introduced by the United Asbestos Company, of 161, Queen Victoria-street, E.C. The asbestos in a finely divided state is mixed with a fluid material, and is used in a similar manner to other paints. Unlike them, however, it is uninflammable, and not only so, but is capable of communicating this valuable attribute to such substances as it may be applied to. This applies alike to cotton fabrics and to timber or other inflammable materials used for constructive or decorative purposes. Hence its great value in connection with theatrical properties and appliances, especially those connected with the stage arrangements. It was to demonstrate this valuable feature that the experiments were carried out, in the presence of the Lord Mayor and Lady Mayoress, the representative of the Lord Chamberlain, Captain Shaw, and a number of other ladies and gentlemen who had been specially invited, besides the visitors at the Palace generally. The first experiment consisted in submitting to the action of fire some linen, cotton, and gauze fabrics which had been partially treated with the paint. On setting fire to them, the unprotected portions quickly blazed away into tinder, the protected parts remaining intact. The next experiment consisted in placing on one part of a fierce fire some blocks of wood painted with asbestos paint, and on another part similar blocks of wood not painted. In the course of a short time the unpainted blocks were entirely consumed, while those which were painted resisted the action of fire for a long time without showing signs of deterioration. At length, however, the fierce heat of the fire raised some blisters, which on bursting admitted the intense heat, which charred the wood, the external coating of paint, however, being greatly preserved. In the final experiments, four timber erections were employed, two being about 12 ft. wide by 8 ft. deep and 10 ft. high, and representing theatrical stages, with ropes, curtains, and effects. The other two were open timber sheds, about 6 ft. square in plan and 8 ft. high. One of each of these two classes of structures was protected with the asbestos paint, the other two being of plain timber. Piles of shavings and other inflammable materials were placed under and upon the floor of each structure, and lights were applied to all simultaneously. The unprotected stage quickly caught fire, and in about twelve minutes it was a heap of blazing ruins. The unprotected shed, being open-sided, did not take fire so soon nor burn so rapidly, but the flames eventually got hold of it. Both the protected stage and shed resisted the effects of fire to the end most successfully, although inflammable materials, including naphtha, were occasionally employed. In the course of half-an-hour some portions of the fittings were found to be smouldering away, but at no time was there any outburst of flame from the protected materials. The interior of the woodwork, however, was well blistered, but the wonder is that it showed so little evidence of damage. The gauze and lighter fabrics disappeared at an early stage, but only by crumbling gradually away in an incandescent condition, and never once by bursting into flame. These results are highly satisfactory, and fully demonstrate the value of the asbestos paint as a fire-resisting medium in respect of its application to theatrical stages and effects, or, in fact, to any other structures or their fittings. On returning to the palace after the experiments the visitors were gratified by a private view of the concert-room, which was beautifully illuminated for the first time by the Edison electric light. The demonstration was distinctly not a public one, but was only the engineer's experimental trial of the engines and Edison machines, which have just been got in position.



Letters to the Editor.

The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with their writers. All communications should be as short as possible, consistently with full and clear statements of the writer's meaning.]

All Editorial communications should be addressed to the Editor of KNOWLEDGE; *all Business communications to the Publishers, at the Office, 74, Great Queen-street, W.C.*

All Remittances, Cheques, and Post-Office Orders should be made payable to Messrs. Wyman & Sons.

•.• All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.

All Letters or Queries to the Editor which require attention in the current issue of KNOWLEDGE, *should reach the Publishing Office not later than the Saturday preceding the day of publication.*

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

Our Correspondence Columns.

CHINESE CALCULATION.

DURING my first visit to America, in the winter of 1873-74, a Chinese calculating man gave some remarkable proofs of the rapidity with which the persons of his profession in China can execute some of the common processes of calculation. His rapidity in executing long sums, or what with most would be long sums, in addition, was remarkable. A series of numbers, each of four digits, were named to him as fast as they could be entered by a clerk, and when the last of some thirty had been called out, he was told to add them together. "It is already done," he said, naming the total. When the numbers as entered by the clerk had been carefully added together, it was found that the total so named was correct in every figure. It seems to me that there is something in this feat which, though akin to the power some of our banking clerks possess of adding in a single operation numbers of four digits, yet so far surpasses that power as to indicate the use of some entirely different system of arithmetical training, for I am given to understand that what the Chinese calculator did, though it seemed so remarkable, fell far short of what many Chinese computers could do. In fact, I was told he was only an ordinary computer. Can any of our readers give any account of Chinese arithmetic, or of their processes of rapid calculation? ED.]

MIND-DOCTORS.

[224]—What Molière ridiculed in the doctors of his age is true, to some degree, of the mind-doctors of the present time; there is a tendency among them to threaten all manner of evil consequences from quite ordinary and familiar symptoms, unless their aid is quickly called in. It occurs to me, by-the-way (my momentary forgetfulness on the point is one of the slight symptoms in question, and not, I venture to assure myself, a sign of approaching mania), that in "Monsieur de Pourceaugnac," Molière has satirized the very tendency with which we are here specially dealing: "Qu'ainsi ne soit," says the First Doctor to the Second, speaking of the unfortunate De Pourceaugnac, "pour diagnostique incontestable de ce que je vous dis, vous n'avez qu'à considérer ce grand sérieux que vous voyez, cette tristesse accompagnée de crainte et de défiance, signes pathognomoniques et individuels de cette maladie, si bien marquée chez le divin Hippocrate; cette physionomie, ces yeux rouges et hagards, cette grande barbe, cette habitude du corps, menue, grêle, noire et velue, lesquels signes le dénotent très-affecté de cette maladie, procédante du vice des hypocondres; laquelle maladie, par laps de temps, naturalisée, envieillie, habituée et ayant pris droit de bourgeoisie chez lui, pourroit bien dégénérer ou en manie, ou en phthisie, ou en apoplexie, ou même en fine frénésie et fureur. Tout ceci supposé, puisqu'une maladie bien connue est à demi guérie, car, *ignoti nulla est curatio morbi*, il ne vous sera pas difficile de convenir des remèdes que vous devons faire à monsieur," and so forth. CEREBRUM.

SEA SERPENT, OR SEAWEED?

225 If I am rightly informed you are a believer in the sea serpent. The following extract from a daily paper may serve to change your ideas on that subject. "A good sea serpent story—rather better than those which are often published about this time of year—comes from Madras, in the shape of reminiscences of Captain Taylor when lying at anchor in Table Bay some years ago. One day an 'enormous monster,' about a hundred feet in length, was seen advancing with snake-like motion round Green Point into the harbour. The head appeared to be crowned with long hair, and the keener sighted amongst the observers could see the eyes and distinguish the features of the monster. The military were called out, and after peppering the object at a distance of five hundred yards, and making several palpable hits, it was observed to become quite still, and boats ventured off to complete the destruction. The 'sea serpent' proved to be a mass of gigantic seaweed, which had been undulated by the ground swell, and had become quiescent when it reached the still waters of the bay. Probably if mariners would attack the 'monster' in the same manner whenever it is seen, we should hear little more of the sea-serpent." I am, sir, yours &c." J. DAWKINS.

[Mr. Dawkins appears to think that we have never heard of sea weed being mistaken for sea serpents. The following extract from an essay on Strange Sea Creatures, p. 223 of "Pleasant Ways in Science," may change his views in that respect:—"When the British ship *Brazilian* was becalmed . . . Mr. Herriman, the commander, perceived something right abeam, about half a mile to the westward, stretched along the water to the length of about 25 or 30 feet, and perceptibly moving from the ship with a steady, sinuous motion. The head, which seemed to be lifted several feet above the waters, had something resembling a mane, running down to the floating portion, and within about 6 feet of the tail, it forked out into a sort of double fin. Mr. Herriman, his first mate Mr. Long, and several of the passengers, after surveying the object for some time, came to the unanimous conclusion that it must be a sea-serpent. As the *Brazilian* was making no headway, Mr. Herriman, determining to bring all doubt to an issue, had a boat lowered down, and taking two hands on board, together with Mr. Boyd, of Peterhead, near Aberdeen, one of the passengers, who acted as steersman under the direction of the captain, they approached the monster, Captain Herriman standing on the bow of the boat, armed with a harpoon to commence the onslaught. The combat, however, was not attended with the danger which those on board apprehended; for, on coming close to the object, it was found to be nothing more than an immense piece of seaweed, evidently detached from a coral reef, and drifting with the current, which sets constantly to the westward in this latitude, and which, together with the swell left by the subsidence of the gale, gave it the sinuous, snake-like motion."

As a mere matter of detail, it may be remarked that there is some difference between the distance of half a mile in this case, of 500 yards in the other case, and in yet another case of a sea weed sea serpent 800 yards, and the distance of 200 yards at which Captain M'Quhae, and other officers of the frigate *Dædalus*, saw what they stated to be "beyond all question a living animal, moving rapidly through the water against a cross sea and within five points of a fresh breeze, with such velocity, that the water was surging against its chest as it passed along at a rate probably of ten miles per hour." Captain M'Quhae would not go after *his* seaweed because he saw he had no chance of overhauling it, so that for seaweed, travelling against the wind, it was tolerably active. Albeit Mr. Dawkins will not find a word in the essay above-named implying that we believe in the sea serpent. A paddling, long-necked sea creature, probably akin to the *Plesiosaurus*, seems suggested by that portion of the multitudinous evidence relating to supposed sea serpents which is trustworthy. Such a creature would not be a sea serpent, however.—ED.]

COLD SATURATED SOLUTION.—MARINE BOILERS.

[226]—In answer to query 185, Thorpe, in *Journ. Chem. Soc.* for October, 1881, gives a method for estimating total salts in water. In the same Journal, Page & Keightley [2], X. 566, give amounts of nitrate and chloride of K. and Na. in cold saturated solution. Can find no reference to methods in *Dic. Chem.*. If Messrs. G. & S. want a method for separate salts, there is no better way than adding excess of salt, heating and shaking, then cooling to 60° (this applies equally for common salt); or, if salt less soluble in hot water, saturate the solution at a lower temperature, and let it rise to 60° Then take 250 c.c. and make volumetric analyses. W. G. must be wrong in assuming that grease is present in boiler crust. Dust, or any substance that will not wet, make water spheroidal.

C. T. B.

INTELLIGENCE IN ANIMALS.

[227]—Reading to an old sportsman your article on this subject in No. 6, and the correspondence which followed, he related to me how he used to accompany a friend possessing a pointer (bitch), who, when her master missed three consecutive shots, would slink home, heedless of any calls to remain in the field. The friend alluded to was a first-rate shot, and rarely missed his mark, except after an evening's extra glass.

In thus administering a rebuke to her master, the animal, I think, exhibited a rare amount of humour. It could not be said that the dog herself was unmindful of her own duties, for on one occasion, when her master went home to lunch, returning after two hours' absence, she was found at the same spot where left, pointing to a hare lying at the bottom of a hedge. This, of course, was due to good training, but her conduct in the former instance must have been the result of reasoning. A. GAUBERT.

ELECTRICAL IMAGES.

[228]—Having been lately reading Maxwell's "Elementary Treatise on Electricity," I have been able to follow the reasoning and do all the mathematics except one part on page 85 relating to electrical images. The difficulty is to obtain his results without a clumsy and laborious process of multiplying. He says: "If we now write—

$$qaa = a + \frac{a^2 b}{c^2 - b^2} + etc.$$

$$qab = \frac{-ab}{c} - \frac{a^2 b^2}{c\,(c^2 - a^2 - b^2)} - etc.$$

$$qbb = b + \frac{ab^2}{c^2 - a^2} + etc.$$

the whole charge on the sphere a will be

$$Ea = qaa\,Pa + qab\,Pb,$$

and that of the sphere b will be

$$Eb = qab\,Pa + qbb\,Pb."$$

That is all clear enough. But he goes on to say: "From these results we may calculate the potentials of the two spheres when their charges are given, and if we neglect terms involving ^3b, we find—

$$Pa = \frac{1}{a}\,Ea + \frac{1}{c}\,Eb$$

$$Pb = \frac{1}{c}\,Ea + \left(\frac{1}{b} - \frac{a^3}{c^2\,(c^2 - a^2)}\right) Eb."$$

The second result, I cannot for the life of me make out, though I have tried lots of times. On page 186, Fig. 48, it seems to me that the direction of the current in the branch $C\,O$, should be from C to O, and not, as in the figure, from O to C, the battery being placed at E. It occurs in the large treatise as in the elementary one. Surely the Editor should have seen that such an important matter as Wheatstone's Bridge should have been correctly represented. He is not reputed to be over merciful towards the blunders of other minor planets. A STUDENT.

MORTALITY FROM CANCER.

[229]—In an article on the "Duration of Life" (KNOWLEDGE, No. XI., p. 228), Mr. Allinson states that diseases of more advanced life, "such as cancer," are increasing in fatality. Is it a fact that cancer is increasing, or is there only an increase in the number of recorded cases, due to a readier diagnosis on the part of medical men? In the Registrar-General's Report for 1879, the total average mortality from cancer is stated at 0·5 per 1,000 of population. If this is correct, my neighbourhood contrasts badly with the country in general. In the south-western suburb of London, in which I live, there have been in my own small circle of acquaintance within the past year five cases of cancer in elderly persons, in four instances abdominal. Can Mr. Allinson point to any trustworthy statistics in support of his statement, which my experience latterly goes far to confirm? It seems to me a question of great interest whether the occurrence of this most frightful disease is influenced by locality. H. A. EVEREST.

RICHTER'S DREAM.

[230]—Richter's beautiful "Dream of the Universe," or "*Traum über das All*," is in the book called "*Der Komet*," vol. 28, p. 129, of the 34 vol. Berlin edition. Loosely translated by De Quincey, vol. 14, p. 134. J. KIRKMAN, M.A.

ARRANGED SQUARES.

[231]—In the Villa Albani, near Rome, opposite the foot of the staircase, as you descend, is a stone tablet let into the wall. On it is engraved the subjoined arrangement of the square of 9, with the quaint Latin inscription, which I have copied and annexed to the square figure:—

15	58	29	34	63	49	74	41	6
7	27	31	81	23	76	80	18	26
38	8	30	71	47	20	21	78	56
73	19	25	42	10	33	50	65	52
22	55	72	1	45	60	28	16	70
79	35	39	66	2	48	17	24	59
14	64	69	12	77	3	51	68	11
46	36	61	53	40	43	4	54	32
75	67	13	9	62	37	44	5	57

"*Quadratus Maximus.*

Lector si doctus admirator; si ignorus scito quadratus hic mathematicé constructus ab uno usque ad octoginta unum 3321 unitates includit quadlibet ipsius columnæ tam in lineâ planâ quam in rectâ et transversali unitatis 369 quæ ductæ per norem easdem 3321 unitates restituunt et appellatur maximus quia maximam possidet extensionem. Vale.—Caietanus Gilardonus Romanus philotechnos inventor A.D. MDCCLXVI."

The inscription is in capital letters, and without punctuation. I am unable to discover any principle of construction in the arrangement of figures, and, therefore, do not see how it admits of unlimited extension. Can you suggest what the principle of construction is?

As regards the squares of even numbers, I have before me the square of 4, 6, and 8, but can discover no principles whatever in them. The square of 4—absurdly called "the *game* of 34," every body knows. The square of 6 stands thus:—

$S = \frac{n}{2}(n^2 + 1)$
$= 3 \times 37$
$= 111$

1	30	19	18	12	31
32	26	23	20	8	2
33	9	16	22	28	3
4	10	15	21	27	34
35	29	14	17	11	5
6	7	24	13	25	36

The square of 8 stands thus:—

$S = \frac{n}{2}(n^2 + 1)$
$= 4 \times 65$
$= 260$

1	16	48	33	25	24	56	57
63	55	42	34	26	18	15	7
62	54	19	27	35	43	14	6
5	13	20	28	36	44	53	61
4	12	21	29	37	45	52	60
59	51	22	30	38	46	11	3
58	50	47	39	31	23	10	2
8	9	41	40	32	17	49	64

$S = 2 \times 17$
$= 34.$

I believe these squares may be arranged by placing the diagonal numbers in what I may call their *natural* squares in the first instance, and working up to them; but I have only *succeeded* with the square of 4, as shown by the large and small figures in the annexed square.

1	15	14	**4**
12	**6**	**7**	9
8	**10**	**11**	5
13	3	2	**16**

In the engraving by Albert Durer, called "Melencholia I," the square of 4 is represented on the wall of a house, and Mr W. B. Scott, in his "Life of Albert Durer" (p. 99), when describing the engraving, calls the squares on the wall "the magic quadrant of numerals of Cornelius Agrippa." This engraving was executed by Albert Dürer probably between 1507 and 1514. So much for "the *new game* of 34," as it has been lately called. Faithfully yours,

E. V. R.

MAGIC SQUARES.

[232] In my former paper on this subject I gave examples of Bachet's and Poignard's method in squares of odd roots; and I now proceed to give a third rule, and to show how many different arrangements may be made of each square by these methods.

Rule 3, for Odd Squares.

Example of a Square of 5.

Fig. 1.

4	2	1	5	3
2	1	5	3	4
1	5	3	4	2
5	3	4	2	1
3	4	2	1	5

In Fig. 1 place the mean numbers of the series (in this case 3) in the right-hand top-corner cell, and the rest of the numbers in the other cells of the top row in any order at pleasure. Begin the second row with the second number of the first, and so on till the square is filled up.

Fill up Fig. 2 with the multiples of the root, beginning with 0, by placing the mean number in the left-hand top corner cell, and the others in any order. Then begin the second row with the last number of the first, and so on, and it will be seen that the mean numbers, 3 and 10, occupy the diagonals of the squares in a contrary direction. In Fig. 3 place the sums of the numbers in the corresponding cells of Figs. 1 and 2, and the result is a magic square wherein, in every case, the mean number of the progression occupies the centre square.

Fig. 2.

10	0	15	20	5
5	10	0	15	20
20	5	10	0	15
15	20	5	10	0
0	15	20	5	10

Fig. 3.

14	2	16	25	8
7	11	5	18	24
21	10	13	4	17
20	23	9	12	1
3	19	22	6	15

By this method 576 different arrangements can be made of the square of 5. The square of 7 may be varied in the same way 518,400 times, and the square of 9 upwards of 20 million times.

By Poignard's rule the square of 5 may be varied 57,600 times (exactly one hundred times as often as by Rule 3), and the square of 7 no less than 406,485,600 times!—all differing from the results of Rule 3.

And Poignard's squares have another superiority over the others, in the number of ways the total castings of the figures is obtained from them. In Bachet's and the squares by Rule 3 these consist of the addition of each vertical and horizontal band, and of the two diagonals making twelve readings in the square of 5 and sixteen in the square of 7. Poignard's give eight more readings in the square of 5, and twelve more in the square of 7 —as shown in the annexed figure, where, in addition to the sum of 65 being made by the addition of the vertical and horizontal bands, and by the two main diagonals; it is also made by adding each partial diagonals a and the cell or cells on the opposite side a', c' &c., making five cells, and b and b', c and c': &c., four only are here shown, but four more are found in the partial diagonals at right angles to those marked (twenty readings in all).

	a'	b'	c'	d'		
a'	11	7	23	4	20	a
b'	24	5	16	12	8	b
	17	13	9	25	1	c
d'	10	21	2	18	14	d
	3	19	15	6	22	
		a	b	c	d	

But this is not the limit of the number of readings, for Mr. Snart has, by great perseverance and ingenuity, constructed a square of 7 having no less than forty-two readings, which I will send to you.

J. A. MILES.

---

[233] As a climax to odd Magic Squares, I send you a square of 49 cells, which was constructed by a Mr. Snart, having some curious properties, not to be found in other squares:—

			a				
40	39	8	34	9	25	20	
3	12	47	7	45	33	28	
16	42	11	22	10	48	26	
31	17	15	49	13	18	32	d
27	41	21	4	14	44	24	
35	19	37	30	46	6	2	
23	5	36	29	38	1	43	
g			f			e	

(Corner letters: b at top right, h at left of the middle row.)

In a Bachet's square of 7 there are sixteen readings of the total 175. In Poignard's there are twenty-eight; but in the above square, by Snart, there are forty-two readings.

14 horizontal and vertical.
2 diagonal.
8 Right angles $a, c, d,$—$b, c, e,$ &c.
8 Acute angles—$a, c, b,$—$b, c, d,$ &c.
8 Obtuse angles $a, c, e,$—$b, c, f,$ &c.
1 The centre square and the four corner squares.
1 The centre square and the four squares $a, d, f,$ and h.

Total 42

It is seen that the highest number of the progression occupies the centre cell, and is called into operation thirty times.

Fakenham, *Dec.* 30, 1881. J. A. MILES.

VARIABLE MAGIC SQUARE.

[234] In the arrangements given in KNOWLEDGE of numbers 1 to n_2 n is always an odd number, and there is no mention of the following arrangement of the numbers from 1 to 16:—

1	8	11	14
15	10	5	4
6	3	16	9
12	13	2	7

in which 34 is made by the addition, not only of either diagonal or any horizontal or perpendicular line, but also by *every four adjacent figures*, and by the four corner figures, as:—

1 8 / 15 10 or 10 5 / 3 16

Moreover, these conditions are still fulfilled if I shift the horizontal lines from top to bottom, or *vice versâ*, or the perpendicular lines from right to left, or *vice versâ*. I may, in short, by so moving about the lines, bring any number I choose to any given position in the square.

FLORENCE E. BOYCE.

ABSTRACT TERMS IN SCIENCE (*Abstract*).

[235]—Words which are merely abstractions and convenient working terms to the scientist and mathematician are handled in such a way that non-scientific persons are apt to give to the purely abstract conception a concrete meaning.

Take the word Energy. We hear of energy "poured into," "taken from," "transferred," "accumulated," "wasted," and so on. What wonder that many should regard "energy" as something having as substantial an existence as a glass of wine. So with the language used in describing the so-called electric "accumulators" and "storers." Ninety-nine hundredths who read about the Faure accumulators suppose that a real substance called electricity is poured, which can be tapped like beer from a barrel, that a Faure accumulator is as innocent of containing any electricity as the Meteorological Office is of knowing how to make weather forecasts,—it is not a container of, but a producer of, electricity—that it consists of lead plates practically double, back to back, the one side being electro-positive, the other electro-negative; and that it is, in fact, nothing different in the principle of its action, from an ordinary galvanic cell.

There is no need for this foolishly figurative language. The discovery is as wonderful, when described as an improved method of preparing plates for the generation of electricity, as when called, in inflated language, "a method of carrying so many millions of foot-pounds of energy from London to Edinburgh, &c."

EDMUND P. FOY.

PERSONAL IDENTITY *v.* TATTOO MARKS.

[236]—Physiologists admit that the human body is in a constant state of change, fresh materials being added and fresh waste removed, in fact these are the characteristics of every living organism; but that a complete change and reconstitution of the body takes place every seven years will, I am afraid, be admitted by very few. In old age it may take longer than seven years, whilst in disease and in those who live dissipated lives a much shorter period will suffice. Mr. Maguire inquires why it is tattoo marks do not disappear. I will endeavour to answer his query. Tattoo marks are produced by making small punctured wounds into the true skin with sharp needles dipped in some colouring matter. Slight inflammation is produced, but soon passes off, leaving the colouring matter encysted permanently in the substance of the cutis vera and cellular tissue below it. The colouring material used is insoluble as carbon, vermillion thence cannot be taken up by the absorbents or lymphatics and excreted from the body by the excretory glands.

Occasionally tattoo marks do disappear. M. Hutin found that out of seventy-eight persons tattooed with vermilion, the marks disappeared in eleven; and out of 104 tattooed with China ink, not one had become obliterated. The cases where the marks have disappeared have been due to their having been inefficiently performed, and not to the colouring matter having been removed by the lymphatics. This only occurs when soluble and fugitive colours are used, and when the surface of the cutis only is penetrated.

CHAS. BOYCE, M.B.

TELESCOPE.

[237]—I am contemplating the expenditure of from £40 to £60 or so in a telescope and accessories. Perhaps some of your numerous correspondents, and notably "A Fellow of the Royal Astronomical Society," would not object to give a little aid in the way of laying out my money to the best advantage. I am an amateur, and that only, and likely so to remain, as my time available for astronomy is very limited. I should, however, like an instrument that would take me well through Webb's "Celestial Objects," and one which would be of real use to me in case at any time hereafter I might have more time to devote to the subject. I am doubtful as to whether a Reflector or Refractor would best suit my purpose. The great objection to the former seems to be that it must almost necessarily be a *fixture*, and this would scarcely suit me at present. Neither is it so handy.

COUNTRY SOLICITOR.

HOW TO CONSTRUCT A TELESCOPE.

[238]—The astronomical papers now running a course in this magazine have, no doubt, induced many readers to begin a telescopic scrutiny of the skies. Pecuniary considerations, however, may have affected the number of astronomical recruits, and, therefore, I think it not uncalled for to show how one may become possessed of a good three-inch telescope at a comparatively trifling cost. In the beginning of last year I succeeded in making an effective telescope of that size, and where I have succeeded very few need fear failure. Its cost was inconsiderable, and I shall be glad if the following short account of its construction encourages anyone to make a similar attempt.

Having ascertained the dimensions of a 3 in. telescope, I got a carpenter to make several cylindric moulds, the largest being 34 in. in length and 3½ in diameter: and upon this mould I made the principal tube, employing in its construction nothing but the stoutest and largest-sized cartridge paper and an unlimited supply of thick flour paste. The method was as follows:—As much of the first sheet as formed the internal circumference of the tube was painted dead black, and wound, not too tightly, round the mould, the second layer being formed by the continuous winding of the first sheet. No paste was used for the second layer, in order that the inside of the tube might not be blistered, a slight touch of paste being given where the sheet overlapped and ended. Then two sheets at a time were wound round as evenly as possible, paste being laid on with a liberal hand, and each layer allowed to dry thoroughly before winding on its successor. This process was continued until a thickness of over one-eighth of an inch was reached; and when removed from the mould, and carefully evened at the ends, I had a light tube, 29 inches in length, and as hard and smooth as the tube of a 5-inch telescope need be. It is astonishing how hard pasteboard can be made when good paper and plenty of paste are used. (I may mention, as a warning, that the removal of the tube from the mould was somewhat difficult, as the contraction of the paper has been greater than allowed for.) The other tubes, of which details are given below, were made in the same way, and of nearly the same thickness. The method of fitting the pieces together will be readily understood from the following longitudinal section of the instrument:—

It will be seen that the telescope has two "draws," B and C, which slide in fixed tubes, D and E. Round the middle of the fixed tubes strips of paper 2 inches in width for tube D, and 1 inch for tube E, are firmly pasted, forming bands of sufficient diameter to fit the tubes into the ends of which they are fixed. The fixed tubes are thus tightly fitted, and are held in their places by small brass screws. The second draw, C, is a short tube, whose diameter is just sufficient to admit the tube of the eye-piece. The draws are fitted with stops, and the order in which the various pieces have been placed together is obvious. Each part was allowed to dry and contract thoroughly before being fitted. A brass screw is fitted into the mouth of the tube, and receives the screw attached to the cell of the object-glass. I may mention that, for the sake of appearance, the telescope is covered with dark-coloured imitation morocco. The dimensions are as follows:—Tube A, length 29 inches; internal diameter, 3½ inches. B, 10 and 2¾ inches. C, 5½ and 1¼ inches. Fixed tube D, length 6 inches, projecting 2 inches beyond end of tube A. Fixed tube E, 6 inches, projecting 1 inch. Focal length, over 41 inches; closed, under 34 inches.

When the various tubes, &c., were completed, glasses of first-rate quality were purchased, and the whole parts put together and carefully adjusted. There is little or no vibration.

The mounting of the telescope was a simple matter. For out-door work a tripod garden stand and cradle was purchased, and for work indoors a pillar and claw-table was seized, the top removed, and, with a little trouble, fitted so as to receive the cradle of the garden stand, and the claws loaded with lead, for the sake of steadiness. Including the extra expense caused by bungling and ignorance, the total cost of the instrument was about £12. The table stand, it must be remembered, however, cost only a remonstrance.

I need hardly say that in the course of its construction I enjoyed many distinguished failures; parts were made and remade, fitted and re-fitted before all defects were rectified and the instrument entitled to rank as a telescope. Its appearance may not be smart, but a telescope is handsome according to its performance.

A. P. M.

COLOURS OF STAMENS.—ORIGIN OF EVERGREENS.

[239]—Mr. T. Dowse's objection (214) to my theory that flowers were all originally yellow, has already been advanced in a private letter by Mr. Darwin (who, I am glad to say, is inclined to agree with my general view that petals are derived from flattened stamens). Still, I feel disposed, in spite of such an authoritative critic, to adhere to my first statement. It is true the filaments of many stamens are white, pink, or purplish; but this is the case chiefly (so far as I have observed) with very highly developed flowers, in which the petals have undergone much change of colour. It is especially noticeable in tubular blossoms. On the other hand, most very simple flowers, such as buttercups, have bright yellow filaments, and my general impression has been that petalless flowers (for example, catkins) usually have yellowish stamens. The point is an important one, and I will make definite observations

upon it during next season. Conservatory flowers are the worst possible test in such a case, because they are especially chosen for their highly-developed petals; and yet eleven instances examined by Mr. Dowse out of twenty-three, even there had yellow filaments, while only seven had white, and five red.

In answer to "Plesiosaurus" (182), should say that evergreens never had any origin. The real problem is exactly the reverse—the origin of deciduous trees. Clearly any plant is benefited by having its leaves at work all the year round; and all plants were evergreens till a comparatively late geological period, when the poles began to grow cold. A few trees then acquired the habit of deciduousness, in adaptation to the new conditions, but to this day, in the tropics, evergreens are universal. The process by which deciduous leaves were developed I have already attempted to explain in my little book, "Vignettes from Nature," and I will not attempt to give the explanation over again here. GRANT ALLEN.

INFLUENCE OF SEX ON MIND.

[240]—Under the above heading, "J. McGrigor Allan" makes the following assertion—"Women lack the highest quality of the human mind—justice." If this be true, how is it that Shakespeare and Sir Walter Scott, to say nothing of lesser men, have left so widely different a testimony respecting the dignity of woman. To quote the words of one of the most polished writers of the present day, "Shakespeare represents women as infallibly faithful and wise counsellors, incorruptibly *just* and pure examples, strong always to sanctify, even when they cannot save." In Sir Walter Scott's imaginations of women, we find (with endless varieties of grace, tenderness, and intellectual power) a quite infallible sense of dignity and *justice*. Are these immortal writers wrong, and J. McGrigor Allan right, or *vice versâ*? Will you or any of your readers answer this question, and oblige one who is— ONLY A WOMAN.

WINDMILL ILLUSION.

[241]—The Windmill Illusion, p. 233, reminds me of two somewhat similar illusions I have noticed:—1. The "governors" of a steam-engine appear to rotate either way you please; 2. The same with the "parallel rods" of a locomotive when seen from a railway-carriage near the engine.

Perhaps "Enquirer," Query 166, p. 234, may be glad to know of the Dot and Dash system of shorthand, invented by T. S. Noble, which is considered by some to be more easily learnt than Pitman's. VEGA.

VOLCANIC PROJECTILES.

[242]—In the article on Vulcanology, pp. 129 and 130, it is stated that Vesuvius sometimes exerts such force as to project matter nearly four miles high; and, further on, it says astronomy has taught us that the world is not, as was long believed, a liquid mass surrounded by a thin solid shell.

Now, with regard to the first statement, will you, sir, kindly say if there is any foundation or belief that this force and height has ever upon exceptional occasions been greatly exceeded, and, if so, whether you think it possible that such matter might be thrown beyond the direct power of the earth's attraction, to return, perhaps, at some future time in the shape of meteoric stones, or never; also what height would be necessary for such matter to attain to get beyond this attraction.

With regard to the second statement, how does astronomy prove that the earth is solid throughout? JOHN ROUSE.

POPULAR FALLACIES.

[243]—You would indeed deserve the thanks of all sane peoples if you could reason away fallacies as suggested (No. 203, page 233), even if they only concern the familiar poker that does *not* "draw up" the fire.

In some parts of Scotland nervous people think that to praise their possessions will necessarily bring destruction upon the thing praised. A man says his horse has never been ailing for a day, or that he loves a favourite tree, or admires his wife's dress. He has sinned in uttering his thought, but if he raps three times on the table and says "I must not *forespeak* myself," perhaps his horse will not go lame, his tree will not be blown down, his wife's dress will not be torn.

It would be impossible to talk gravely of such a custom were it not for the repeated surprise with which one observes that educated (*and religious!*) people still have a lurking belief in its efficacy, just as they still have a lurking dislike to thirteen at a dinner table. M. McC.

ANIMAL PHYSIOLOGY (THE EYE.)

[244]—In the adjustment of sight, found in the vision of birds, the eye is peculiarly adapted for long and short sight. It is like a cup in shape, has bony substance in the form of plates, extending along the top and bottom of the eye, forming a support of the sclerotic or hard lining, stretching from the cornea to the back part of the eye, along which the retina is spread. These plates contract or distend at will, but when contracted, they press the humour, causing the cornea to protrude, and the retina to recede from the lens. Connected with this pressure, and alteration of humour, there is a peculiarity in the eye of the bird—a thing like the appearance of a feather, which enters the vitreous humour by the optic nerve, commonly called a pecten, from its likeness to a comb; containing a lot of blood-vessels, mixed with pigment granules. The question is, what effect has this upon the eye? Whether it is subject to erection or distention on being filled or deprived of blood, or does this pecten act as a sieve, passing through it matter from the blood to feed the vitreous humor; or is it used in passing off what surplus humor there may be, and forming it into blood? If so, in what manner does it affect the vision? Something of the same kind is found in fish, although situated differently, it being between the two layers of choroid, instead of entering into the vitreous humor, as in the case of birds. In an animal belonging to the highest class of mollusca, there is a thin retina at the back of the eye, and at the back of this a choroid, while at the back of this again, is another retina, apparently for some other purpose than to receive impressions. Is there any similarity between this and the pecten of the bird, and what purpose do they perform? GEORGE BROWN.

SOLAR PUZZLE.

[245].—There could be nothing between the sunlight and the window-blind, except such things as those you suggest; but you must admit that not one of them could obscure, in regular and very slow succession, about three minutes to each hole, and in a diagonal direction, the sun-patches, and, be it remembered, without interfering in the least with the long horizontal patch of sunlight, or with the two or three holes on the extreme right. Forgive me for my pertinacity. I see the difficulty in arriving at any solution, and that was why I wrote to KNOWLEDGE. One solution occurred to me; it was—could an insect have obscured the holes? Possibly, but then it could only have been one at a time, for to have three, or even two of the holes obscured at once would have required an "animal." I can't say an "insect," six inches long, at the least. A. T. C.

A NEW FACT IN PRISMATIC ANALYSIS.

[246]—With reference to the "Answers to a Correspondent" in KNOWLEDGE, vol. 1, page 257, about the delay in furnishing more "Blowpipe Lessons," I would reply that I have now sent four communications to KNOWLEDGE, only one of which, apparently, has been considered up to the publishing mark.

It has just struck me that the Editor's complaint against too many of us is prolixity. However, it is refreshing to see, by the Editor's remark above-mentioned, that, in Bow-street language, "I am wanted"; and so I have the pleasure to enclose another "easy lesson," with what may be termed a "telegraphic summary" of a curious result, obtainable by anyone possessing a lens and a glass prism on a sunshiny day:

Focus face beam	sunbeam prism within	admitted : instead prism	Venetian blind of | spectrum : why | green?	on green

Hoping our "no-rent" friends across St. George's Channel will admit this "bangs Ballagher" (*i.e.* Dr. F. G——e) in scientific conciseness.—I remain, &c., W. A. ROSS.

WEATHER FORECASTS (ABSTRACT).

[247]—I use the forecast chart and remarks, in conjunction with observations of the general aspects of the weather and sky around me. The value of forecasts thus applied, which I hold to be the true and rational method, is, on the whole, satisfactory. "Bad shots" are exceptional. It is well known that persons constantly out of doors—seamen, farmers, millers (wind), &c.—become highly sensitive to atmospheric changes, and are able to judge fairly well of impending weather. Knowledge of this kind is only gained by long observation, and cannot be communicated.

Storm warnings are, as a matter of fact, of most use to seafaring people; crops must risk the weather, favourable or unfavourable to their growth. Weather-wisdom in harvesting is a great help in securing crops in sound condition.

Of late the *New York Herald* has made some remarkable "hits." The stormy character of the past three months would permit them to risk almost anything resembling a "weather-case" with a fair chance of success. Nevertheless, they make some "very bad shots," and if they venture to add more to the simple announcement of a storm, such as "developing energy," "snow in the North," "attended by electrical phenomena," &c., it betrays an attempt on the part of the "prophet" to obtain credit for an amount of sagacity he does not possess. I shall not dwell upon the sweeping character of these warnings generally, but draw the attention of Mr. Spiller and A Fellow of the Royal Astronomical Society to the circumstance that up to the present time the Americans have not attempted to foretell fine harvest weather, or settled weather of any kind, which, if done, would cause me to view their weather predicting more favourably.—Yours, &c., ALFRED DONBAVAND.

COMMUNICATION WITH THE MOON.

[248]—The following excerpt from the *Edinburgh New Philosophical Journal* for October, 1826, may interest "X. Davine," (letter 206, p. 233). As the Editor has remarked, the idea is due to a German, not a Frenchman.

"Gruithuisen, in a conversation with the great astronomer Gauss, after describing the regular figures he had discovered in the moon, spoke of the possibility of a correspondence with the inhabitants of the moon. He brought, he says, to Gauss's recollection, the idea he had communicated many years ago to Zimmerman. Gauss answered, that the plan of erecting a geometrical figure on the plains of Siberia corresponded with his opinion, because, according to his view, a correspondence with the inhabitants of the moon could only be begun by means of such mathematical contemplations, and ideas which we and they must have in common."

Gruithuisen was one of the most painstaking and keen-visioned selenographers who ever lived, though the exuberance of his fancy frequently led him astray. The work done by him in the earlier part of the present century is something marvellous, considering the comparatively small instruments he employed. Many of his drawings, &c., have been recently published for the first time in the German periodical *Sirius*, and are worthy of careful study.—H. SADLER.

A NEW COMPARISON OF POISONS.

[249]—"S. E. P.'s" (No. 172, p. 208) criticism of my letter is somewhat hieroglyphical, and at first rather puzzling. He says the allegation is, that li. ce. is three times as poisonous as ba. ce. I am curious to know what li. ce. and ba. ce. mean.

"S. E. P." continues that li. ce. has 20 per cent. of metal, where the citrate has probably much less than 7 per cent.; so li. ce. cannot be his formula for lithium citrate (Li_3, C_6, H_3, O_7 10°/$_\circ$ Li), nor yet for lithium chloride (Li Cl), which contains 16·4 per cent. of metal.

That some metals may be very poisonous in one form of combination, and not in another is very true, but it depends upon the properties of the substance employed for combination.

I think if anyone has mystified M. Richet's plain proposition it is "S. E. P.," for lithium as a metal is non-poisonous.—I am, Sir,

TECHNICAL CHEMIST.

ANIMAL *versus* VEGETABLE FOOD.

[250]—If the following be worthy of a place in your pages, I shall be glad if you will insert it, for it has struck me that in the arguments that have been brought forward in KNOWLEDGE on this subject, and those used by vegetarians generally in favour of their system, one thing has been lost sight of, or disregarded—viz., the provision and requirements of nature.

If we go to the frigid zone, we find that practically vegetable food is out of the question, for the simple reason that it cannot grow in those sterile regions, amongst the snow and ice. The inhabitants are therefore obliged to eat that which is provided in abundance—viz., animal food.

We are told that the effect of the extreme dry cold to which the inhabitants are exposed, is to produce a desire for the most stimulating food that they can obtain; that in such a climate bread is not only not desired, but is comparatively impotent as an article of diet; that pure animal food, the fatter the better, is the only sustenance which maintains the tone of the system, and supplies the degree of muscular energy necessary for the particular wants of the locality.

But if we turn our thoughts to the torrid zone, the state of things is entirely reversed. There, the general use of animal food would be exceedingly difficult; for, as your readers will doubtless be aware, in a hot climate, animals, if not cooked immediately they are killed, become tainted and unfit for food; therefore, it can only be used on special occasions, and vegetable food is, to all intents and purposes, the only food in hot climates.

Also in the tropical regions of the globe, where the fertility and productiveness of the soil are so largely increased by the high temperature, less labour suffices for the raising of food; less labour is also required to provide habitation and raiment; less demand, therefore, is made upon muscular energy, and, consequently, less or no animal food is required to keep it up.

If we take into consideration personal taste, we find that, on a hot summer's day, nine persons out of ten would prefer a dish of cold, stewed fruit with rice, to a joint of hot, cooked meat; and in the winter it would be just the reverse.

It seems to me, then, the conclusion to be drawn from the above facts *is that as we* (to bring the matter home), live in a temperate latitude, between the two extremes, where a moderate amount of musaular energy is required to provide food, clothing, and shelter, our diet should consist of both animal and vegetable, with a predominance of the former in winter, and of the latter in summer.

C. E. H.

P.S.—Does the trichina exist in bacon, or only in pork in its uncured state?

TOADS.

[251]—On p. 202 of No. 10, for Jan. 6, 1882, appears a paragraph on the vitality of toads, in which it is said that M. Legrips asserts that toads are inoffensive.

In Professor du Boiz-Reymond's lectures on "Physiology," which I had the pleasure to hear last summer in Berlin, he distinctly stated that toads were poisonous, and that they ejected the poison when molested. If the poison reached the face, striking, for example, the eye, it would be dangerous certainly to the eyesight; if not more. He declared that they have poison enough to kill a small bird; and that even when experimenting with the common frog, he had learned through painful experience to keep his eyes shut when seizing them; for on more than one occasion they had spurted out poison which caused pain enough on striking his eye to make one keep to the sofa for the afternoon, as he expressed it.

If you would, through your appreciated paper, throw some light on this contradiction, you would much oblige yours, &c.,

D. R. McC.

SUNLIGHT ON FIRE.

[252]—With regard to my query, "Effect of Sunlight on Fires" (136), I am much obliged to "C. T. B." for his reply, giving, as it does, I think, a right explanation of what I called attention to. I would remind "Paugul" that contradiction is not explanation, and that no good is gained by telling querists that the evidence they quote is useless, and that their suppositions are absurd. As a matter of fact, nine people out of ten that I have asked tell me that they think that bright sunlight does interfere with the burning of an ordinary house fire, and that a cigar, unless continually puffed, will go out much sooner if the sun is shining on it, than otherwise. I enclosed a query with 136 concerning the nature of noises made by trains, but apparently it was not worth insertion.

X.

DARWINISM AND THE MICROSCOPE.

[253]—I beg to offer a few observations upon Darwinism, although drawn from a new source. The microscopic examination of the blood corpuscles belonging to different classes of the vertebrata.

Now it is well known that these discs vary greatly in form, size, and structure, not only in those classes themselves, but even in the different species of the same class. Thus in birds, reptiles, and fishes, they are, as Dr. Carpenter in his "Animal Physiology" observes, "much larger than in the Mammalia, their form is oval instead of round, and instead of being depressed in the centre, they bulge out on each side." In man, for example, their diameter is, according to him, 1-3200 of an inch, and in other mammals it varies from 1-4000 to 1-5000 of an inch, although in the musk deer (*Moschus javanicus*), it is only about 1-12000 of an inch. In birds their long diameter is from 1-700 to 1-2400 of an inch; in reptiles from 1-1000 to 1-1800 of an inch; and in fishes the long diameter of the blood discs is about 1-1900 to 1-2000 of an inch." It is remarkable too that the smallest British mammal, the harvest-mouse has as large corpuscles as those of the horse, and that in the common mouse they are even larger than in the horse or ox."*

* "Henfrey's Micrographic Dictionary."

The red corpuscles of the blood are, moreover, highly important organisms, inasmuch as they are supposed to be the carriers of oxygen from the respiratory [illegible] to every structure and tissue of the animals in which they are [illegible]. Seeing, then, that their function is so essential, and that they differ so widely in size and form throughout the Vertebrata, are we not justified in asking how, if Darwinism be true, the fishes, reptiles, birds, and mammals, could have been derived successively from "one common ancestor," according to Mr. Darwin's definition? In other words, how [illegible] these differences, so necessary, as far as we can judge, to the very existence of these separate classes, have been brought about by any set of internal or external influences. For instance, by what unseen agency, or by what special "environment," were the very large corpuscles in the Reptilia changed into the minute circular blood discs of the Mammalia?

It is also an admitted fact, that the capillaries or terminal blood-vessels are, in every animal, formed in strict relation to the size of its blood-corpuscles, so that the blood of one animal cannot support the life of another whose blood-discs differ in size from its own. We have, then, to ask the defenders of Darwinism to account, not only for these specific differences in the blood-corpuscles themselves, but also in the capillaries through which they circulate? W. H. O.

SUNGOD FESTIVALS.

[254]—It having been pointed out to me that in the number of your excellent journal for Dec. 30th there had appeared an extract from an article in the *Times*, entitled "Babylonian Sun-worship," I beg to take the opportunity thus offered of saying a few words thereon. When I first heard of the appearance of this article in the *Times*, it was then too late to point out its inaccuracy in that journal, and a letter, sent to one of the leading periodicals, was not inserted. I had hoped, however, that it would have gone no further, but as the most inaccurate portion has been given again in your journal (which, I am glad to see, has already a very wide circulation), I have thought it well to warn the public, who cannot judge for themselves, against such newspaper articles.

However surprising it may seem, it is nevertheless true, that the word translated by the *Times'* correspondent as "festival," does not mean anything of the kind. It is the usual word for "clothing," or "investment," and in the text from which this translation was made it is stated that these clothes or vestments were "the gift of the king." It will easily be seen that this correction changes at once the whole bearing of the passage, for though it is not impossible that the dates upon which these gifts were made were festivals, yet, as there is no statement to that effect, they are of but little use in determining the times of the festivals of the Sungod in Sippara. Of course, the rendering of the other words in the lines translated (excepting the dates) are in every case mere guesses.—I am yours, &c. THOS. G. PINCHES.

Queries.

[195]—QUICKSILVER, NON-POISONOUS. I am able to give rather a curious instance of the non-poisonous effects of mercury in the form of quicksilver. Having always been informed that quicksilver would be poisonous if swallowed, I was rather struck with the following. In a "lab" where I was studying some years ago, a young lad employed there told me that he swallowed quicksilver without feeling any the worse for so doing. From what I had previously heard, I did not believe him. He took some out of the Hg. bottle and swallowed it! Thinking that if he had really swallowed it he had taken a large enough dose, I waited until the next day, when I myself put some in his mouth which he swallowed! Could S. E. P. tell me if the mercury would pass through the system unaltered, also if it would be safe for any one to try the experiment on himself?—F.C.S.

[196]—CHEMICAL HEATING APPARATUS. Where can I find an account of such an apparatus for rooms? Has any method of cooking by chemical heat yet been discovered and applied? J. H. B.

[197]—REFRIGERATOR.—Where can I find an account of the most improved refrigerator for ships—such a one as enabled salmon to be recently brought from Hudson's Bay (I think)?—J. H. B.

[198]—TRICHINÆ.—Does the operation of curing destroy the trichinæ which may happen to infest the green hams or sides?—J. H. B.

[199]—TOBACCO AND SCIENCE. When a smoking-pipe is placed with the stem vertical and the bowl uppermost, the smoke issues downwards from the mouthpiece and then immediately ascends. When it is inverted, so as to have the mouthpiece uppermost, no smoke issues therefrom. Why is this?—P. J. GRALL.

[200]—LEASES. Can you inform me through KNOWLEDGE how to determine the value of leases? JAMES GREGG. [The conditions must be specified. ED.

[201]—COLOUR-HEARING. Will be glad if anyone can refer me to any books or magazine articles giving information as to Bernstein's researches into the phenomenon of colour-hearing.—B. J.

[202]—GRAVITY. Required the distance from the earth's centre at which the earth's attraction is balanced by that of the moon when at her mean distance. [If moon's centre be at distance d from earth's, and x the required distance, we have

$$\frac{\text{earth's mass}}{x^2} = \frac{\text{moon's mass}}{(d-x)^2}, \text{ or } \frac{d-x}{x} = \sqrt{\frac{1}{81}} = \frac{1}{9}; \text{ whence } x = \frac{9d}{10}$$

ED.]

[203]—SATELLITES—URANUS'S AND JUPITER'S MOONS.—Will you be kind enough to tell me how many moons of Uranus have been discovered, and what are the names of Jupiter's and Uranus's moon's? C. W. JEWITT. [Uranus and Jupiter have each four known moons. Those of Uranus have been called Ariel, Umbriel, Titania, and Oberon (do not know why). Those of Jupiter were called the Medicean stars, and by other names; also Io, Europa, Ganymede, and Callisto. They are always called by astronomers by the impressive names, I., II., III., and IV.—ED.]

[204]—ALGOL.—I should feel obliged for a list of the minima (those occurring in the night hours would suffice) of this variable star for the present season.—L.

[205]—OBSERVATION WEATHER.—I have read the remarks upon favourable nights for telescopic observation, both in the Editor's "Half-hours with the Telescope," and Webb's "Celestial Objects," but am anxious to find out whether any more definite laws are known on the subject, as, for instance, whether barometrical pressure or change makes any difference?—a wind or calm seems to have little to do with it.—L.

[206]—OPIUM.—Will some reader, "up in medicine," kindly inform me why opium, a powerful stringent, should in the case of lead-poisoning act as a purgative?—YOUNG PILL-BOX.

[207]—HEAT.—Will you kindly, through your columns, let me know whether there is any truth in the statement that a man may plunge his hand into molten iron with impunity? Has this anything to do with the spheroidal state?—PERCY V. DODD.

[208]—COMPOUND PENDULUM.—Will you kindly refer me to the best work for a description of a "compound pendulum," and the beautiful figures obtained by its aid? Would it be too unscientific to deal with in KNOWLEDGE?—JAS. A. GEE.

[209]—SATELLITES OF URANUS.—Please say whether any, and if so what, theory has been advanced in order to account for the retrograde motion of the satellites of Uranus. I believe the axis upon which that planet revolves lies in a plane nearly coincident with that of his orbit, and presume that the orbits of the said satellites are about at right-angles thereto, also that the rotatory motion of the planet itself accords with that of the satellites. Are these things so?—W. A. M. D.—[Very little is known about the axial rotation of Uranus. It is generally supposed to be as W. A. states. The satellites certainly move so.—ED.]

[210]—HOMER'S ILIAD. Could you or any of the readers of KNOWLEDGE inform me of the name, price, and publisher of any volume containing a *prose* account of Homer's "Iliad," "Labours of Hercules," "Jason's Expedition," &c.?—W. GIBSON.

[211]—PSYCHOLOGY.—Can you recommend me a good Catholic work on Psychology written in English, or a translation into English, and of moderate price, that is, not exceeding twelve or fifteen shillings?

[212]—What is the 12th (last) axiom of the First Book of Euclid? It is given differently in different books.—URSA.—[Euclid's 12th axiom is a veritable crux, and another is often substituted. It ran, "If a straight line falling on two other straight lines make the adjacent angles on the same side together less than two right angles, these two straight lines being produced, will at length meet on that side on which are the angles which are less than the right-angles.—ED.]

[213]—ORGANIC COMPOUNDS.—In Mr. W. Mattieu Williams's article in No. 3 of KNOWLEDGE, it is said that "many organic compounds have been made in the laboratory from mineral materials." Will you kindly inform me if such have been made from non-vegetable materials? I know that madder has been made from coal-tar, but then coal-tar is a vegetable material, and in this

instance, therefore, the vegetable laboratory has been at work before that of the chemist.—Siquis.

[214]—Voltaic Electricity.—Is any elementary treatise (with experiments) on voltaic electricity, similar in style to Tyndall's "Lessons on" (frictional) "Electricity" published?—Reynell W. Hay.

[215]—Book on Blowpipe Analysis.—Can you recommend me a good practicable work on blowpipe analysis?—F. Graham Fairbank, C.E.

[216]—Works on Botany.—Would you acquaint me with the latest work on systematic botany, or is there a later edition of Bentham's Botany than 1866?—E. A. Snell, M.B. (London).

[217]—Palæobotany.—(1.) Where can I find *Heterangium* and *Kataxylon* figured and described? (2.) Is it difficult to obtain *Sipillaria* (internal)? Any information will oblige.—F. R. M. S.

[218]—Tennyson.—Please explain the lines :—

. . . . "Look you, there is a star
That dances in it (the comet)."
"Harold," Act I. Sc. 1.

. . . "and over those ethereal eyes
The bar of Michael Angelo."
"In Memoriam," Canto 87.

. . . "the sea-blue bird of March."
Ibid. Canto 91.

May I venture to endorse the sentiments in the Laureate's lines :—

"Who loves not Knowledge?
. . . . . May (it) mix
With men and prosper!
. . . Let (its) work prevail."
"In Memoriam," Canto 114.

—Rufverte.

Replies to Queries.

[84]—Ancient Man.—Might not the depth at which the "pieces of burnt brick and pottery," &c., were found, viz., 60 ft., be partly explained by the action of worms, according to the researches of Darwin, as recorded in his latest work? This would diminish the estimated antiquity.—E. A. Snell, M.B. (Lond.)

[130]—Languages of the Earth.—The languages of the earth are estimated at 3,064, of which

587	are spoken in	Europe.
937	,, ,,	Asia.
276	,, ,,	Africa.
1,264	,, ,,	America and Australia.
3,064		

—Yours faithfully, Jno. Holmes.

[146]—The following is the result of experiments with substances injected into the jugular vein of a rabbit, the urine being collected and afterwards examined :—Maltose is partly converted in the blood into grape-sugar, and partly passes out unchanged. Soluble starch yields dextrin and grape-sugar. Achroodextrin (α) suffers only partial change, grape-sugar and maltose being found in urine, together with dextrin. Achroodextrin (β) yields a similar result. Achroodextrin (γ) yielded no sugar. We may conclude, as a rule, that the changes starch undergoes in the body are similar to those it undergoes when under the action of diastase. Diastase has this effect on starch—viz., that starch, submitted to its action, yields soluble starch, maltose, grape-sugar, and three forms of dextrin, α, β, and γ achroodextrine respectively. The term "achroodextrin" means a dextrin not coloured by Iodine.—F.C.S.

[153]—Doubtful Organisms.—The many attempts to define the line of demarcation between plants and animals have all broken down one after another, and modern definitions have no chance of a better fate. Hence Haeckel's group of intermediate forms, which, however, is not followed by most English biologists. Hooker, Cooke, and other botanists regard the *Myxomycetes* as vegetable fungi. A newer and probably better view is that of Mr. Saville Kent, who insists on their animal nature as closely intermediate between certain types of Infusoria and Sponges. The *Mycetozoa*, as he calls them, comprise several genera, as Æthalium, Stemonitis, Prichia, Arcyria, Lycogala, Didymium, Reticulariæ, &c. The subject is one of great difficulty and much interest.—Eclecticus.

[155]—Tortoises.—There is a large tortoise now in Ceylon (at east, I have not heard of its death since I was there four years ago) which was brought to the island by one of the last Dutch Governors consequently about eighty years before. It came, I think, from Java, and its age was then unknown, but it was supposed to be even at that time of very great age. I knew it for over twenty years. There was, I believe, an account, with illustrations, of this tortoise, either in the *Graphic* or *Illustrated London News* at the time of the Prince of Wales' visit to Ceylon. A turtle was taken on the coast of Ceylon some years since which had on one of its flappers a ring, which, by the date on it, was placed there some thirty years before. —B.M., F.R.C.S.

[177]—The Nautilus.—"M. Webb" asks the use of the *siphuncle*, or tube running through the *septa*, or partitions of the shell of the pearly nautilus. This tube is believed by some to have the function of maintaining a low vitality between the disused chambers of the shell. Others say that it is used to alter the specific gravity of the animal, and that by receiving or expelling fluid from or to the body, in which it terminates, the nautilus is enabled to sink or to swim in the sea. The exact functions of the sipuncle are, I believe, still unknown.—Andrew Wilson.

[165]—Horseradish.—This herb, I believe, possesses merely astringent and stomachic properties. Its use as an adjunct to food corresponds with that of mustard, and it may, therefore, assist in stimulating the flow of gastric juice.—Andrew Wilson.

[169]—Light and Lantern.—1. Tyndall's "Six Lectures on Light," delivered in America (Longmans, London, 1875). 2. "Light," by Mayer & Barnard (Appleton & Co., New York, 1878). 3. "Art of Projecting," by Professor Dolbear (Dillingham, New York, 1877). 4. "Optics for the Lantern," by Lewis Wright (Macmillan, London, 1882).—Thos. S. Bazley.

[180]—The Pole.—The reply given to the question, "How Arctic explorers could tell exactly when they reach the pole?" (p. 234), can only apply to the (nearly) six months that the sun is invisible at the pole, for during the remaining half of the year, the constant sunlight would prevent any astronomical observations of a *star* being taken, as no stars could be seen. If the pole is ever reached, it is likely to be during the time of sunlight there, and it is, therefore, by observations of the sun's altitude at different positions round the heavens—correcting such altitudes for changes of the sun's declination during the intervals of observation—that the explorer will be able to tell, with very considerable accuracy, when he has arrived at the extreme north of the world.—J. Rae. [That is one method among many; but, after all, determining when the pole is reached is really determining the latitude. At any time, the latitude can be determined by taking the sun's apparent altitude at noon. If this, corrected for refraction, &c., $=A$, and sun's declination (north position) at the time $=\delta$, then the latitude, λ, is given by the formula—

$$\lambda = 90^\circ - (A - \delta)$$

At the pole, where the latitude = 90, we have—

$$A = \delta$$

So that if the sun's observed noon altitude, corrected, is equal to his known northerly declination, the observer is at the pole.—Ed.]

[182]—Evergreens.—I believe the origin of the "evergreen" condition in plants is traceable to the general principles which regulate "natural selection." If we suppose that any plant during its spring and summer life acquired gradually an extension of its period of active nutrition, along with a fixation, so to speak, of the products of nutrition in the leaves (chlorophyll, &c.), the origin of the evergreen state is not difficult to conceive. We find an analagous case in the storage of starch as reserve-material in bulbs and tubers. Such a food-supply enables a plant (like a person with a deposit-receipt at his bank) to flower earlier than its neighbours, and there is no difficulty in conceiving that this habit of storing food may have grown and intensified by slow degrees.—Andrew Wilson.

[184]—Almanacs and Celestial Maps.—Middleton's Atlas is published by Whitaker & Co., Paternoster-row; Gall's (only a shilling), by Gall & Inglis, 25, Paternoster-square. Both are superseded by Proctor's "Half-Hours with the Stars" and "Library Atlas."—Thos. S. Bazley.

[188]—Optical Illusion.—This is very antique: it is the *shadow* of the pin which is thrown on the retina, and not being inverted, is paradoxically seen upside down.—S. J.

Electric Clocks.—A system of electric clocks has been organised for Genoa, and is now being carried into execution. The clocks will be arranged in five distinct lines, all branching out from the Municipal Palace. The first will extend to the Porta Lanterna; the second to the Molo Vecchio: the third to the Carignano: the fourth to the Palazzo Tommaseo; and the fifth to the Piazza Manin.

Answers to Correspondents.

**** All communications for the Editor requiring early attention should reach the Office on or before the Saturday preceding the current issue of* KNOWLEDGE, *the increasing circulation of which compels us to go to press early in the week.*

HINTS TO CORRESPONDENTS.—1. *No questions asking for scientific information can be answered through the post.* 2. *Letters sent to the Editor for correspondents cannot be forwarded; nor can the names or addresses of correspondents be given in answer to private inquiries.* 3. *No queries or replies savouring of the nature of advertisements can be inserted.* 4. *Letters, queries, and replies are inserted, unless contrary to Rule 3, free of charge.* 5. *Correspondents should write on one side only of the paper, and put drawings on a separate leaf.* 6. *Each letter, query, or reply should have a title, and in replying to letters or queries, reference should be made to the number of letter or query, the page on which it appears, and its title.*

ARCH. P. MAIKER. Many thanks; your letter useful. MIME. SO BIRD ARCH. All your communications interesting.—J. A. MOWATT. Thanks. PHINEAS FOGG. Our classification seems to need no explanation. As you say, we have Correspondence Columns, Queries, Replies, and Answers to Correspondents; ought it to be necessary to explain that we receive some letters we like to publish as such, many queries, not a few replies, and a number of letters, which though not suitable for publication, require answering? The difficulty is to classify letters, &c., at all, we receive so many more than we can deal with properly. We may soon have to open a new section for the names only of correspondents. The title, "genre painters," is applied to painters who take for their subjects scenes of ordinary life in some special line. (2.) The name "Millais," is ordinarily pronounced "Millay." (3.) Dionysos, or Dionysus, is the Greek equivalent for Bacchus, as Aphrodite for Venus, Pallas Athene for Minerva. (4.) Do not know who made the Pears' soap "Dirty Boy." You see, perhaps, why some questions are dealt with here, instead of under the head "Queries."—W. C. The Star Maps are intended to be used by persons who, having looked at the maps *otherwise* than "in the dark," had observed that such and such constellations were to be looked for towards the south, east, west, north, and so forth. I do not think their use would be very great to anyone who took them out in the dark, and tried to decipher them without a light.—W. RIDD, C. E. ROBERTS, W. G. BELL, R. W. J., R. C. AULD, and others. It would be well if correspondents would kindly remember that remittances and business communications generally, must be sent to the publishers, not to the editor. All letters received after Saturday lie over, and are not even opened till the ensuing Friday or Saturday. If they contain stamps or P.O.O., with orders for KNOWLEDGE, there is naturally a week's dissatisfaction, through whose fault would you say? R. W. J. thinks it is very good of us "in our old age, to give to the world at a price which is equivalent to a free gift, so much of the vast quantity of knowledge which we have accumulated from within the four quarters of this second-rate planet." We are really very much obliged to Mr. Jenkins. We should not have taken credit, perhaps, for extreme old age, but for his kindly suggestion; but we have no objection. Sixteen years have passed since our first book appeared, and it seems to us now that we were very young then; we must have been mistaken, however.—P. W. JACKSON. The difficulty at present is to find space for mechanical engineering.—NEMO. We give a short sketch of Dr. Draper's life, as you request.—AQUARIUS. Twelve years having passed since we rowed in a race, and though sliding seats had then come in, we did not care to give up the old-fashioned fixed seats on which we had learned rowing, so we cannot give any practical explanation as to their working; but, unquestionably, when properly used, they greatly help the rower. We seldom get a chance now of rowing in outrigged boats, the only rowing we much care for.—J. J. D. The difficulty is that while, as you say, the picture in Guillemin's Heavens is manifestly incorrect, a correct picture of the moon's path round the sun seems to differ in no observable respect from the elliptic path assigned to the earth. See chapter on the moon's motions in my treatise on the moon.—ROBERT WALKER. Bezique is not in our line.—NEWTON CROSLAND. I also thought "we had parted for ever." Would insert your letter if it were about one-sixth of its length, but as it is, it is impossible. J. T. POWELL. Thanks, but we have forty ten thousand names of schools, &c., and expect before long to reach them all. We are growing fast, and we "seek not to proturpate." We agree with you that Mr. Grant Allen's papers are charming. M. A. TURNER. Did not Professor Wilson refer to Mr. Gosse's well known chapter about immured toads, in his first reply on the subject? It seemed certainly clear to us that he was well acquainted with it, as we suppose most students of natural history must be. J. WANKLYN. Quite so; these questions of classification are of themselves of no importance whatever. A new rule for classifying introduces no new facts. It is as though one who had been familiar with the arrangement of the bookshelves in a library, and with the contents of the books in them, should be told—when he came back a year or two after, perhaps, to the place, and some changes had been made in the arrangement of books—that he knew nothing about the books because he knew only of their old arrangement on the shelves. C. A. T. "Five of Clubs" has already corrected the omission. A. E. S. Brewster spoke of faith in other inhabited worlds as the hope of the philosopher and the faith of the Christian, or the faith of the philosopher and the hope of the Christian. I forget which exactly. It matters not; the theory is not "based on any religious principles."—R. E. J. You can hardly say that the sails of a windmill go with the sun, or against the sun. If the wind is from the north the eastern arm goes down, and the western arm up. If the wind is from the south the reverse holds.—F. COWLEY. If you will try to picture a star as emitting rays in all directions, which, once emitted, travel continuously outwards, you will see that there is no real difficulty. As to the waves arriving when the star is "below the horizon or anywhere else," you might equally say that sea-waves, leaving England for—let us say America—may arrive there when people in England are in bed or anywhere else.—C. UPHAM. Many thanks for your letter.—SIDNEY JEWBURN. Never mind the gilt edge, or the crest, or the scent; it is not the paper, but what is written on the paper, we care for. Thanks, all the same. As to the toad story, we heard the same story told about *Cimex lectularius.* The ring of light round the moon emphatically can *not* be explained as you suggest. Is not the cat's-eye phenomenon known to most persons? Your plan for testing a cat's power of turning is not cruel to the cat, but might be inconvenient to the experimenter. Cats do turn with wonderful rapidity, but we really cannot advise our readers, in their search for knowledge, to "seize a cat, the two front legs in the left hand, the two hind ones in the right, to hold her upside down, with her back two inches from the floor, let go, and observe how, in the two-inch fall, she will turn round and fall on her feet." Unless, indeed, you could guarantee her quiescence before she was "let go." NEW ZEALAND. The Wanganui is a splendid river for rowing. A year ago we had a splendid twenty miles spin on the Wanganui in a four-oared clinker-built outrigged boat, and judged it to be better for boat racing even than the Paramatta in New South Wales, on which river also we had some capital rowing. The Paramatta, indeed, is hardly a river, it is an inlet of the sea—at least as far up as the racecourse.—JOHANNISBERG. The *English Mechanic* is—or used to be, when I knew it—an excellent paper, but it does not give the latest method of calculating "the phases of Orion." You must be mistaken. In the first place, no reviewer could be so ignorant as to write such rubbish; but if any could, the sub-editor would know it was nonsense, and certainly would not think of allowing it to be quoted. Of course Orion has no phases, and, therefore, it stands to reason no method—earliest or latest—has ever been invented for calculating them.—M. McC. Phrenology would certainly not be proved by the evidence you mention. But there can be very little doubt that there is a certain correlation between the capacity for music, painting, &c., and the conformation of those parts of the head where phrenologists place the bumps corresponding to these arts. So, readiness of verbal expression is almost always found in full-eyed persons, as Gall noticed at the very beginning of his observations. There is a very strong argument from induction with regard to many of the localisations of phrenology, but they have no more real bearing on cerebral localisation than has the observed fact that blue-eyed white cats (male) are commonly deaf.—J. WHITLEY. We propose soon to have a paper on the approaching transit of Venus. If we put in your query we should have about fifty replies, every one of which, though only one could appear, would involve just as much trouble to us as though we used it. If you could see our parcels of letters (they are no longer forwarded from the head office in envelopes, even the largest sizes failing to hold them), you would understand our reluctance to insert queries. We note that with a power of 200 the apparent diameter of Venus in transit next December will be 1° 51′ 40″. The size of the O.G. and its focal length need not have been mentioned when the power was given.—F. RHYLES. You are right about the heading, but we know from the aspect of the ring when seen edgewise that there is no such inequality there. —WALTER G. WOOLLCOMBE. It does so chance that we had forgotten about Clerk Maxwell's Theory; but it is the merest accident that you should be right, for there are many subjects about which we are ignorant. We thought this was one of them, but, instead of ignorance, must now plead forgetfulness.—J. O. M. Excuse us; but no space for the Cuddapah question. So few readers would be interested, and so many other subjects crowding in upon us.—J. N. BARNES. The paragraph to which you refer appeared only in the first edition of the "Vestiges;" more experienced students of science explained to the author of that work that he had erred.—JOHN ROUSE. We have searched for one of the questions in vain. We remember receiving them, and had intended

to insert both. Probably it was among some which were lost out of a large envelope which burst open during postal transit.—M. H. C., FRED ROLLET, E. LUXMORE, M. B. E., and others. Thanks about the Zoetrope corrections. We propose to discuss shortly the principles of magic wheels, &c.—URSA. Query on Logic should be sent to a paper dealing specially with such subjects; the others inserted.—A. E. S. Daylight *seems* only to change unequally in the morning and evening. If we took true solar noon instead of mean noon, the increase would be found uniform.—CONTINENTAL SHADE. Depends on the eyesight, and no one has yet determined the average eye power for discerning shades.—G. W. NIVEN. How very carefully you make your articles too long for any use. A column of arithmetical curiosities would be pleasant enough, but four pages would be too much.—W. GIBSON. Letter on sun heat crowded out.—A. J. MARTIN. It is absolutely necessary that all "copy" should be in the printers' hands by first post on Monday. The greater part should reach them on Friday. We are far from being able to promise that letters or queries received before Saturday will appear in the next number. To say the truth the correspondence has grown so as to interfere most unduly with the conduct of the paper generally. If our paper were high-priced enough to command the services of six sub-editors (besides chess, whist, and mathematical editors), with a sixteen-page correspondence supplement weekly, we might give due attention to the progress of the more important departments. But it is not. The worst of it is that so many correspondents send us really valuable matter, that we *must* go through the multitudinous heap of matter not valuable, received weekly. If everyone who proposes to write to us were to ask first whether he really has something which he ought to say, or whether a question he is going to ask might not quite readily be answered if he took a little trouble for himself, if, then, having decided to write, he would put his communication in the fewest words, and afterwards strike out all the extraneous matter which the inexperienced pen *will* throw in, it would save us a world of really trying labour. Even then, judging from what reaches us, two out of three communications might well have reached the writer's waste-paper basket as ours. Kindly read again what F.R.A.S., says about wiping the object-glass of your telescope.—H. T. EDMONDS, W. B. RUSSELL, &c. Every one who has studied natural history at all knows that the distinctions you mention exist between the Batrachians and the animals now classed as the only Reptiles. The question how they are classified is one thing, the question why they are so classified is another. To myself, all such questions seem to me not only unimportant, but often mischievous—the student is led to think more of form and phrase than of substance; he is content to learn the long names which have been adopted in classification, and to attend very little to the observed facts of nature. One may begin to suspect a man's zeal for science (as well as his common sense) when he is anxious to display his familiarity with scientific terminology.—MOGUL. Many thanks for rectangles, &c. We had taken your first letter as only private in regard to name.—ONLY A WOMAN. The toad difficulties tolerably obvious. As to the other question, the upper weight would be brought to the ground by the effect of the impulse communicated to it. There being no friction, and the weights being equally balanced, there is nothing to destroy the momentum once imparted until the weight reaches the ground. The worm actually causes the anæmic condition, being a bloodsucker.—URSA MAJOR. Would you kindly put your questions into compact query form? One is suitable for the Mathematical Column, the other for the general queries.—J. O. M. So many magic square communications have reached us that "we know not what to do" with them. Each requires careful study, and would occupy a large amount of space; they interest but a small proportion of our readers. We must now cease to deal with them.—A. HOWARD. No space at present.—J. F. RUSSELL. See answer above about Electro-Magnetic Theory of Light.—O. DAWSON. We must not perplex correspondents by being too particular.—C. G. R. The plan has been thought of; but the trouble is that opportunities for observing are so few, and all the part proprietors of an amateur observatory would want to use it when observing conditions were favourable.—A. T. E. No reply to query 49, page 161, has been received. After all, a quarter of a man's weight (say he weighed 160 lb.) is not much to lift, and the conditions are of course more favourable after a long breath has been drawn, as you find in lifting a dead weight. This, and a lively imagination, will explain the phenomenon, I think.—I. G. O. No longer space for a question not very important in itself.—AN OCTOGENARIAN. Mercury can readily be seen with the naked eye at the time when the "Nautical Almanac" speaks of him as at his greatest elongation, east or west,—before sunrise when the elevation is west, after sunset when it is east, and of course near the sun's place below the horizon.—ACCIDENT. The correction already made.—C. E. H. Your solution correct and neat.—J. RICKARD. Cat story too long for its purport. (Cat troubled with her milk brought a peace offering—fruitlessly—to a before-hated kitten for relief.)—R. W. J. Dog story, ditto; the "fore and aft chaff" between yourself and friend about the bulldog has no scientific bearing. (Bulldog, having passed two men on his way to a gate, which he found locked, went back after them, and persistently bullied them till they opened it.)—CHEVALIER. Game sent to Mephisto.—A. T. C. Seriously, do you want me to say it was a miracle? A distant flight of birds crossing the sun's disc, so that you could have seen them in transit from the nail-holes, but not from the opening just above, would account for what you saw. Or, some other light intercepting passing object, at a suitable distance. If you had looked at once through one of the holes you would doubtless have seen it; looking otherwise you would probably have missed it. All I know (not having been there) is that no miracle probably occurred, and that therefore as a shadow was thrown there was something which threw it. Your solutions of the three-square problem are neat. It would surprise you very much, I take it, to hear that the number of solutions is *infinite*, "if you may cut them as you will."—SIMPLEX. Not knowing Bell's shorthand system, cannot say; can you describe it briefly?—A. Thanks about trotting horse; regret that your question about Induction Coils has remained unanswered.—W. WILSON. It seems to me the ordinary expression "I see the light" is the one which needs correction. Define light, and put the words "I see" before your definition, and see what comes of it.—F. HALLE. Scarcely in our line.—M.A., F.S.A. *Is* there a mathematical demonstration of the divisibility of matter?—F. L. C., or Z. L. C., or F. C. S., or F. C. L. About sunlight on fire and poker across it in our article on "Fallacies."—C. R. T. Seeing Debillissima with a $3\frac{1}{4}$ inch O.G. would, in my opinion, mean good eyesight. It is impossible to infer the qualities of a telescope from such observations. Mr. Sadler counts about a hundred mistakes in the new edition of Smyth's Bedford Catalogue. You do not say what sort of catalogue you mean.—T. R. CLAPHAM. Thanks.—SUBSCRIBER. Your query indistinct. Besides, to "give you all that is known on the subject," would be to give you two or three numbers to yourself.—A. T. WRIGHT. Cannot enter into discussion of various shorthand systems; they are not scientific matters.—J. H. GARFIT. Your article on the giraffe reminds us of his neck; obliged to decline it.

[Here, for the present, we stop. We beg to invite correspondents' attention to the fact that we have been able to go through only about two-thirds of the correspondence which reached us up to Saturday afternoon (Jan. 21), that only about half of those letters, queries, and replies which we should have liked to use can find a place in our columns, and perhaps only about half of that can appear next week. Original matter, notices of books, and paragraphs suited to our columns may make way in some degree, but they must not make way altogether, for correspondence, queries, and replies, for matter in fine, which we insert to oblige correspondents.—ED.]

Notes on Art and Science.

"COLD CATCHING."—It is noteworthy as a curious yet easily explicable fact, that few persons take cold who are not either self-consciously careful, or fearful of the consequences of exposure. If the attention be wholly diverted from the existance of danger, by some supreme concentration of thought, as, for example, when escaping from a house on fire or plunging into cold water to save life—the effects of "chill" are seldom experienced. This alone should serve to suggest that the influence exerted by cold falls on the nervous system. The immediate effects of a displacement of blood from the surface, and its determination to the internal organs, are not, as was once supposed, sufficient to produce the sort of congestion that issues in inflammation. If it were so, an inflammatory condition would be the common characteristic of our bodily state. When the vascular system is healthy, and that part of the nervous apparatus by which the calibre of the vessels is controlled performs its proper functions normally, any disturbance of equilibrium in the circulatory system which may have been produced by external cold will be quickly adjusted. It is, therefore, on the state of the nervous system that everything depends, and it is, as we have said, on the nervous system the stress of a "chill" falls. Consciousness is one element in the production of a cold, and when that is wanting the phenomenon is not very likely to ensue. It is in this way that persons who do not cultivate the fear of cold-catching are not, as a rule, subject to this infliction. This is one reason why the habit of wrapping-up tends to create a morbid sensibility. The mind by its fear-begetting precautions keeps the nervous system on the alert for impressions of cold, and the centres are, so to say, panic-stricken when only a slight sensation occurs.

cold air applied to the surface, even in the form of a gentle current of air only a little lower in temperature than the skin, will produce the "creeping of the flesh." Conversely, a thought will often give rise to a sensation of cold applied to the surface—for example, of "cold water running down the back." Many of the sensations of cold or heat ... are referred by the experiencer to some external cause ... are purely ideational in their mode of origination, and ... *Ed.*

... COMPRESSION OF SOLIDS.—According to the *Revue* ..., Mr. W. Spring, a German chemist, has recently published an interesting memoir, giving the results of a series of experiments undertaken to ascertain the effect of powerful compression on the most diverse bodies. The substances experimented with were taken in the form of fine powder, and submitted, in a steel mould, to pressures varying from 2,000 to 7,000 atmospheres, or about 7,000 kilogrammes per square centimetre. The facts observed are given in a series of tables, from which we extract some of the more curious results. Lead filings, at a pressure of 2,000 atmospheres, were transformed into a solid block, which no longer showed the least grain under the microscope, and the density of which was 11·5, while that of ordinary lead is 11·3 ... At 5,000 atmospheres the lead became like a fluid and ran out through all the interstices of the apparatus. The powders of zinc and bismuth, at 5,000 to 6,000 atmospheres, gave solid block having a crystalline fracture. Towards 6,000 atmospheres zinc and tin appeared to liquify. Powder of prismatic sulphur was transformed into a solid block of octahedric sulphur. Soft sulphur and octahedric sulphur led to the same result as prismatic. Red phosphorus appeared also to pass into the denser state of black phosphorus. As may be seen from this, simple bodies undergo chemical transformations by the simple action of pressure. The change of amorphous powders, like that of zinc into crystalline masses, is a sort of self-combination. Certain hard metals do not lose their pulverulent structure at any pressure. Binoxide of manganese and the sulphides of zinc and lead in powder weld when compressed, and exhibit the appearance, respectively, of natural crystallised pyrolusite, blende, and galena; while silver and the oxides and sulphides of arsenic undergo no agglomeration. A certain number of pulverised salts solidify through pressure, and become transparent, thus proving the union of the molecules. At high pressures the hydrated salts, such as sulphate of soda, can be completely liquefied. Various organic substances, such as fatty acids, damp cotton, and starch, change their appearance, lose their texture, and consequently undergo considerable molecular change.

Our Mathematical Column.

MATHEMATICAL QUERIES.

[20] I find in "Todhunter's Differential Calculus" the following problem:—What is the greatest equilateral triangle which can be circumscribed about a given triangle? I am not able myself to employ the methods of the "Differential Calculus." Is there any way of solving this problem by geometrical methods?—NO ANALYST.

[Try the following:—On the three sides of the triangle, describe, outside the triangle, segments each containing an angle of sixty degrees. Then it can readily be seen that if any straight line be drawn through an angle A of the triangle, to meet the two arcs on AB, AC in P and Q respectively, then PB and QC produced will meet on the arc which has been drawn upon BC, and the triangle thus formed will be equilateral. All we have to do then is to determine the greatest straight line which can thus be drawn through A; and it needs very little familiarity with geometrical methods to see that the greatest straight line which can be thus drawn is the straight line parallel to the line joining the centres of the arcs on AB, AC. From this the formula in "Todhunter's Differential Calculus" for the side of the maximum equilateral triangle follows at once.—ED.]

[21] "Zares" asks us to give the formulas for solving the problem, How fast should the earth rotate that the centrifugal force at the equator should just counterbalance the attractive force of gravity?

The following is the solution of "Zares'" problem:—

Let T be the time in seconds in which the earth should rotate that the force of gravity g should be exactly balanced at the equator by centrifugal force. Let the earth's equatorial radius $= r$. Then the velocity of a point at the equator $= 2\pi r \div T$, and the centrifugal tendency which gravity has to resist is represented by (the vel.)² divided by (the radius). So that when the force of gravity is exactly balanced, we have

$$g = \left(\frac{2\pi r}{T}\right)^2 \cdot \frac{1}{r} = \left(\frac{2\pi}{T}\right)^2 r$$

wherefore $T = 2\pi\sqrt{\frac{r}{g}}$.

Taking a second for the unit of time, a foot for the unit of length, and the earth's equatorial radius as 20,925,000 ft. (the equator is not perfectly circular, its greatest and least diameters differing by about two miles), we have, in numbers,

$$T = 6{\cdot}2432\sqrt{\frac{20{,}925{,}000}{32{\cdot}2}}$$

$$\begin{aligned} \text{Now, } \tfrac{1}{2}\log.\ 20{,}925{,}000 &= 3{\cdot}6603328 \\ \tfrac{1}{2}\log.\ 32{\cdot}2 &= 0{\cdot}7539279 \\ \text{Difference} &= 2{\cdot}9064049 \\ \log.\ 6{\cdot}2432 &= 0{\cdot}7954072 \\ \text{Sum} &= 3{\cdot}7018121 = \log.\ 5032{\cdot}83 \end{aligned}$$

Therefore, $T = 5032{\cdot}83$ seconds
$= 83$ m. $52{\cdot}83$ s. $= 1$ hr. 23 m. $52{\cdot}83$ s.

If the earth rotated, then, in this time, or roughly in 1 hr. 24 m., bodies at the equator would be absolutely without weight.—ED.

[22] EQUATIONS: ASTRONOMICAL PROBLEMS. Can any of your readers give solutions of the following equations?

$$(\alpha)\qquad \frac{x^2}{a^2}+\frac{y^2}{b}=\frac{a}{x}+\frac{b^2}{y}=a+b$$

$$(\beta)\qquad 2x\sqrt{x^2+a^2}+2x\sqrt{x^2+b}$$

(i.) When is Venus brightest

(ii.) My watch loses 5' per day. I travel eastward at such a rate that it keeps correct time. In what time shall I complete the circuit?

(iii.) A star's meridional zenith distance and north declination are equal (θ), how long is the star above the horizon?

(iv.) Find the difference between the synodic periods of Jupiter and Saturn with the earth, assuming mean distances as 1 : 5 : 9.—RUTVERTE.

---

[15] By a ridiculous oversight, after sending this question (see p. 258) to the printers, we dealt only with one of the equations we had written down. Of course, there are two, viz.:—

$$x(y+5) = 1000$$
$$y(100-x) = 1000$$

Or, $x+20y = 100$, whence $(100-20y)(y+5) = 1000$. The rest is obvious.—ED.

Our Chess Column.

PROBLEM NO. 14.

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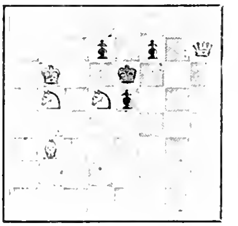
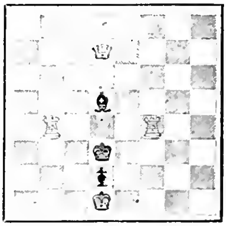


WHITE.

White to play and mate in two moves.

PROBLEM NO. 15.

BLACK.



WHITE.

White to play and "self" mate in two moves.

The two-mover is a prize problem from the *Huddersfield College Magazine* Tourney of 1877. A remarkable fact occurred in connection with this problem. The *Huddersfield College Magazine* published this problem in October, 1877, as composed by W. A. Skinkmann. Simultaneously with this, the *Free Press* published exactly the same position as composed by Mr. G. E. Carpenter. It was afterwards ascertained that Mr. Carpenter composed his problem two years prior to Mr. Skinkmann, and it was also admitted that Mr. Skinkmann had no cognisance of Mr. Carpenter's problem. This forms a remarkable coincidence of ideas by two

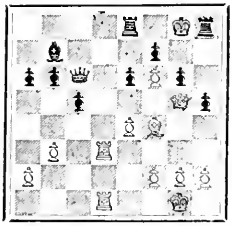
eminent composers. It is now known under the name of the Carpenter-Skinkmann problem. The other is from the *Chess-Players' Chronicle*.

The following pretty ending occurred in a game played in the match of Liverpool *v.* Manchester (H. Jones, Manchester) (Rev. J. Owen, Liverpool). We copy from the *Field*.

Position after Black's 28th move.

Rev. J. Owen.

BLACK.



WHITE.

Mr. H. Jones.

White continued with

29. R. to Q.8.	29. K. to B. sq.
30. Q.R. to Q.7 (a).	30. P. to K.4
31. Q. to R 6 (ch.) (b).	31. R. takes Q.
32. B. takes R. (ch.)	32. K. to Kt. sq.
33. R. takes R. (ch.)	33. K. to R.2.
34. B. to Kt.7.	34. P. to K.Kt.4.
35. R. to R.8 (ch.)	35. K. to Kt.3.
36. R. to R.6 (mate).	

(a) The winning *coup*, which blocks out the adverse Q., and threatens the decisive B. to Q.6 (ch.).

(b) A highly ingenious master-stroke. Mate in six more moves is forced after this.

Game played at Mephisto's Rooms, 48A, Regent-street, between Mr. W. Cook and Mephisto:

WHITE. Mr. W. Cook.	BLACK. Mephisto.
Bishop's Gambit.	
1. P. to K.4.	1. P. to K.4.
2. P. to K.B.4.	2. P. takes P.
3. B. to B.4.	3. P. to Q.4.
4. B. takes P.	4. Q. to R.5. (ch.)
5. K. to B.sq.	5. P. to K.Kt.4.
6. P. to Q.4.	6. B. to Kt.2.
7. Kt. to Q.B.3.	7. Kt. to K.2.
8. Kt. to B.3.	8. Q. to R.4.
9. B. to B.4 (a).	9. P. to Kt.5. (b)
10. Kt. to K.sq.	10. P. to B.6.
11. P. to Kt.3.	11. B. to Q.2.
12. B. to K.3.	12. Kt. to Q.B.3.
13. P. to Q.R.3.	13. Castles Q.R.
14. P. to Q.Kt.4 ( ).	14. B. to K.3 (d).
15. B. takes B.	15. P. takes B.
16. P. to K.5 (e).	16. Kt. to B.4.
17. B. to B.2.	17. Q.Kt. takes Q.P
18. Kt. to Q.3	18. Q. to R.6. (ch.)
19. K. to K.sq.	19. B. to R. 3.
20. Kt. to K.4	20. Kt. to K.6.
21. B. takes Kt.	21. B. takes B.
22. Kt. to K.B.4 (f).	22. Q. to Kt.7 (g)
23. Kt. takes Q.	23. P. takes Kt.
24. Kt. to B.2 (h).	24. K.R. to. B.sq. (i).
White resigns (j).	

NOTES BY MEPHISTO.

(a) The new edition of Mr. Cook's synopsis of the openings gives Q.P. to K.R.4, which, in our opinion, is the better move, as then Black could not venture on capturing the Bishop, as it is part of the plan of attack in this opening to get the Queen's Knight to Q.5.

(b) The hasty advance of these Pawns is sometimes inadvisable, as the White King, although apparently exposed, is nevertheless fairly safe. A somewhat similar position of the King runs in the Salvio Gambit. Mr. Steinitz here prefers Kt. to Q.B.3.

(c) This move lost the game. Black's intention of Castling on the Queen's side was obviously to obtain an attack on the White centre, which P. to Kt.4 facilitates, as, on account of the pinning action of Black's Bishop on Kt.2, the White Queen's Pawn and Knight are fixed in a disadvantageous manner.

(d) This move forces the position. The Queen's Pawn cannot be defended.

(e) Played, perhaps, with the intention of exchanging Rook and piece for Queen. He had no good move.

(f) Played with the intention of preventing Q. to Kt.7. If, now, Black plays, B. takes Kt. followed, on Pawn retaking, by Q. to Kt. 7. Then White plays Kt. to B.2. White's position is very bad in any case.

(g) This position is as sound as it is forcible. He threatens Q., takes R. (ch.), and Kt. takes P. (ch.), which compels White to take the Queen.

(h) As good as any other move. If 24. R. to B.2, then Kt. to B.6 (ch.) 25. K. to K.2 / R. takes Q. 26. R. takes R. / P. Queens. 27. R. takes Q. / B. takes R. with a piece ahead.

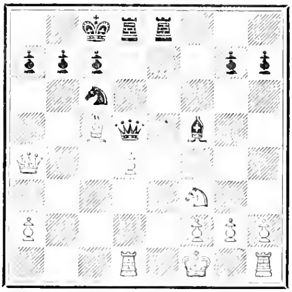
(i) P takes R., followed by Kt. to B.6 (ch.), would also have left Black with a piece more.

(j) To find out the precise mode of winning, whatever White may do, might be interesting to our young readers.

The death is announced of Mr. S. G. Boden, a chess-player of great excellence. The *Field* gives the following brilliant end game as a specimen of fine play on the part of the deceased:—

BLACK.

Mr. Boden.



WHITE.

Rev. G. A. MacDonnell.

It was Black's turn to play on the 20th move, and the game proceeded thus:—

WHITE.	BLACK.
	20. Q. takes Kt.
21. P. takes Q.	21. B. to R.6. (ch.)
22. K. to Kt.sq.	22. R. to K.3.
23. Q. to B.2.	23. R. takes Q.P.
24. B. takes R.	24. Kt. takes B.
Resigned.	

The sacrifice of the Q. in conjunction with the ultimate giving up of the R. belongs to the finest specimen of chess tactics in actual play.

A. J. MARTIN and J. P.—In Problem 5, if 1. K. to K.3, (1) B. takes P. (ch.), then 2 R. covers, disclosing check, and Black King goes to K.3; there is then no mate. Of course, the first move in our solution should have been K. to K.2., not Q.2. He cannot go to Q.2. How does J. P. make out that if K. goes to K.2, there is no mate in three? In Problem 11 no mate in two.—ED.

VICAR. Your treatment of Mr. Maas's end game is correct. What seems White's obvious first move leads to defeat. Problems 10 and 11 correctly solved.—ED.

CAROLUS.—Your solution of Problem 11, p. 240, is erroneous. After the two Knights have checked, when Bishop checks as you propose, what is to prevent Black from playing R. takes B.?—ED.

W. G. [illegible] Y [illegible] of No. 6 and No. 11 [illegible] [illegible]
G. W. M. [illegible] Y [illegible] 6, 7, and 8, correct. [illegible]
[illegible] variation on p. 215 [illegible] 12. K [illegible] takes Q B P [illegible]
12. B [illegible] Q [illegible] B [illegible] Black would [illegible]

12. Q [illegible] B [illegible] Mephisto.
[illegible] 11 and 12 in No. 12 should have been [illegible] 12 and 13.
[illegible] Editor [illegible], which has till now been divided [illegible] M[illegible] and the General Editor, will henceforth be [illegible]. Letters belonging to this department should be [illegible] the Editor of Knowledge, 74, Great Queen-street.

Our Whist Column.

By "Five of Clubs."

AN OMISSION FROM OUR LEADS IN PLAIN SUITS.

We omitted to note among our leads four, one lead of Knave, two of Ten and one of Nine. Besides the two cases [illegible] Knave [illegible] led from King, Queen, Knave, with or without [illegible] besides the cases noted, Ten is led from King, Queen, Knave, Ten, with or without others. The object is, in either case, to get the Ace, even though held by partner, out of the way, after which the entire command is retained in the suit. Again, Ten is led from King, Knave, Ten, with or without others. Lastly, Nine is led from King, Knave, Ten, Nine, with or without others.

We give this week a game which, as it happens, illustrates the lead of Knave just mentioned. It is intended, however, to illustrate what we said in our last about playing a waiting game in trumps. It may be mentioned as rather amusing, that in the actual game, one of the players, forgetting the strict rule of whist, remarked, when the second round of trumps was played without the Ace falling, "Well, some one must be an unmitigated ——" (the rest was left unexpressed). He was one of the losers, and slightly changed his tone about the tenth round. It is hardly necessary to say, however, that he should have been silent all the time, whatever his opinion of the play.

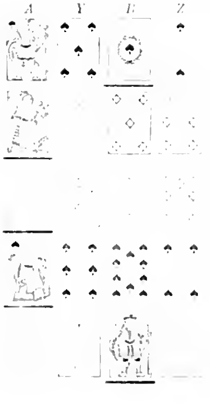
THE HANDS.

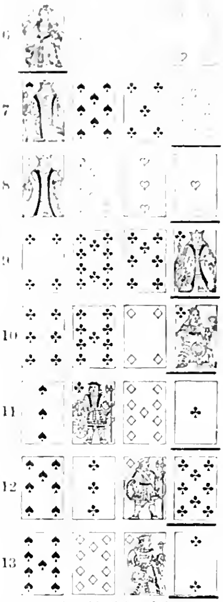
A.		Y.
Hearts—K, Q, 7.	B	*Hearts*—8, 6, 2.
Spades—K, Q, Kn, 9, 7, 3.		Spades—8, 6, 5.
Diamonds—A, Q.	Dealer	Diamonds—10, 8, 3.
Clubs—6, 4.	Y Z	Clubs—Kn, 10, 9, 3.
B.	*Trump Card, Four of Hearts.*	*Z.*
Hearts—[illegible] 9, 3.		*Hearts*—A, 10, 5, 4.
Spades—A, 10.	A	Spades—4, 2.
Diamonds—K, Kn, 9, 5, 4, 2.		Diamonds—7, 6.
Clubs—7, 5.		Clubs—A, K, Q, 8, 2.

Score: A B, 4; Y Z, 4.

Note.—The underlined card wins trick, and card below it leads next.

A Y B Z





Y-Z win the odd trick and the game.

REMARKS AND INFERENCES.

1. *A* may have led from Knave, ten, nine, so far as *Y* or *Z* can tell. *B*, holding the ten, knows the lead is from King, Queen, Knave. He puts on Ace to give his partner command of the suit. If he played Ten, *A* would continue with Queen, and Ace would take it, whereas by playing ten, on returning the suit, *B* leaves his partner the option of taking the trick, or letting the Ten take it. As the cards lie, *B* does not get the chance of returning his partner's lead, after showing his own strong suit.

2. *B* leads the ante-penultimate, Drayson's plan for showing six of a suit.

3. Seeing that Four of Diamonds cannot lie with *Y* or *Z* (from their play), *A* should place it with *B*, and credit *B* with all the remaining diamonds but one.

4. *A* has all the remaining spades but one, which may be either with *Y* or *Z*.

5. *A* does not continue his established suit, fearing to force his partner *Y* trumping first. But he had two chances in his favour if he had led them. First, he would as probably be forcing an adverse strong trump hand, as one held by *B*; secondly, *Z* may have the remaining spade, in which case *B* would be over *Y* in ruffing, and still only trump for trump be drawn. *A*'s fault here loses the game, though it requires keen play by *Y* and *Z* to win it with such a wretched hand as *Y* has. *Z* does not win the trick, knowing that his only chance lies in taking the last round of trumps. He plays Five (Four being the trump card), to show his partner one more card.

6. *B* leads the highest of two cards left; the fall of the cards shows *A* that *B* must have the Three, and therefore no other, and *A* should tremble. Yet with such cards as he knows to remain in his hands and *B*'s, with reasonable probability that at least one good club lies with *B*, *A* does not yet despair. He knows, however, that Ace must lie with the enemy, and the way it is kept back is ominous of trouble.

7.—*A* resumes his long suit when the mischief is done. If he had drawn a trump from *Z*, he might himself, holding King and Queen, have played the waiting game. *Z* trumps, disregarding the probability that *A* held originally four trumps,—for this simple reason, that, as the score stands, *Y* and *Z* must make every other trick.

8. *A* should tremble still more; yet even now a single trick will save and win *A-B*'s game, and nothing but very careful play can win *Y-Z*'s.

9. *Y*'s play of the Club Ten is excellent. He knows that *Z* will place the Knave in *Y*'s hand so soon as another round has fallen, if not at once, so that if *Z* has only Ace and King at the head of his club suit, and draws the Queen from the enemy, second round, *Z* will still credit *Y* with the power of making another trick in Clubs and returning a small one; while if *Z* has Ace, King, and Queen at the head of his Club suit, *Y* will be able to throw away the remaining high cards. If at this stage *Y* had played the Three, *Y-Z* would have lost the odd trick and the game. As the cards lie, he can do no harm by leading either Knave or Nine. Even if *A* has not the Queen, and it falls at the second round, *A*'s knowing that *Y* has the Knave may not be essential to *Y-Z*'s success*; but it is good whist to give the information, all the same. Observe: the Knave could not possibly be played by a good whist player; the nine would be the usual play; but by playing the Ten, *Y* shows his partner the position of the only card to be dreaded, if *Z*, having Ace and King, can draw the Queen. *A* having Ace, King, Queen, the Knave is not wanted, and *Y* throws it away, leaving *Z* to make both Eight and Two.

* Put Queen of Clubs in *A*'s hand, and Six of Clubs in *Z*'s, then *Y-Z* would equally win the game, thus:—

	Z	*A*	*Y*	*B*		*Z*	*A*	*Y*	*B*
10.	C A	C Q	C 9	D 4	12.	C 8	S 7	C 3	D Kn
11.	C 2	S 3	C Kn	D 9	13.	C 6	S 9	D 10	D K

NOTICES.

The Publishers beg to announce that in future Monthly Parts of Knowledge will be issued. The following can now be had:—

Part I.—(November, 1881.) Containing the first four numbers. Price 10d. Post-free, 1s.

Part II.—(December, 1881.) Containing five numbers. Price 1s. Post-free, 1s. 2d.

Part III.—(January, 1882.) Containing four numbers. Price 10d. Post-free, 1s.

The Back Numbers of Knowledge, with the exception of No. 2 (Nov. 11, 1881), and No. 3 (Nov. 18, 1881), are in print, and can be obtained from all booksellers and newsagents, or direct from the Publishers. Should any difficulty arise in obtaining the paper, an application to the Publishers is respectfully requested.

Subscribers wishing to complete their sets are advised to make early application to the Publishers, as no further reprints will be ordered.

KNOWLEDGE
AN ILLUSTRATED MAGAZINE OF SCIENCE
PLAINLY WORDED—EXACTLY DESCRIBED

LONDON: FRIDAY, FEBRUARY 3, 1882.

CONTENTS OF No. 14.

THE AIR OF STOVE-HEATED ROOMS.

By W. MATTIEU WILLIAMS.

WHATEVER opinions may be formed of the merits of the exhibits at South Kensington, one result is unquestionable—the exhibition itself has done much in directing public attention to the very important subject of economising fuel and the diminution of smoke. We sorely need some lessons. Our national progress in this direction has been simply contemptible, so far as domestic fireplaces are concerned.

To prove this we need only turn back to the essays of Benjamin Thompson, Count of Rumford, published in London just eighty years ago, and find therein nearly all that the Smoke Abatement Exhibition *ought* to teach us, both in theory and practice—lessons which all our progress since 1802, plus the best exhibits at South Kensington, we have yet to learn.

This small progress in domestic heating is the more remarkable when contrasted with the great strides we have made in the construction and working of engineering and metallurgical furnaces, the most important of which is displayed in the Siemens regenerative furnace. A climax to this contrast is afforded by a speech made by Dr. Siemens himself, in which he defends our domestic barbarisms with all the conservative inconvincibility of a born and bred Englishman, in spite of his German nationality.

The speech to which I refer is reported in the "Journal of the Society of Arts," Dec. 9, 1881, and contains some curious fallacies, probably due to its extemporaneous character; but as they have been quoted and adopted not only in political and literary journals, but also by a magazine of such high scientific standing as *Nature* (see editorial article Jan. 5, p. 219), they are likely to mislead many.

Having already, in my "History of Modern Invention, &c.," and in other places, expressed my great respect for Dr. Siemens and his benefactions to British industry, the spirit in which the following plain-spoken criticism is made will not, I hope, be misunderstood either by the readers of KNOWLEDGE or by Dr. Siemens himself.

I may further add that I am animated by a deadly hatred of our barbarous practice of wasting precious coal by burning it in iron fire-baskets half buried in holes within brick walls, and under shafts that carry 80 or 90 per cent. of its heat to the clouds; that pollute the atmosphere of our towns, and make all their architecture hideous; that render scientific and efficient ventilation of our houses impossible; that promote rheumatism, neuralgia, chilblains, pulmonary diseases, bronchitis, and all the other "ills that flesh is heir to" when roasted on one side and cold-blasted on the other: that I am so rabid on this subject, that if Dr. Siemens, Sir F. Bramwell, and all others who defend this English abomination, were giant windmills in full rotation, I would emulate the valour of my chivalric predecessor, whatever might be the personal consequences.

Dr. Siemens stated that the open fireplace "communicates absolutely no heat to the air of the room, because air, being a perfectly transparent medium, the rays of heat pass clean through it."

Here is an initial mistake. It is true that air which has been artificially deprived of *all* its aqueous vapour is thus completely permeable by heat rays, but such is far from being the case with the water it contains. This absorbs a notable amount even of bright solar rays, and a far greater proportion of the heat rays from a comparatively obscure source, such as the red-hot coals and flame of a common fire. Tyndall has proved that 8 to 10 per cent. of all the heat radiating from such a source as a common fire is absorbed in passing through only 5 ft. of air in its ordinary condition, the variation depending upon its degree of saturation with aqueous vapour.

Starting with the erroneous assumption that the rays of heat pass "clean through" the air of the room, Dr. Siemens went on to say that the open fireplace "gives heat only by heating the walls, ceiling, and furniture; and here is the great advantage of the open fire;" and, further, that "if the air in the room were hotter than the walls, condensation would take place on them, and mildew and fermentation of various kinds would be engendered; whereas, if the air were cooler than the walls, the latter must be absolutely dry."

Upon these assumptions, Dr. Siemens condemns steam pipes and stoves, hot-air pipes, and all other methods of directly heating the *air* of apartments, and thereby making it warmer than were the walls, the ceiling, and furniture when the process of warming commenced. It is quite true that stoves, stove pipes, hot-air pipes, steam pipes, &c., do this: they raise the temperature of the air directly by *convection*; *i.e.*, by warming the film of air in contact with their surfaces, which film, thus heated and expanded, rises towards the ceiling, and, on its way, warms the air around it, and then is followed by other similarly-heated ascending films. When we make a hole in the wall, and burn our coals within such cavity, this convection proceeds up the chimney in company with the smoke.

But is Dr. Siemens right in saying that the air of a room, raised by convection above its original temperature, and above that of the walls, deposits any of its moisture on these walls? I have no hesitation in saying very positively that he is clearly and demonstrably wrong; that no such condensation can possibly take place under the circumstances.

Suppose, for illustration sake, that we started with a room of which the air and walls were at the freezing point, 32° F., before artificial heating (any other temperature will do), and, to give Dr. Siemens every advantage, we

will further suppose that the air was fully saturated with aqueous vapour, i.e., at the condition at which some of its water might be condensed. Such condensation, however, cannot take place by cooling the air below 32°, and unless the walls or ceilings or furniture are capable of doing this they cannot receive any moisture due to such condensation; or, in other words, they must be below 32° in order to obtain it by cooling the film in contact with them. Of course Dr. Siemens will not assert that the stove or steam-pipe (containing the steam, of course), or the hot air or hot water pipe will *lower* the temperature of the walls by heating the air in the room.

But if the air is heated more rapidly than are the walls, &c., the *relative* temperature of these will be lower. Will condensation of moisture *then* follow, as Dr. Siemens affirms? Let us suppose that the air of the room is raised from 32° to 50° *by stove-heating purely*: reference to tables based on the researches of Regnault, shows that at 32° the quantity of vapour required to saturate the air is sufficient to support a column of 0·182 inches of mercury, while at 50° it amounts to 0·361, or nearly double. Thus the air, instead of being in a condition of giving away its moisture to the walls, has become thirsty, or in a condition to *take moisture away from them* if they are at all damp. This is the case whether the walls remain at 32° or are raised to any higher temperature short of that of the air.

Thus, the action of close stoves and of hot surfaces or pipes of any kind is exactly the opposite of that attributed to them by Dr. Siemens. They dry the air, they dry the walls, they dry the ceiling, they dry the furniture and everything else in the house.

In *our* climate, especially in the infamous jerry-built houses of suburban London, this is a great advantage. Dr. Siemens states his American experience, and denounces such heating by convection because the close stoves *there* made him uncomfortable. This was due to the fact that the winter atmosphere of the United States is very dry, even when at zero. But air, when raised from 0° to 60°, acquires about twelve times its original capacity for water. The air thus simply heated is desiccated, and it desiccates everything in contact with it, especially the human body. The lank and shrivelled aspect of the typical Yankee is, I believe, due to this. He is a desiccated Englishman, and we should all grow like him if our climate were as dry as his.* The great fires that devastate the cities of the United States appear to me to be due to this general desiccation of all building materials, rendering them readily inflammable and difficult of extinction.

When an undesiccated Englishman, or a German endowed with a wholesome John Bull rotundity, is exposed to this superdried air, he is subjected to an amount of bodily evaporation that must be perceptible and unpleasant. The disagreeable sensations experienced by Dr. Siemens in the stove-heated railway cars, &c., were probably due to this.

An English house, enveloped in a foggy atmosphere, and encased in damp surroundings, especially requires stove-heating, and the most inveterate worshippers of our national domestic fetish, the open grate, invariably prefer a stove or hot pipe heated room, when they are unconscious of the source of heat, and their prejudice hoodwinked. I have observed this continually, and have often been amused at the inconsistency thus displayed. For example, one evening I had a warm contest with a lady, who repeated the usual praises of the cheerful blaze, &c., &c. On calling afterwards, on a bitter snowy morning, I found her and her daughters sitting at work in the billiard-room, and asked them why. "Because it was so warm and comfortable." This room was heated by an 8-inch steam-pipe, running around and under the table, to prevent the undue cooling of the india-rubber cushions, and thus the room was warmed from the middle, and equally and moderately throughout. The large reception room, with blazing fire, was scorching on one side, and freezing on the other, at that time in the morning.

The permeability of ill-constructed iron stoves to poisonous carbonic oxide, which riddles through red-hot iron, is a real evil, but easily obviated by proper lining. The frizzling of particles of organic matter, of which we hear so much, is—if it really does occur—highly advantageous, seeing that it must destroy organic poison-germs. Under some conditions, the warm air of a room *does* deposit moisture on its cooler walls. This happens in churches, concert-rooms, &c., when they are but occasionally used in winter time, and mainly warmed by animal heat, by congregational emanations of breath-vapour, and perspiration—*i.e.*, with warm air supersaturated with vapour. Also, when we have a sudden change from dry, frosty weather, to warm and humid. Then our walls may be streaming with condensed water. Such cases were probably in the mind of Dr. Siemens when he spoke; but they are quite different from stove-heating, which increases the vapour capacity of the heated air, without supplying the demand it creates.

* In each of my three visits to America I lost about thirty pounds in weight, which I recovered within a few months of my return to the "home country" (of English-speaking nations).—Ed.

YOUNG ON THE SUN.

By the Editor.

In the beginning of my former notice, I remarked that, although Professor Young is one of those to whom science is very largely indebted for our present knowledge respecting the sun, few would suppose so from the treatise before us. It so chanced that, on the very day when that notice was passing through the press, a review of Prof. Young's book appeared in *Nature*, of which the opening paragraph ran as follows:—

"Since the method of artificial eclipses was introduced in 1868, Prof. Young, the author of the book under notice, has from time to time done good work in utilising the capital climate of his native country, and his relatively superior optical means, to confirm in many essential points, and to add a little shading here and there, to the bold outlines of the new science, for which we are indebted to his predecessors."

I must confess I read these lines with a sense of regret—almost of shame. It is not fitting that any student of science in this country should be ignorant of the services which our American fellow-workers have rendered in solar research, as in other departments of science. It would be something worse if, knowing what those services have been, they should seek to ignore them. But, apart from this, the reasoning involved in the slur cast on Professor Young's original work is ridiculous on the face of it. One might as reasonably say that the Herschels, utilising their larger telescopes, did good work by adding a little shading here and there to the bold outline of telescopic astronomy, for which we are indebted to their predecessors; or that Huggins, Secchi, Lockyer, and others, employing better spectroscopes, have usefully applied the general principles for which we are indebted to Fraunhofer. No one who considers what Young has done can fail to see that while even that part of his work which depended on the method of artificial eclipses is full of original

value (three of the most important observations by this method were made by him), he has done most valuable work outside this particular line of research. He was one of those who first demonstrated the gaseity of the solar corona; he first demonstrated the existence of the atmosphere of multitudinous gases existing close to the visible solar surface; we owe to him the recognition of nine-tenths of the lines of the solar sierra (*all* of those lines which were at all difficult to discover); and he invented the method (successfully applied by Respighi and Lockyer in 1871) of observing the corona with a slitless spectroscope; these, and a number of other researches, more than justify what I said last fortnight. It appears to me—I speak under correction—that a rival worker in the same field, like the editor of *Nature*, one who formerly held views opposed to those which Professor Young's original researches have established, should not have allowed the above-quoted paragraph to appear in the journal which, we must assume, he controls. Some critics might attribute the review to his pen, and point out that while science must always gain, both in progress and in tone, from emulation amongst scientific fellow-workers, the same cannot be said of the quality which is associated with "malice and all uncharitableness," and in reference to which the old proverb says that *Qui invidet minor est*. We must not suppose, however, that the review was written by the editor of *Nature*.

In this, my second notice, promised in the last number but one, I propose to touch on some of the remaining subjects admirably dealt with by Professor Young, and to show that the sole subject worth referring to is not, as the reviewer in *Nature* seems to think, that investigation of the nature of the elements, in which Mr. Lockyer is understood to have been engaged (as that reviewer, indeed, asserts) during the last thirteen years,—with important results hereafter, we may suppose, to be made more fully known. (The communications to the Royal Society in 1878 and 1879 are understood to be merely preliminary.)

In the first place, there is an admirable account of the solar spots and faculæ, with absolutely the best description we have yet seen of the various stages of the growth and development of the larger spots. The discussion of the proper motion of the spots, and of the theories which have been advanced in explanation of the seemingly more rapid rotation of the equatorial regions is admirable. Here, though the subject does not require any difficult mathematical discussion, Professor Young's familiarity with mathematical methods of reasoning stands him in good stead. We may note, in passing, that Professor Young has not allowed his book to be disfigured by that ridiculous picture illustrating (save the mark!) the sun's axial inclination, which first appeared, we believe, in Guillemin's "Heavens," and has since done duty in so many astronomical works (by writers who should assuredly know better, and doubtless do). We could have wished he had substituted another, but it is certainly better to have no picture at all than to admit one conveying quite erroneous ideas.

Passing to the portions of the sun outside the photosphere we first note an important omission. In dealing with the lower atmosphere of the sun, and the beautiful observations by which it was first recognised in 1870, Professor Young calmly leaves out all reference to the name of the eminent astronomer to whom the discovery was due—Professor Young, then of Dartmouth, now of Princetown, N.J. We find an account of the invention of the method of observing the prominences without an eclipse, in which, while due credit, to say the least, is given to Janssen and Lockyer for their success in observing the bright lines of the prominences without an eclipse, the important share which Dr. Huggins had in the work is not overlooked, as it has been by some writers. "It seems to have been reserved," he says, "for Dr. Huggins to be the first to show practically that a still simpler device would" serve to show the whole contour and detail of a protuberance at once—that simple device being the one actually in use, and the only one which has ever been successful, the widening of the slit. The account of the var ns orders of prominences is very full of interest, and, thoug.. concisely written, it is the best and fullest extant.

Professor Young's discussion of the corona is, naturally, full of interest to myself. When I first made his acquaintance in America, the views which I had advocated respecting the corona were as yet but half accepted. In 1869, a certain degree of controversial energy had characterised the contradiction which Mr. Lockyer advanced against the belief which I then asserted to be mathematically demonstrable, that—apart from any further observations—the solar corona is a truly solar phenomenon, and not due to our own atmosphere. But during the eclipse of 1870, photographic observations showed that the theory which had been rashly characterised as "ridiculous," is, as a matter of fact, true. Even then, however, a section—a very small one truly—of solar students, maintained that only the inner and brighter part of the corona belongs to the sun, the outer part being a partly optical, partly atmospheric, phenomenon. The eclipse of 1871, during which two sets of six photographs, all agreeing together, were made, enforced a still further concession of coronal matter to the sun. For my own part, I was content to wait, the clear evidence of mathematics (elementary enough, too), assuring me that scarcely any appreciable portion of the light seen outside the body of the eclipsing moon could come from other than solar matter—that is, matter by the sun. It did not seem to me worth while to argue the matter; there had been enough, and more than enough, of argument, approaching sometimes to dispute; and I had had time to see that science can never gain by contention, though controversy has not always been unfruitful. At last the eclipse of 1878 disposed of all that had been in question. And in Professor Young's book, we find a picture of the corona of 1878, combined from various drawings, showing the real solar corona extending to a distance corresponding to some three million miles from the sun. Referring to these later views, Professor Young remarks that "as has been pointed out by Mr. Proctor, the observer at the middle of an eclipse is in the centre of an enormous shadow, generally from fifty to a hundred miles in diameter. If we grant that the air retains some sensible density and power of light reflection, even at an altitude of a hundred miles, and assume for the shadow a radius of only twenty miles, no particle of air illuminated by sunlight could, under these circumstances, be found within eleven degrees of the sun's apparent place in the sky. If there were no corona truly solar in its origin, there would, therefore, be around the moon a circle of intense darkness, twenty-three degrees at least in diameter; at the edge of this circle a faint illumination would begin, forming a luminous ring, something like a halo, outside of which the sky would be lighted by rays from an only partially hidden sky." I could have wished (it is not very important, but would have served to show the value of a little reasoning applied to observations, even to tolerably old ones,) that Professor Young had mentioned that this reasoning of mine was published in my "Treatise on the Sun," and insisted upon in papers communicated to the Astronomical Society several months

before the eclipse of 1870, when observations, showing what I had already demonstrated, began to be made. My proof was not the less a proof that, if it was not *clearer* to the general, it was not so readily understood by them as the comparatively rough photographic demonstration; and the lesson needs insisting upon that very often we possess, already demonstrated, truths which a little careful reasoning will suffice to educe from observations already made. The cry for fresh observations is, in such cases, caused either by laziness or inaptitude, either by unwillingness to work out the truth from the observations in hand, or by incapacity to reason soundly and accurately.

I note that Professor Young accepts without reservation Dr. Henry Draper's important discovery of the existence of oxygen in the sun. I fully agree with him. His views respecting the elementary constitution of matter he has described in these pages. How far they agree (as the reviewer in *Nature* seems to suggest) with Mr. Lockyer's theory—or rather the theory advanced (earlier) by Professor Clarke, of Cincinnati, we leave our readers to judge.

Professor Young's account of the valuable researches made by his fellow-countryman, Professor Langley, into the question of the sun's heat and light, is full of interest. I note that Professor Young regards 10,000° Centigrade as an altogether more reasonable estimate of the sun's effective temperature than the monstrous numbers adopted by Secchi, Ericsson, and others.

I would fain dwell longer on this valuable work and on the fascinating subject of which it treats, but space will not permit. I recommend all who wish to know the present position of solar research to take this treatise for their guide—a guide thoroughly honest and trustworthy. The book is not a book to be tasted, or merely swallowed, but "to be chewed and digested." It has one fault (I think a somewhat serious one): Professor Young is exceedingly unwilling to claim his own work; as some of it has been rather unceremoniously claimed by others, he, in this, hardly does justice to those who in this country have urged his claims (of the justice of which, be it remarked, he is fully conscious, little though he cares to assert them). There is such a thing as carrying modesty too far. He also fails sometimes in expressing with sufficient confidence views which he is known to regard as unquestionably just. These, however, are, after all, very amiable faults. Perhaps I should not have noticed them if he had left me any others to note.

FOUND LINKS.

By Dr. Andrew Wilson, F.R.S.E.

PART III.

Having in my previous paper tried to show that the Mud fishes were veritable links between the fish-class and the frog-class, we may now turn to the history of the latter group itself, by way of showing how, within its own limits, gaps and gulfs have been bridged in Nature's own way. The history of a frog is in itself an interesting study. It begins life as a tadpole, and lives, as most readers know, a perfectly fish-like existence. It is fish-like in form: its heart is two-chambered, and thus resembles that of the fish, and it breathes at first by outside gills. By-and-bye a broad fold grows over the gills, and ultimately covers them; whilst internal gills grow from the gill-arches. Meanwhile, the tadpole has been cropping the waterweeds by means of the horny jaws with which it is provided, and has been digesting its food within the long and spiral intestine which is the right and heritage of the vegetable feeder. Soon, however, the hind legs, which in the frogs and toads are the first to appear, are developed; and these are in turn succeeded by the front limbs. Lungs begin also to grow, as all lungs do, namely, in the form of two sacs or bags from the hinder or lower wall of the gullet. At this stage, the likeness of the frog to the fish has disappeared, and it closely resembles one of the common tailed "efts" or newts, which are familiar denizens of our ponds and pools. If it had retained its outside gills after its legs had become developed, the young frog would have exactly resembled that curious creature, the *Proteus*—found only in underground caves in Central Europe—or the curious Axolotl of North America. But the ways of frog-development do not permit it to remain in the guise and likeness of its eft-cousins. Whilst its lungs have been undergoing development, the heart has been approaching that of the frog-type, which possesses a three-chambered heart, as already observed. Then, as development is completed, the tail shrivels. Growing "small by degrees," it is represented in the adult frog by a mere rudiment; and, as the obliteration of the tail takes place, the young frog leaves the water and assumes the habits of a land-existence; breathing by lungs alone in its adult state, and exchanging, moreover, the vegetarian tastes of its infancy, for an insect dietary in after-life.

Now, the history of a frog is beset with questions of interest for the earnest mind that studies even its superficial features. Why, firstly, should a frog pass through these changes at all? is a very pertinent inquiry; and if this be capable of being answered, why, secondly, should its development run in the lines sketched out? If we start with the idea that animals and plants were simply "created" as we find them—and that view of matters is, of course, not yet displaced in unscientific circles—then, so far as I can see, no explanation whatever of the frog's development can be offered. "It is so, because it is so"—such is the logical dead wall that awaits the student who turns to the "special creation" theory for an explanation. There is no accounting for a supernatural creative fiat; we cannot give reasons for a "special creation:" in a word, we must, on this theory of nature, simply accept the fact of the frog's existence, and have done with it. But there exists the alternative idea of *evolution* and *descent*. What if it be admitted that one species or group of animals arises by natural variation and descent from another group? What if in the frog's development we are led to see a panorama—a moving picture, of the descent of its race? The reasonableness of evolution may thus, I think, become very apparent: contrariwise, I know of no other rational explanation of the frog's tadpole-stage, and its subsequent development.

What evolution, then, says is this: the frog is at first a fish-like, gill-breathing tadpole, with a fish-heart, because its earliest ancestor was a fish; and it is interesting to note that the young of some well-known fishes (*e.g.*, dog-fishes) breathe by outside gills. I have a beautiful specimen of two of these young fishes with their outside gills in my museum. Furthermore, the resemblances of the tadpole to the type of some primitive fish do not end with its outside aspect. Mr. F. M. Balfour says the anatomy of the tadpole points to its relations with the living lampreys, which, as every naturalist admits, must be fishes of a very ancient type. But, secondly, the tailed tadpole becomes four-legged, and it thus resembles, as we have seen, a newt or eft. The reason of newt-stage is evident if we assume that the frog-stage was attained through a newt-stage. Abbreviate the tail of the newt, elongate its hind legs, and with a few other modifications, we find

the higher frog to be represented. For the frog, let it be remembered, is the highest type of its class; and the evolutionist's contention is that it has ascended to that place and dignity by successively rising from fish to newt, and from newt to frog. The reasons for the "metamorphosis" of the frog are clear enough, on the principle that *development repeats descent*—not always clearly, it is true, and with much modification, but still plainly enough to reveal the ways of the "becoming" of the animal world.

If it is asked, Why do not all animals show their descent as clearly as does the frog? I reply, because their development has been modified. But it is none the less true that in the development of all animals we see glimpses of the lines of their genealogy. The great difference between a frog's development (or that of an insect or crustacean which also undergoes "metamorphosis") and that of, say, a fish which hatches directly from the egg, consists simply in the fact that the frog's development is mostly passed outside the egg, whilst the fish developes within the egg.

But it is interesting to note that the frog in itself thus serves to link together groups of its own class. Thus its own development—not to speak of that of the newts themselves—teaches us that the newts have arisen from the fish-stock, and that they represent a lower phase of amphibian life than do the frogs and toads with their shortened tails. Indeed, the study of the frog itself not merely proves to us its own evolution, but demonstrates an orderly sequence in the descent of its class—a sequence wherein the newt-type followed the fish, and wherein the frog-type, in turn, was evolved from the newt.

That some such explanation—or, at least, an explanation based on similar grounds—is the only feasible method of explaining the metamorphosis of a frog, may be stoutly maintained against all comers. Evolutionists may differ regarding the exact lines along which the descent proceeded. They do not differ regarding the main facts at issue, namely, that fishes are linked to frogs in more ways than one, and that the history of the frog-race, rightly viewed, is really a connecting-thread on which the various forms of living and extinct members of its class may be strung. In my next paper, I shall endeavour to trace the "links" which bind birds to reptiles.

INTELLIGENCE IN ANIMALS.

KEPLER'S (the mastiff's) claim to be looked upon as a reasoning dog may be regarded by some as being better based, perhaps, on what his master and mistress described as actual mathematical calculations. "Kepler," says the latter, "like his great namesake, is an excellent mathematician. Many distinguished men have been delighted with his performances in this direction. The mode of procedure is this: His master tells him to sit down, and shows him a piece of cake. He is then questioned, and barks his answers. Say he is asked what is the square root of 16 or of 9; he will bark three or four times, as the case may be. Or such a sum as '6 + 12 – 3 divided by 5,' he will always answer correctly: more prolonged calculations rather fatigue him. The piece of cake is, of course, the meed of such cleverness. It must not be supposed that in these performances any sign is consciously made by his questioner. None whatever. We explain the performance by supposing that he reads in his master's expression when he has barked rightly: certainly he never takes his eyes from his master's face." A singular performance, and one showing that some dogs possess not only keen vision, but keener powers of perception than most men. It would, however, be a mistake to regard Kepler's performance as illustrating the possession of actual reasoning power by animals. For certainly the calculations he seemed to conduct were conducted in reality by his master.

This intelligent animal showed excellent judgment when a large photograph of one of Landseer's dogs (that is, a photograph of a dog pictured by Landseer) was shown him. He showed his perception of the painter's skill by at once distinctly recognising that the photograph represented a strange dog, of whom, by-the-way, he manifested decided jealousy. Kepler knew the meaning of many words. He recognised clearly when his master was ill, and showed at such time real concern and sympathy. "He was exceedingly kind and unselfish to a little English terrier, called Tycho 'Brahe'"* (I quote again from Mrs. Huggins' interesting little sketch; only, as Kepler is dead, I change the tense in these few last sentences from the present to the past), "who often tried him, and to a very unamiable cat, who both formed part of the household in which he dwelt. Altogether, there was in Kepler's every look, and motion, and utterance, a noble and intelligent individuality which endeared him to all who knew him. Much might be learnt from him in many ways; and he was indeed worthy of a large share of an inclusive love—that love which loveth

'All things both great and small.'"

The question whether animals can count in any way, or discriminate, at any rate, between different numbers, is one about which different opinions have been expressed. We cannot consider that the question was answered (affirmatively) by Kepler's achievements, though he seemed to do more than count. On the other hand, the common opinion that a bird, whose nest has been robbed of all the eggs but one, is as well content with that one as with the entire set, is not supported by evidence, and, indeed, seems to have been devised to comfort the consciences of those who like to go birds'-nesting, but might be troubled with regret for the troubles of the parent birds, were it not for this ingenious theory. We all remember the remonstrance of Tom Brown, when East proposed to take all four of the eggs in the nest robbed by Martin, "No, no! leave one, and then she won't care," said Tom. "We boys," says the author, "had an idea that birds couldn't count, and were quite content as long as you left one egg. I hope it is so." However this may be with birds (and, on the whole, I incline to think even penguins, "boobies" though sailors call them, have some idea of the number of their eggs), the following story seems to show that dogs can count their young. "To my friend, Dr. Velasquez Level, a respectable physician of this city," writes M. A. Ernst, of Caracas; "and for several years a resident of the island of Margarita, I am indebted for the following touching instance of the sagacity of a bitch. Her owner, for some reason or other, had destroyed all the female puppies in two successive litters. On her having brought forth a third one, it was found that there were but three male puppies. The bitch, however, was observed to leave her whelps occasionally, and to return some time after. Being followed, she was discovered suckling three female puppies, which she had hidden under some brushwood, undoubtedly with the intention of saving them from the master's cruel hands." This, perhaps, is the most striking of all the cases we have yet considered. It would seem that when the female puppies of the first litter were destroyed, the mother either did not recognise the circumstance that all the male puppies were left, or else

* Tycho Brahe (the dog, not the astronomer) was commonly called by Dr. Huggins, Tyko Barky.

regarded it as merely accidental, for otherwise she would probably have tried with the second litter the plan she actually tried with the third. When the female puppies of the second litter were taken, she recognised the rule by which selection had been made. Thus she had up to this point reasoned well and with due caution, not adopting a conclusion until the evidence in its favour had become very strong and convincing. She had also shown a power of counting; for obvious though the result she obtained may seem to one of ourselves, capable of dealing readily with much larger numbers, yet if we conceive a mind so far inferior in matters of calculation to that of a savage (and such savages are known) who can scarce count up to five, and has to run through a process of calculation before he can say how many children he has, as the mind of such a savage is to that of a skilful mathematician, we see that to such a mind the process gone through by the animal in this case would be what a very profound calculation would be to the mathematician. In other words, we here have evidence that the difference between the mind of an animal and the mind of man is but one of degree, and that the animal is not more widely separated from man in this respect, than the lowest among men is from the highest.

NIGHTS WITH A THREE-INCH TELESCOPE.

By "A Fellow of the Royal Astronomical Society."

BY this time the student will have become tolerably familiar with his instrument. We propose to employ it to-night in the examination of some of the more striking objects in the glorious constellation of Orion (Map, pp. 204 and 205). And first we will turn it upon β Orionis

FIG. 9

RIGEL

or Rigel, fig. 9, which will furnish the young astronomer with good, if easy, preliminary practice in the detection of small stars in the neighbourhood of larger and more brilliant ones. Probably, at first, his eye will be dazzled with the brilliant blue coruscation surrounding Rigel itself; but a little careful attention will show just above and to the

FIG. 10.

λ ORIONIS

left of it a small bluish point, as shown in the figure. From Orion's foot he may proceed to his face, in which we shall find λ, a very pretty pair, tolerably close together, the larger star being yellowish, the smaller one more of a lilac hue. Our figure represents it as seen with a power of 120. The lowest, or most easterly of the three stars in the Giant's belt ζ, will next claim our attention, and to show this properly will be a pretty severe test of the excellence of the observer's instrument. As shown in our drawing, this

FIG. 11.

ζ ORIONIS

star is triple; the principal and second stars, with a power of 150°, being almost in contact, and the third below and to the right of them. Some considerable gazing will be required on the part of the beginner before he succeeds in making out the duplicity of the principal pair in this asterism. Our engraving may help him to understand exactly what to look for.

We now turn to σ, which will be seen beneath ζ in the Map. This is a triple, or, perhaps more correctly, a septuple star, all the components shown in our next figure being well within the same field with a power of 120.

FIG. 12.

σ ORIONIS

The object marked θ in the Map is one of the most wonderful in the whole heavens, consisting, as it does, of a mass of nebulous matter (now known to be intensely heated gas!)* surrounding, and seemingly physically connected with, a curious group of stars.

FIG. 13.

θ (AND 42 M NEBULA) ORIONIS

No woodcut can possibly do justice to this most marvellous object; but in our sketch, copied above, we have endeavoured to give some faint idea of its aspect as viewed

* Gas: but is it intensely hot?—Ed.

with a power of 80. The black gap leading up to the trapezium of four stars is known as "the fish's mouth." The nebulosity surrounding an isolated star, towards the bottom of the field, will be noted. The difference in colour of the stars forming the trapezium will be readily detected. There are a fifth and a sixth belonging to this group; but they are entirely beyond the power of such an instrument as that which we are using.

Having gazed our fill on this wonderful sight, and, furthermore, particularly scrutinised the trapezium of stars with the highest power at our disposal, we will lower the telescope a little to ι Orionis, a very pretty triple, in a fine field.

FIG. 14.

ι ORIONIS

Its aspect, as seen with a power of 120, is shown in fig. 14. The smallest of the three stars will require careful looking for before the unpractised observer will see it at all.

An even more difficult star is ρ^1 Orionis, represented in fig. 15. This will require a power of 150 at least, and, in

FIG. 15.

ρ ORIONIS

fact, as high an one as the observer possesses, to see the companion fairly. The small star is so faint and difficult with a three-inch aperture as to form a very fair light-test indeed. ρ^1 may be found by carrying an imaginary line through the three stars ζ, ε, and δ, in the belt, on which line, at double the length of the belt from δ, it will be found.

The last illustration we shall give is of 52 Orionis, a severe test of the separating power of such an instrument as we are considering. At moments of the finest vision, with the highest power at the observer's disposal, it will be seen as in fig. 16.

FIG 16

52 ORIONIS

Such are a few typical stars among a very mine of such objects in which the student may well search by sweeping for himself. Should he succeed in exhausting such a treasury in one night's work, he may turn his telescope down to Lepus, where, *inter alia*, he will find a pretty, and somewhat difficult pair in κ. This is the star to the right of λ, and just beneath ι, in the map on pp. 204 and 205.

BRAIN TROUBLES.

Partial Loss of Speech.

It will generally be noticed that some words are more troublesome than others when the mind is in this special state. Each person, probably, has his own peculiarities in this respect, and can (or might) recognise, from the misplacement or misspelling of particular words, the necessity for rest or change of occupation. It may be noticed, even in more remarkable cases of defective power of articulation, that some words suffer more than others. In the following case the patient had had an apoplectic fit, from which he recovered so far that his intellect and bodily strength were restored, but he could not speak intelligibly. He laboured under no paralytic affection, and could articulate freely, only, unfortunately, the syllables which he uttered with great apparent ease were meaningless. "When he came to Dublin, his extraordinary jargon led to his being treated as a foreigner in the hotel where he stopped, and when he went to the college to see a friend, he was unable to express his wish to the gate-porter, and succeeded only by pointing to the apartments which his friend had occupied." He perfectly understood every word addressed to him. He could read and understand written words and printed matter. "Having procured a copy of Andral's 'Pathology' in French, he read it with great diligence, having lately intended to embrace the medical profession." He expressed his ideas in writing with considerable fluency; and when he failed, it appeared to arise merely from confusion, and not from inability, the words being orthographically correct, but sometimes not in their proper places. We pass over other details indicating that he retained full possession of his mental faculties. The peculiar imperfection of language which he exhibited was thus tested by Dr. Osborn: He selected and laid before the patient the following sentence from the bye-laws of the College of Physicians:—

"*It shall be in the power of the College to examine or not examine any Licentiate previous to his admission to a Fellowship, as they shall think fit.*"

This being set him to read, he gave (at intervals of a few days), the two following versions:—

(i.) "*An the be what in the temother of the trotholodoo to majorum or that emidrate ein einkrastrai mestreit to ketra totombreidei to ra fromtreido as that kekritest.*"

(ii.) "*Be mather be in the kondreit of the compestret to samtreis amtreit emtreido and temtreido mestreiterso to his eftreido tum bried rederiso of deiddaf drit des trest.*"

Here it will be noticed that the words "*be in the*" were correctly given on both occasions, except that on the first the word "*what*" is interposed between "*be*" and "*in*" (no doubt, merely as a question,) (What?). Again, the words "*of the*" were given correctly on both occasions. The word "to" was given correctly three times out of four, and on the fourth was probably nearly right, though lost in the written transcript in the word "*tum*" query *toom?*). Strangely enough, the words "*to his*," correctly given on the second occasion, were represented by the singular combination "*to ketra*" on the first. The word "*examine*," which occurs four times altogether, is represented by the following dissimilar combinations of syllables, (i) "*majorum*," (ii) "*emidrate*," (iii) "*samtreis*," and (iv) "*emtreido*," which only resemble each other in this, that all contain an "*m*" in the first syllable, and an "*r*" in the second. It is noteworthy again, and seems to prove the utter absence of method in this patient's defective articulation, that, whereas on the second occasion the three dissimilar words "*examine*," "*licentiate*," and "*admis-*

[illegible] represented by the similar sound, "*[illegible]*," [illegible] *[illegible]*, the [illegible] were represented on the [illegible] the utterly dissimilar sounds "*[illegible]*," [illegible] "*[illegible]*." This patient knew when he spoke wrongly.

Reviews.

SCIENCE LADDERS.*

THESE little books are capital. They deal in the plainest possible words with their respective subjects, out what they describe they describe neatly and exactly. They are suited for the youngest readers, yet they contain a number of facts which many who are not very young will find full of interest. Even those who know most of the facts contained in these books will enjoy reading them simply because of the pleasant way in which the facts are presented. Take, for instance, the following account of the way in which hydras are born into the world. "For a long time no one knew. "At last, however, a patient naturalist, who had long watched a hydra in a glass case, saw the growth of a tiny egg on its body, below the mouth and arms. Three days after the discovery of the egg, it was loosened from the body of the mother, and fell to the bottom of the water. When it first appeared it was cream colour; it had now turned a bright orange. It remained at the bottom of the water for fifty-five days, and the only changes which took place in it during that time were that the outside skin became rough and the shape changed from round to oval. At the end of fifty-five days the egg cracked, and a baby hydra pushed out part of a soft, transparent, crystal-like body, quite round and smooth. Two hours afterwards this baby began to put out threads, and in seven days its shape was just like that of the mother hydra, only much smaller. This baby hydra took no food till it was more than a month old. It is now known that hydras do not always come from eggs, but grow out like branches from the bodies of grown-up creatures. From these branches spring yet younger branches, so that sometimes quite a family tree is made."

SCIENCE FOR ALL.†

THE object of this work so closely resembles that which we have in view in KNOWLEDGE, that it would be strange (considering who have been Dr. Brown's coadjutors) if the work itself did not meet with our approval. In the present volume, which is the fifth of the series, we find most interesting articles by Dr. Wilson, on Zoological subjects; by Prof. Duncan, on Earthquakes and Animals, old and new; by Dr. Mann, on Lighting, &c.; and by other writers on subjects with which they are respectively more or less conversant. The editor, Dr. Brown, discusses ably the question how plants were distributed over the earth, and in another article considers the question, What is a fruit?

Nearly all the articles are well written and well illustrated—many, quite admirably. The volume, as a whole, is a charming contribution to popular scientific literature, well printed on excellent paper, and handsomely bound.

* Science Ladders. Series I. No. 1. Forms of Land and Water. Series II. No. 1. Vegetable Life. Series III. No. 1. Lowest Forms of Water Animals. By N. D'Anvers. Price 6d. each. (London: Sampson Low, Marston, & Co.)

† *Science for All.* Edited by Robert Brown, M.A. (Cassell, Petter, & Galpin, London, Paris and New York.) Price 5s.

Here and there are some shortcomings in style of treatment, and occasionally, but more seldom, in scientific accuracy. We may cite, as instances of the latter kind, Mr. Denning's statement that the Saturnian rings, if solid, may be maintained in equilibrium in the way suggested by Laplace. Nothing can be more certain than that the rings could not possibly be maintained in equilibrium as Laplace supposed. It is also not true that most careful measurements show the rings to be slightly eccentric. Some measurements have done so on particular occasions, that is all. It is clear, from Mr. Denning's account of the appearance of the rings, as supposed to be seen from the planet, that he has not given the matter even that cursory examination which Dr. Lardner gave it, otherwise he would assuredly have seen that in the vicinity of the poles no part at all of the rings could possibly be seen. Nor is it true that at night the rings would be seen as a vivid semicircle of light; during the winter half of each Saturnian year they would not be visible.

Apart from minor defects such as these, the volume before us is one which, whether regarded as part of the "Science for All" series, or judged by its intrinsic merits, can be warmly recommended. It is full of interesting matter, plainly worded, and, for the most part, exactly described.

THE SCIENCE OF THE STARS.*

WE admire Mr. Pearce's calm audacity. He sends to us for review a work on Astrology with as much confidence as if it were such a work as "The Courses of the Stars," by our esteemed correspondent Mr. Bazley, a work bearing a title which might cause it to be confounded with astrological treatises (instead of being one of the most valuable contributions made during the last few years to the science of the stars), while Mr. Pearce's book, instead of justifying its title "The Science of the Stars," advances gravely all the absurdest views of the astrological charlatan.

Perhaps the most remarkable feature of this work is the mixture of literary lore and utter ignorance of science. We have references to the sayings of a number of more than respectable writers, in company with statements based on the authority of such an ignoramus (to speak charitably) as the late Lieutenant Morrison, the Zadkiel of the notorious almanac bearing that name. One specimen of the manner of reasoning adopted by modern astrologers (the ancients had some reason for their errors) may be quoted—it will suffice, we should imagine:—

"It may appear arbitrary to take the moon as general significatrix (in mundane astrology) of the common people. Yet it would seem to have some show of reason when we remember that the Tay-bridge catastrophe, by which nearly one hundred lives were lost, only one or two of the passengers ranking above the class of 'common people,' took place on the very evening (Dec. 28, 1879) of the partial eclipse of the moon in the sign Cancer (which rules Scotland)—aye, and before the shadow had entirely passed away from the moon's disc. Ramsey avers that such an eclipse falling in Cancer denotes 'the death and slaughter of obscure, common, plebeian kind of people.' On July 12, 1870, a total eclipse of the moon, visible in Europe, took place. Three days afterwards Louis Napoleon declared war against Prussia. The slaughter in that war was horrible."

THE EFFECTS OF TOBACCO.

By Dr. Muir Howie.

PART I.

THE use of tobacco is becoming so extensive, that it is incumbent upon all who are interested in the health of the community, to devote some attention to the effect of such increased consumption. How does tobacco affect the human organism? Does it increase or diminish its capacity for physical or mental work? Does it tend to prolong life, or to bring on premature decay? Does it make life, as a whole, more pleasant and agreeable, or are its comforting and soothing effects rendered nugatory by subsequent irritability? Many such questions naturally arise in the mind of

* "The Science of the Stars," by Alfred J. Pearce. (Simpkin, Marshall, & Co., London.)

the inquirer; but in the present paper I intend to narrow my consideration of the subject to one special point, viz., the food action of tobacco; and, in order to show you that I am not fighting against a man of straw, permit me to quote the words of one of our most eminent writers on the subject of narcotics—the late Dr. Anstie, who, in "Stimulants and Narcotics," says:—

"Next, perhaps, to coca in its power of replacing ordinary food, we must reckon tobacco. The power of this substance to compensate, to a certain extent, for the want of food, is very well known, but, strangely enough, it is generally assumed that this property of tobacco is dependent upon its power to disgust the appetite, by prostrating the nervous power of the stomach. A very little reflection should be sufficient to entirely discountenance such a view. There are very many substances capable of destroying appetite by a depressing influence upon the nervous system; such, for instance, as the salts of antimony, or the preparations of ipecacuanha; yet no one will pretend that the action of any such drugs would relieve the sense of faintness produced by fatigue, endured in the absence of food—an effect which tobacco undoubtedly produces in persons with whose system it agrees. The experienced sportsman, accustomed to tramp long hours over the heather in quest of game, would laugh at such an explanation of the effect of his favourite "cutty." He knows very well that it is by no mere disgusting of his appetite that he comforts himself for the indefinite removal of the prospects of dinner. By the time he had succeeded in depressing his stomach to the level of indifference to food, he may be sure he would have rendered himself incapable of continued strenuous exertion were tobacco effective only in this way. That tobacco is not an exact equivalent for roast beef, nobody knows better than the smoker; at the same time, it would be impossible to persuade anyone who had practical experience of the use of it to believe that its only effect is to depress nervous power. The fact is, that all such statements are made on the authority of persons either practically ignorant of the effects of smoking, or else naturally incapable, as some are, of deriving benefit from it. There are a few people whom no amount of care and skill exercised in the taking of tobacco, nor any moderation in the dose used, can save from unmistakable poisoning, whenever they indulge in it. These cases are rare, and they ought to be carefully separated from the evil results which are produced by mere unskilfulness in smoking, such as causes the trouble of beginners in the art."

Now in order to enter intelligently into the discussion of such an utterance as the above, we must pursue a line of argument like the following:—

1. What constitutes any substance a food? Does tobacco possess those attributes? If not,

2. Are there no substances of advantage in nutrition other than those properly termed foods? If not,

3. To what class of agents does tobacco belong, and what is its exact influence upon nutrition?

1. What constitutes any substance a food? We answer, whatever can be used either to build up the body or add energy to it is a true food. The human body, like the steam-engine, requires two classes of materials for its efficiency. It requires nitrogenous material, by which the machinery is built up, just as the steam-engine is made of iron, brass, &c.; and it requires carbonaceous and other material, whose combustion gives motive power, just as the steam-engine requires coal and water for purposes of motion. Every nerve and muscle in the body is a vast assemblage of cells, and each cell is filled with explosive material, ready to burst on the application of the slightest stimulus, and thereby to liberate its pent-up energy in order to conduct the vital functions for which it is adapted. The vital processes are thus conducted by a continued series of explosions, and so great is the heat generated by such explosions, that unless the human body were mostly composed of water, it would go off in smoke like a bomb-shell, or quickly disappear by spontaneous combustion. When one end of a nerve is irritated, a series of explosions runs along its entire length. If this nerve leads to the brain, it excites thought; if to a muscle, it excites movement by originating a series of explosions in the brain or in the muscle. Those of you who, as boys, have amused yourselves by setting fire to an end of a long train of gunpowder, and watched with delight the glowing force gliding, hissingly, along its course, will at once appreciate this explanation of nervous communication. When once the cell contents have liberated their energy by explosion, they are henceforth as useless as the washings of a gun or the spent ashes on the hearth, and must be swept out of the body as waste matter, to make room for a fresh supply of stored-up energy. It is through the medium of the ever-circulating blood that a continuous supply of such material is brought within reach of every cell in the organism; and it is by means of that same current that the waste matter is carried away which would otherwise as effectually extinguish life as an accumulation of ashes will extinguish the kitchen-fire. This waste matter we call poison, because of its power to interfere with vitality. I may mention, in passing, that it is among this waste matter that alcohol is found in the body of every man, be he the most ardent teetotaller or the most ardent spirit-drinker in these realms. Alcohol is the ashes which remain after the explosion of sugar in the body, and, like all other ashes, it is rapidly thrown out of the system.

We are sometimes told that alcohol must be a food, because it is found in the body. We might as well be told that spent ashes are good fuel because they are found in the fire.

Now, it is not maintained by any scientific authority that tobacco either assists in building up the tissues or in supplying them with explosive material; but such authorities do assert that it is useful in some other way. This brings us to our second consideration, viz.

2. Are there no substances of advantage in nutrition, other than those properly termed foods? We answer that there are. Just as the engine-driver cannot attain express speed without a liberal use of the poker, so the human machine cannot be kept in healthy activity without the administration of *stimuli*. Observe, I do not say *stimulants*, because that word has been corrupted, and now refers to a class of compounds which ought properly to be termed alcoholic narcotics. Alcohol is almost entirely used for its narcotic properties, and where thus used, cannot be admitted under the head of a stimulus. A stimulus is an agent which makes life more active, although it adds no energy whatever to the system; just as the poker will make the fire burn brighter, although it adds no heat or brightness of its own. Stimuli may be applied externally, or administered internally, and the more stimulus the body encounters among its surroundings, the less does it require mixed with its food. The man who takes a cold bath every morning before going to business does not require strong coffee to goad his nervous system to its daily toil. Those who have abundant open-air exercise may live entirely on vegetable diet, which contains but little stimulus; while those whose life is monotonous and sedentary require a more stimulating diet. But the healthiest stimulus is unquestionably the external. Open-air exercise, cold bathing, and pleasurable mental excitement will give sounder and better stimulation than the most savoury diet. Internal stimuli must only be resorted to when the external cannot be secured. There is one criterion by which you can always distinguish whether or not any agent is a stimulus, viz., by its power to increase the demand for food. The more you employ your poker, the more coal you burn; and just as you can extinguish your fire by a too vigorous application of the poker, and without adding fresh supplies of coal, so you may extinguish life by using too much stimulus without giving, at the same time, an increased supply of food. For example, if you feed a dog entirely on Liebig's extract of meat, which contains the stimulating properties of beef without much of the nutrient property, it will not live so long as if you fed it upon water alone. This proves that the extract of meat is a true stimulus, because it induces a greater necessity for food. It is thus useful for invalids with failing appetite, provided that true food be given at the same time. Now, tobacco is not a true food, neither is it a stimulus; for it rather diminishes the desire for food. Indeed, the boast of its advocates is, that it enables a man to do with less food, and even to do without food altogether for considerable periods.

3. How, then, does tobacco affect the animal tissues? It is not equivalent to the coal of the fire, nor to the poker. Where, then, can we find an analogous agent? Tobacco has the same effect upon the nerve-cells that water has upon a coal fire. Apply water in small quantity, and your fire will burn more slowly; apply a large-enough bucketful, and it will cease to exist. When the cook rakes up the ashes, and covers her fire before going to bed, she performs the same physical experiment as her master, who soothes his nerves with tobacco before retiring for the night. The cook wishes her fire to smoulder during the night. She therefore applies an agent which will check combustion by partially excluding oxygen from her fuel; her master applies to his nervous system an agent which diminishes oxidation, and thus seriously interferes with vital action. In both cases there will be less material burned, less coal and less explosive food. But is this a real advantage to the usefulness of the fire or of the human machine? The cook would be very late with breakfast if she trusted such a fire to boil the kettle, and the work accomplished by a brain much affected by tobacco is both small in quantity and inferior in quality. It is as difficult to send proper messages along a nerve which is under the influence of tobacco as it is to fire a train of damp gunpowder. "Praise God, and keep your powder dry," said the great Oliver Cromwell; "Praise God and keep your brain clear," would have been his burning advice had he lived in these latter days.

PROFESSOR GRANT ON METEOROLOGY

AT the lecture given at Glasgow by Mr. John Burns to the representatives of several of the great railway companies of the United Kingdom, Dr. Grant, Professor of Astronomy in the University of Glasgow, gave an interesting summary of the present state of the science of meteorology. In the course of his remarks, he said:— A person wrote to Sir William Herschel requesting to be informed respecting the state of the weather during the next few months which would elapse after the date of his letter. The distinguished astronomer replied in terms to this effect:—"The question of predicting the weather is one which is above the comprehension of astronomers and men of science in general." This letter was written about 100 years ago, and it expressed very concisely the immense difficulty of the problem. In those days the observations of the weather were very imperfect and limited in range. But a more hopeful view presents itself in the present day. During the last thirty or forty years, systematic observations of meteorological phenomena have been carefully made in all the countries of the civilised world. In many instances those observations have been discussed by men of science, and conclusions have been deduced from them which have thrown much interesting light on the climate of the countries to which they refer. Furthermore, the invention of the electric telegraph has supplied the means of rapidly confronting distant observations with each other, and of disseminating with equal rapidity the conclusions deduced from this inter-comparison. The result, then, is that in the present day the science of meteorology includes many valuable conclusions arrived at by careful induction from observation, and that even in the matter of predicting the weather, some progress has been made. It is, however, to be borne in mind that the attainment of this last-mentioned object is due rather to a sagacious interpretation of the observations, combined with the marvellous aid of the electric telegraph, than to a rigorous deduction from established scientific principles. In systematic observations of meteorological phenomena, the Royal Observatory, Greenwich, led the way in this country. Subsequently the observatories of Oxford, Liverpool, and Glasgow devoted attention to the same object. The establishment of meteorological societies in England and Scotland about the same time contributed also to the advancement of meteorology as a science. The Meteorological Office, originally a branch of the Board of Trade, commenced its labours in 1868, the council of scientific men under whose direction it is conducted being nominated by the Royal Society. Now, there are three leading objects which the Council have undeviatingly kept in view since the commencement of the existing organisation in January, 1868. These are:—1. Ocean meteorology. 2. Land meteorology of the British Isles. 3. Weather telegraphy. Allow me to make one or two remarks on each of those objects. I need not dwell upon the vast importance of ocean meteorology. It is upon the information furnished by this branch of the science that our ships, whether of the Royal or the Mercantile Marine, must rely for shaping their courses most advantageously over the trackless ocean. A fine field of scientific work is here opened up to ship captains who have a taste for the observation of meteorological phenomena. Any of such officers who desires to co-operate in taking observations is furnished with instruments for the purpose, and it is gratifying to learn from the annual reports of the Meteorological Council that the labours of many of them in this respect are much appreciated by the Council, as constituting valuable materials for subsequent discussion. Seven observatories have been established in connection with the Meteorological Office with a view to the advancement of the land meteorology of the British Isles. These are the observatories of Valentia and Armagh, in Ireland; Falmouth, Kew, and Stonyhurst, in England; and, finally, Glasgow and Aberdeen, in Scotland. The observations at each of these observatories are all obtained by means of self-recording instruments, and the tabulated results are regularly transmitted once a week to the Meteorological Office in London. The variations of the barometer and of the dry and wet bulb thermometers are recorded continuously upon paper by a photographic process which goes on night and day without intermission. The velocity of the wind is measured by its action upon a system of revolving hemispherical cups, an instrument invented by Dr. Robinson, the director of the Armagh Observatory. I was lately induced to make some calculations based upon the recorded anemometer observations at the seven observatories, with the view of ascertaining the mean hourly velocity of the wind at each observatory during the years 1874-5-6. The results of my calculations were these:—The mean hourly velocity of the wind for the three years in question was—for Armagh, 10·6 miles, 10·0 miles, and 9·8 miles; for Kew, 10·3 miles, 10·8 miles, and 10·8 miles; for Stonyhurst, 10·8 miles, 10·9 miles, and 10·7 miles; for Glasgow, 12·9 miles, 12·1 miles, and 12·4 miles; for Aberdeen, 13·3 miles, 13·5 miles, and 14·2 miles; for Falmouth, 16·8 miles, 17·0 miles, and 17·1 miles; finally, for Valentia, 18·2 miles, 17·7 miles, and 17·9 miles. It will be seen from this how nearly the annual mean results obtained at the same Observatory agree with each other. It will be seen, further, that, while Armagh, Kew, and Stonyhurst have the least wind, the lion's share of the wind falls to Valentia and Falmouth; while, again, the Scottish observatories hold an intermediate position in this respect. These results, it must be admitted, speak well for the observations on which they are based, and for the instruments with which the observations are made. They also afford us an interesting illustration of the presence of law as the regulator and controller of all the phenomena of nature. "Variable and fleeting as the wind" is an expression often used, and yet, when the winds at any place, which blow from all points of the compass, are gathered together, their aggregate velocity from year to year is found to be almost identical in amount. It cannot be doubted that important conclusions tending to throw light on the climate of the British Isles will result from a discussion of the observations received at the Meteorological Office from the outlying observatories. I have, finally, to make a brief reference to weather telegraphy. In considering this matter, the important fact must not be lost sight of, that the forecasts of the weather which emanate daily from the Meteorological Office are not given forth as rigorously deduced scientific conclusions by the eminent men of science who constitute the Meteorological Council. Had they been less fettered in the matter, they would probably have been more cautious, but they have wisely yielded to the public demand for such forecasts, and it must be admitted that in this instance the public instinct was in the right direction. The success of the forecasts, considering the difficulties which meteorologists in the British Isles engaged in such an inquiry have to contend with, has been very decided. During the last two or three years, as many as 75 per cent. of the storm warnings which have emanated from the Meteorological Office have been thoroughly successful. We may therefore confidently indulge the expectation that, with the progress of further researches, the percentage of successes will continue to increase. The Americans have shown great enterprise and skill in this matter. But it is to be borne in mind that they have an advantage in the inquiry which we cannot, from the nature of things, possess. For storms that come from the west—and these are the storms which really strike our shores—the Americans have a whole continent at their backs upon which to plant signals for the purpose of informing them respecting a coming storm; while we, on the other hand, have only the Atlantic, where no signals can be established. The present winter will hereafter be memorable for its storms. I may state that, in addition to Robinson's anemometer for measuring the velocity of the wind, an instrument which belongs to the Meteorological Office, we have also an anemometer by Osler, for the direct measurement of wind pressure, which is the property of the Observatory. During the storm of Friday, the 6th ult., this instrument recorded a pressure of 51 lb. on the square foot, and yet it bore the strain throughout admirably. I may remark in this connection, as an interesting fact, that during the great snowstorm which swept over London and its neighbourhood on Jan. 18, 1881, the Osler anemometer at the Royal Observatory, Greenwich, registered as high as 51 lb. on the square foot. The tremendous storms which occasionally sweep over a country are, no doubt, originally due to the agency of solar heat disturbing the equilibrium of the atmosphere; and, no doubt, the day will come when a close physical connection will be established between those grand phenomena and the origin of nearly all the energy on the earth's surface. But it may be a long time before this conclusion is arrived at. One of the most serious difficulties which meteorologists have to contend with consists in the imperfect knowledge which exists respecting the climatic conditions of the upper regions of the atmosphere. Mr. Glaisher, by his aeronautic ascents, did good service in this matter; but still much remains to be done, and it is probable that balloon ascents, notwithstanding the danger of such enterprises, of which we have had recently a sad illustration, will continue to constitute the only practicable means for arriving at materials which will serve to throw light upon this important question. In connection with this circumstance, I should not omit referring to the series of meteorological observations conducted during last autumn on the summit of Ben Nevis, under the auspices of the Scottish Meteorological Society. This spirited enterprise deserves to be renewed, and it appears to me to be one of those to which the Government might wisely give some support in conjunction with gentlemen of scientific proclivities throughout the country. I would finally remark on the desirability of establishing meteorological observatories on the east coasts of England and Scotland. It is noteworthy that from Dover to the Orkney Isles there does not exist upon the coast a single meteorological observatory, except the one established at Aberdeen in connection with the Meteorological Office. This is a state of things which ought not to

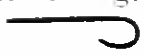
exist, and the want of such an institution was felt at the time of the Tay-bridge accident. Liverpool has set a noble example to other seaports in this respect, which is worthy of being imitated by the important seaports in the North of England, and by the ports of Leith and Dundee, in Scotland.—*Times*.

EASY LESSONS IN BLOWPIPE CHEMISTRY.

By Lieut.-Colonel W. A. Ross, late R.N.

Lesson II.

Having made his blowpipe, the student must now make, or provide himself with, a lamp, in the following manner:—The solid or closed end of an old iron gas-pipe, from 2 to 2½ inches in diameter, price about 2d., is the lamp, and a very excellent one it makes, as shown by my pupil M. Lombardi, of Argyll-place, Regent-street. The wick, a few strands of twisted cotton, held together and supported at one side of the gas-pipe by a piece of zinc-foil bent into the requisite shape, is now put in its place; and the fuel, consisting of any combustible hydrocarbon which solidifies on cooling, as pure beef or mutton fat, old "composite" or other candle-ends, cocoa-nut oil (from which glycerine has *not* been extracted), or all these together, added, and melted by heating the side of the lamp itself before the blowpipe by blowing the flame backwards on the iron rim; and here I must caution the aspiring blowpiper (or, as I prefer to call him, "pyrologist") against demolishing his work at this stage of proceedings by allowing the blowpipe-flame to play on the zinc-foil which he has just made! The wick, about the thickness of the end of a woman's little-finger, is best trimmed and cut with an ordinary pair of scissors, such as the American ones now sold for 6d. a pair.

So far, I feel sure, your working-men readers will admit that a blowpipe apparatus is by no means the expensive luxury it is thought to be; but the absolute necessity I am now about to mention, certainly does cost a little money, which will, I fear, somewhat strain his hard-earned and much-needed wages. I mean platinum wire, for old ends of which, however, half-price is allowed by Messrs. Johnson & Matther, Hatton Garden. This should be about the thickness of an ordinary horse-hair (I, myself, use it still thicker), cut into lengths of 3 in., and rolled into a ring about ¼ in. in diameter, at one end. This is best effected by the ordinary "cage-maker's pliers" of the ironmongers' shops; but anyhow, this ring must be carefully made, and as nearly a circle as possible, if only constructed on a pencil-point; unlike the slovenly figures recommended in some books on the blowpipe; some English works on chemistry even advising the use of a hook , which they tell you to fuse into a glass-tube by way of handle; to which piece of lunacy, I can only say, "Good gracious!" Here is a rough figure of a "platinum wire support," the natural size and thickness.

This wire is best held between the legs of a pair of ordinary "watchmaker's pliers," kept together, when required, by a little brass picture-wire rolled round the shank, so that this wire "strapping" may be slipped up and down; the "slipping up" closing the pliers tightly on the platinum wire, and enabling them to act as a handle for it; the "slipping down" opening the legs, and enabling them to act as pliers again. The pyrologist has thus not only a mere handle for his platinum wire, like the elegant ivory German ones sold at Freiburg, but an instrument by which he can effectually clean and straighten the latter for use. A mathematical pen also makes a capital holder for platinum wires. Now for my student's chemicals. Messrs. Herring & Co., the wholesale chemists, of Aldersgate-street, City (who, I dare say, have never heard of me or my blowpipes, but, for all that, I am a pleased customer of theirs), sell the exact kind of re-agent suitable for this part of my system of blowpipe analysis for 6d. an ounce, or less if taken in quantity; 2 ozs. lasted me four years. It is called "Glacial Phosphoric Acid," and cast in sticks. It is manufactured in Germany, and, I believe, contains from 15 to 20 per cent. of soda, without which it could not be cast; but it acts as a powerful acid before the blowpipe.

It should be broken into small fragments and kept in a wide-mouth stoppered bottle, as it is very deliquescent. To use it, shake out some of the smallest fragments on a white porcelain plate turned upside down, or any clean smooth surface. Make your wire-ring red hot before the blowpipe, and, touching the smallest pieces, which will thus adhere to the ring, heat them very gently, that is, about half an inch in front of the blue blowpipe-pyrocone, along with a few specks of oxide of manganese (of which two pennyworth will last most people a life-time). When the oxide is dissolved, which it is thus very rapidly, a charming amethyst-coloured "bead" is the result. Now plunge the bead into the middle of the blue pyrocone, and hold it there for half a minute; when withdrawn, it will be as colourless as a bit of pure glass. This effect may be repeated as often as desired.

EFFLUVIA AND HEALTH.

The query of J. Maccinslane, No. 183, page 231, is probably answered by one of the quaint and wise remarks of the late Professor Brande, who, speaking of sulphuretted-hydrogen gas, said that "it is not the stinking gas, but the bad company it keeps that is so mischievous."

It is well known to all who have worked at elementary analysis in a laboratory, where this gas is much used as a general reagent, that no mischief arises from breathing an intensity of stink that would, probably, induce fatal disease if it came from a sewer. The same applies to dissecting-room effluvia. In my student-days, in Edinburgh, we used to say that the above-named gas gave us an appetite, but, if placed in a witness-box, I could not swear that this theory may not have been invented to justify suppers at "The Rainbow."

Had the bullock's blood used in Mr. Maccinslane's dye-house been left in a sewer, and sown with germs of fungi, bacteria, &c., the results would doubtless be very different, unless the other chemicals there used are sufficiently volatile to poison the poison.

W. Mattieu Williams.

THE USE OF FLEAS, &c.

The query of "Amateur" reminds me of a paper I wrote many years ago, in which was revealed a discovery made during my wanderings in Greece. I slept—or rather reclined—in the capacious hovel of an Albanian farmer at Marathon, hard by the famous battle-field. My bed was the clay floor of the farm-house, my bedfellows were my fellow tourist, our dragoman and horseboy, the farmer, his wife and five children, besides our three horses, the farm stock of poultry, and a population of the animals under consideration, exceeding in number the possibilities of a census.

During the night the children awoke at intervals, crying piteously, and the fond mother, knowing why, picked them up severally, laid them on her lap, and scratched them liberally all over. After this they slept for awhile in peace. Hereby was suggested my discovery of the use of fleas to mankind. These people were not addicted to washing, which is also the case with a large numerical majority of the human race. The less washing the more fleas, the more *et cæteras*, and the more scratching.

The unwashed majority of human beings require some substitute for washing, in order to effect the necessary removal of the shrivelled and effete epidermal cells. Scratching is such a substitute. But I have shown above that scratching varies directly with the supply of fleas and *et cæteras*, and inversely with washing. Therefore fleas and *et cæteras* benefit mankind as well as themselves, and the obligations between the species are strictly mutual. Q. E. D.

W. Mattieu Williams.

FISH "SOUNDS."

In Dr. Andrew Wilson's very interesting paper on "Found Links" (Knowledge, Jan. 6, page 195), "the *swimming-bladder*, *air-bladder*, or *sound*," are described as synonymous. There is a small mistake here, a misunderstanding of fishermen's technology, that may mislead some readers. The same mistake occurs in anatomical text-books. Dr. Wilson will readily understand the nature of the error by simply buying some "cod sounds" from any fishmonger. They are regular articles of separate commerce, salted by millions in Norway, and exported in small barrels.

He will find that the sound is not the air-bladder, but the *aorta*, or chief blood-vessel of the codfish, laid open, and with some of the larger branching vessels attached. This vessel, forming a stout membranous bag, is attached by its edges to each side of the under part of the spine of the fish, from which the Norse and Newfoundland fishermen tear or rip it when they split the fish for salting.

I have a theory of my own concerning the etymology of the word, viz., that it is of Scandinavian origin, like the commercial article, and is derived from *Söndre*, to sever or rip, from which we also derive our word *asunder*.

W. Mattieu Williams.

Letters to the Editor.

[The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with their writers. All communications should be as short as possible, consistently with full and clear statements of the writer's meaning.]

All Editorial communications should be addressed to the Editor of KNOWLEDGE; *all Business communications to the Publishers, at the Office, 74, Great Queen-street, W.C.*

All Remittances, Cheques, and Post-Office Orders should be made payable to Messrs. Wyman & Sons.

⁂ All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.

All Letters or Queries to the Editor which require attention in the current issue of KNOWLEDGE, *should reach the Publishing Office not later than the Saturday preceding the day of publication.*

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

Our Correspondence Columns.

TELESCOPE. VOLCANIC PROJECTILES.—THE EARTH'S INTERIOR.—WEATHER FORECASTS.—ANCIENT MAN (AND MODERN WORMS.)—"THE STARS IN THEIR COURSES."

[255]—"A Country Solicitor" (letter 237, p. 275) is quite right with reference to the superior handiness of a refractor, and if he is willing to spend, as he says, £60 in the purchase of an instrument which he desires to be able to move about with moderate facility, I should think that a first-class 3¾ in. telescope on a portable universal equatorial mounting, surmounting a strong and heavy tripod stand, would be the very thing for him. This would enable him, as he says, to go through Webb's "Celestial Objects" with profit and advantage. He would not, of course, be able to see *all* the objects described in the fourth edition, as some of them are definitely stated to be tests for instruments of large aperture; but he would find that a large proportion of the double stars and nebulæ included in Webb's lists would be well within his reach. I should be tempted to insist upon the equatorial mounting, as, apart from the ease and comfort of following a star by a single motion, many objects are unrecognisable without it. On the other hand, for less money, my querist might obtain a 6½ in. reflector, equatorially mounted, too, which would give him superb views of the moon, planets, clusters, &c.; but this would be a massive affair, and there is always the nuisance of the mirror requiring re-silvering at longer or shorter intervals.

Mr. Rouso (letter 242, p. 276) may rest thoroughly assured that there is not an atom of foundation for the belief that a stone—or anything else—has ever been projected from a terrestrial volcano at the rate of 6·916 miles a second, the velocity needed to carry it into infinite space. The second part of this question is unanswerable, because the supposititious matter would never get out of the sphere of the earth's attraction until it got within that of some other body, and your correspondent does not say what that body is. If it be the moon, a projectile need only travel 2,141,956 miles above the earth! The query with which he concludes may be answered by saying that it has not been "proved" by astronomy that the earth is solid throughout, but that such solidity is rendered probable from the fact that she resists the tide-producing action of the moon exactly as a solid globe would do. Sir William Thomson has (according to Newcomb) shown that even were the earth less rigid than steel, it would, so to speak, exhibit tides itself, or be drawn out into an elliptical form; and then, earth and ocean moving together, we should have no tides at all. Moreover, the phenomenon of precession (now being described and explained in these pages by the editor) could scarcely occur were there nothing but a thin shell of rocky crust covering the molten interior of the globe, as such shell would slip round the fused nucleus, the liquid, in course of time, rotating in one direction, and the crust in another. Doubtless there are great cavities filled with molten matter, but these are insignificant compared with the size of the whole earth.

That "the Americans have not attempted to foretell fine harvest weather, or settled weather of any kind" (as stated by Mr. Dunkavand at the conclusion of letter 247, p. 277), is the very reason why I place a certain amount of credence in their predictions. Barometric indications of a gale afford the most absolutely certain data which exist for predicting a meteorological phenomenon. Our own Meteorological Office gives us wild shots because they have to say something; the Americans many fairly trustworthy ones, because they have something to say.

Is Mr. Snell (reply 84, p. 279) thoroughly sure that the intercalated quartzose sand and loam of the Nile Valley is the habitat of worms? because I am not.

I do not know whether the work is out of print, or whether (as is exceedingly probable), Mr. Bazley's own modesty has prevented him from referring in reply 184 (p. 279) to his own most beautiful book, "The Stars in their Courses." While heartily endorsing his recommendation of Proctor's "Library Atlas," which I regard as by very far the best one existing for the purpose for which it was designed, I may yet say that I equally look upon "The Stars in their Courses," as unparalleled as a means of learning the face of the sky. A FELLOW OF THE ROYAL ASTRONOMICAL SOCIETY.

WEATHER FORECASTS.

[256]—The remarks of Mr. Spiller (letter 121, p. 140) and "F.R.A.S." (letter 164, p. 207) ought not to pass without protest. The British predictions are not so bad nor the American ones so good as they represent, neither can the former be said to be mere "guess work," as insinuated by "F.R.A.S." The British forecasts, on the contrary, exhibit a large advance from a few years ago in our weather knowledge, and the general success which undoubtedly attends them is quite satisfactory to those who comprehend the grave difficulties of the subject. Prediction, in these islands, can never be otherwise than of a "general" character, and those who insist upon a particular forecast for every town, village, and villa in the United Kingdom, richly deserve the inevitable disappointment. The medical profession at times forecast the appearance of seasonable epidemics and the districts where they will appear, but what sane person would insist upon their walking over the said district and proving their prophetic instinct by pointing out the particular streets or houses where cases will occur. But meteorologists are asked to decide such minutiæ, and Messrs. Brown & Co. think it very hard that the thunder-storm occurring in their particular back gardens was not duly set forth in the day's bulletin. The fact is, that whilst the weather over defined districts maintains the same general features for a time, those features are subject to endless modification of a local character. More particularly is this the case with rain-storms, the non-prediction of which, by-the-bye, seems the only instance of failure given by Mr. Spiller. I have known rain-storms break out over areas of great extent, and within such areas there have been parts deluged and other parts with the finest weather (an instance occurring to my mind is the Whit Monday storm, May 25, 1874). I remember, too, during an excessively rainy autumn, a year or so ago, a "meteorologist" denying any unusual fall during the period, because his rain gauge had shown nothing extra. Again, I may mention an instance where the day's return from a certain district showed "very dry" weather, except at one station therein, where they had a whole month's rain in the twenty-four hours. Supposing another storm to occur like that on May 25, 1874, would Mr. Spiller or "F.R.A.S." expect the Department to say in what locality the rain would fall and in what it would not?

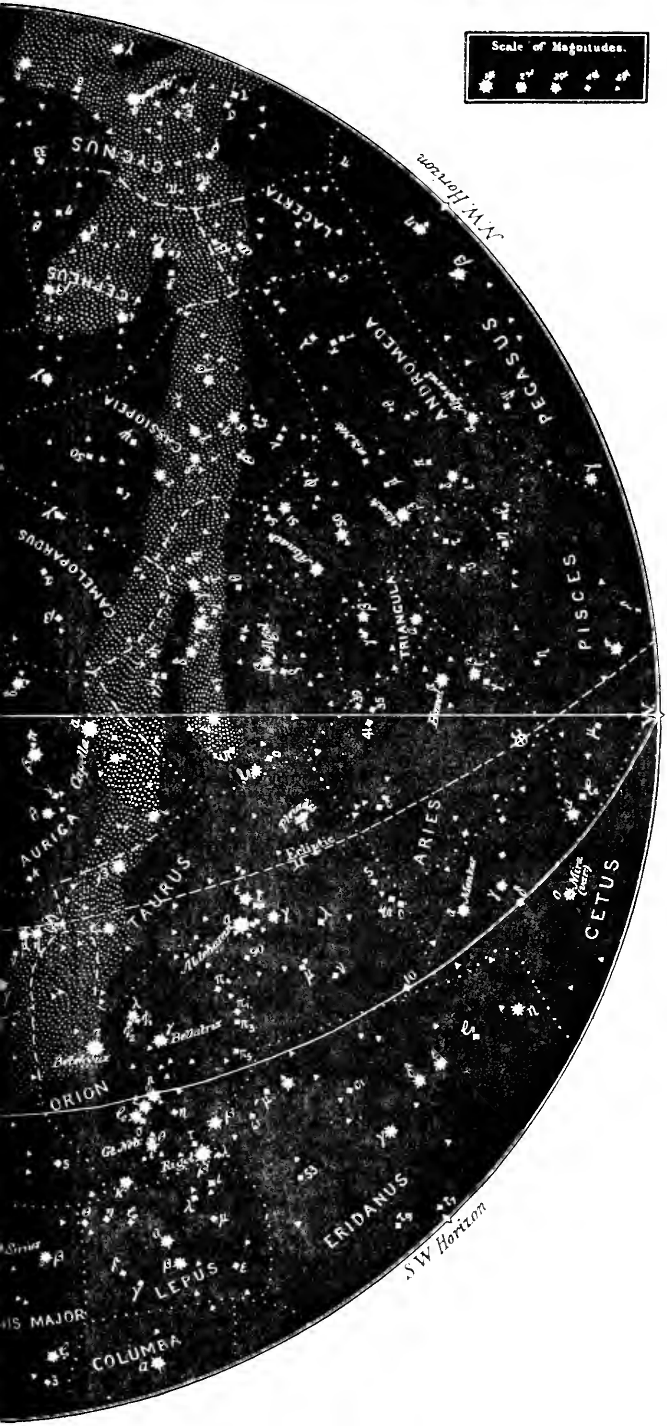
Singularly enough, your correspondents negative their own criticisms by the admiration they express for the American newspaper cablegrams. Putting aside the fact that they are quite as often "out" in their forecasts as our prophets are said to be (indeed, up to date—Jan. 18—four successive storms of theirs have failed to turn up), I must point out that the Americans take three days in point of time, and a coast-line extending from Spain to Norway within which their storms are to appear; and this, too, when the prediction is made after the storms have an actual existence. Will Mr. Spiller or "F.R.A.S." grant our own men equal indulgence? Three days' grace and almost unlimited areas for the fulfilment of their prophecies! Surely, after this, our own seers have little to be ashamed of!

I am not here disparaging the American warnings, for they fulfil to the letter my idea of what these forecasts must ever be—namely, general outlines both in respect to area and time. Beyond this, it is impossible at present to go, and, looking at the nature of the subject, I do not think we shall ever advance much further. In the meantime, it is to be hoped we shall not pour the whole of our felicitations upon foreigners, but accord some measure of praise to our own men, who have certainly not been less successful, and who, it is equally certain, have a much more difficult problem to deal with.

ALBERT P. HOLDEN.

STAR MAP FOR FEBRUARY

On January 29, at 10.30 p.m.
On February 2, at 10.15 p.m.
On February 6, at 10 p.m.
On February 10, at 9.45 p.m.
On February 14, at 9.30 p.m.
On February 17, at 9.15 p.m.
On February 21, at 9 p.m.
On February 25, at 8.45 p.m.
On March 1, at 8.30 p.m.
On March 5, at 8.15 p.m.



OUR STAR MAP.—The circular boundary … the map represents the horizon. The … ap shows also the position of the equator … d of that portion of the Zodiac now most … vourably situated for observation, with … he motions of the planets Jupiter and Mars upon them.—See the Zodiacal map … p. 225, No. II.

CHINESE CALCULATION.

[257]—In reply to your question respecting Chinese Calculation. let me state that in Sir John Bowring's "Treatise on the Decimal System in Numbers, Coins, and Accounts," there is a good description of the Chinese swan-pan, or abacus, and mode of using it. He tells us that the following is the Chinese multiplication table, the simplicity of which recommends the whole scheme. Ten tens are a hundred; ten hundreds a thousand; ten thousand a wan = 10,000; ten wans a zih = 100,000; ten zihs a chaon = 1,000,000. He adds:—"At early morn, one of the first sounds heard in the shops of all the towns and cities of China is the shaking and cleaning of the swan-pans, preparatory to the business of the day. As, in Christian lands, the sound of the bells calls the worshippers to Church, as, in Mahomedan countries, the voice of the Muezzin from the minarets bids the devout Mussulmans to prayers, so, in "the middle kingdom," the rattling of the abacus announces that another morning's labours are begun.

With that instrument the Chinese youth has been as familiar as with his hermetrical classics, the first and most popular of his school books. From it he has received the most correct impressions of the relations of numbers to one another; and he has acquired the habit of moving the balls on the wires of his swan-pan with considerable dexterity and rapidity; wonderful are the ease and accuracy with which all calculations are made and recorded. In my own person I have had to settle a great variety of accounts with various classes of people in China, and I never remember to have detected an error; and in cases where my reckoning has disagreed with that of the Chinese, I have invariably found that their account was correct, and my own erroneous. In China it might almost be laid down as an axiom, that a mistake in an account is in itself strong evidence of fraudulent intentions. I have compared my observations with those of persons of the longest and most extensive experience as to the general correctness of Chinese accountancy; and my opinion has been fully confirmed, that among Chinamen intending to be honest, an error in reckoning is almost unknown.

HONG-KONG.

COMMUNICATION WITH THE MOON.

[258]—It was Gruithuisen, the continental astronomer, who maintained that in his observations on the moon, by means of a large achromatic telescope, he had perceived immense cities, edifices, and artificial structures, apparently the works of some being existing there; and from these appearances he concluded that the moon formed a home for reasonable creatures, with whom we might correspond. This idea he communicated to Gauss, who replied that correspondence might be carried on, but with signs which all rational beings must have in common; such as the right-angled triangle, with the properties of which (Euc. I., 47), the Selenites must be acquainted. The plains of Siberia, or the Great Desert, might be selected for the purpose; and the required figure—a right-angled triangle, a circle, or an ellipse—be formed by channels dug in the plain and filled with, say naphtha. Even if the object was not attained, the work would provide employ for the hordes which, objectless and homeless, roam those wilds. But, provided that the moon is inhabited, and that by rational beings, our intentions and efforts would be most likely misunderstood. But why correspond with those that are not?

HERBERT R. WELLER.

PROBABILITIES.

[259]—Seeing an article in a recent number of KNOWLEDGE on luck and probabilities, I venture to ask your opinion on the following problem, which has occasioned animated discussion in more than one instance:—

A bets B an even sum of money that in three cuts of a pack of cards he will turn up an ace, a knave, or a nine. Of course, it is immaterial what particular cards are decided on.

I have seen this done over and over again, the stake being a shilling, and it has rarely happened that either of the parties has lost or won to any extent; in fact, so close was the running, that an Irish bystander came to the conclusion that the odds were even.

I should very much like to see the correct calculation of the chances, as I have known many attempts made to solve the problem.

At first sight the chances appear all against the cutter, and the bet is usually taken with alacrity, but I imagine that the odds are slightly in his favour, though I don't know why.

Barracks, Dundalk, Jan. 14, 1882. CHARLES A. EDES.

[The solution would run somewhat as follows:—The chance that at a single cutting one of the three cards will be cut is $\frac{12}{52}$, since there are four of each of the cards named (no suit being mentioned in the conditions), or $\frac{3}{13}$; therefore, the chance that none of the cards will be cut at a given trial is $\frac{10}{13}$. Hence, the chance that not one of the named cards will be cut in three trials is $\frac{10}{13} \times \frac{10}{13} \times \frac{10}{13}$ (by the well-known rule for such cases),

$$\frac{1000}{2197}$$

The odds are, therefore, 1197 to 1000 against A—that is, against turning up one of the named cards in three trials. The betting should have been about 6 to 5, or, more exactly, £5. 19s. 8¾d. to £5 against A.—ED.]

MESMERISM.

[260]—As a constant reader of your valuable paper I should like to be allowed to ask a few questions about mesmerism.

My reason for so doing is because, on Saturday evening, I put one of my boys in what I believe is called a mesmeric trance. In joking with him, I told him to keep his eyes on a white spot of paper in the centre of a penny. I then made a few passes in imitation of those I had seen made by professional mesmerists, when I was astonished to see him drop into a trance, from which no amount of shaking, pinching, &c., could wake him. At first, being considerably startled, I did not know what to do, but recollecting how I had seen mesmeric subjects awakened, I blew on his forehead, tapped him on the head and said "Right." He at once awoke, but for some time (half-an-hour) was somewhat dazed, doing things almost unconsciously. Being rather startled at finding I possessed this power, I should like to ask a few queries, viz.:—

1. What is mesmerism?
2. What ultimate effect has it on the subject?
3. How is the subject restored to his senses?

And any other information on the subject that will be useful.

A STARTLED ONE.

Wigmore Schools, West Bromwich, Jan. 16, 1882.

P.S.—I may mention that I have previously been a disbeliever in mesmerism.

[As animals can be mesmerised—to use this rather absurd term for want of a better—it is certain that there is some physical effect to be interpreted. Unfortunately, many professional mesmerists mix tricks with what they can do without trickery.—ED.]

TERRACES IN DORSET VALLEYS.

[261]—Anyone travelling in Dorset must have observed on the sides of the valleys a number of terraces, sometimes rising one above another like steps, varying in number and also in size. I have noticed some as large as giant earthworks, with slope as smooth and top as level as any garden terrace. Others, again, are a few feet in length and about one in depth. They are a peculiar feature of that county, but I have seen occasional outliers in Yorkshire and Cornwall. What is their origin? I have seen it asserted in a newspaper article, in an off-hand way, that they are artificial; but there is no conceivable end commensurate with the immense labour the construction would have entailed. They might be old sea-beaches, if only they were all horizontal. Can anyone say whether there is any accepted theory about them amongst geologists?

S. H. W.

ICE.

[262]—In your current number, p. 252 (208), "A Fellow of the Royal Astronomical Society" appears to have made a slip. It may be a fact, though very improbable, "that ice does not vary in volume, as other solids do, with variation of temperature." But this is by no means a corollary from the other fact, that water in becoming ice undergoes a greater change of volume than most other substances do in the act of crystallisation.

There are other known substances—notably certain bismuth alloys—which expand on crystallisation, and remain permanently larger than before congelation. Does "A Fellow," &c., apply the same assertion to these, and does he consider that ice remains constant in volume at all temperatures at which it is ice, or does he think that it continuously expands with decrease of temperature?

AN ENGINEER.

SHORTENING OF THE DAY.

[263]—Laplace satisfied himself by reference to ancient astronomical records (meaning, no doubt, those of Hipparchus, who lived about 125 B.C.) that no alteration in the length of the day ha

taken place, even to the amount of [illegible] of a second. Yet had there been a diminution of the earth's diameter, the day would have shortened. (See Whewell's "Hist. Ind. Sci.," 1837, vol. 3, p. 560, and vol. 2, p. 183.) Sir Charles Lyell says the diminution of the day is not $\frac{1}{2^{'}}$ of a second. ("Principles of Geology," 1867, vol. 1, p. 304.) "The earth's surface appears now to have reached a temperature which is virtually fixed, and on which the gain of heat from the sun is, on the whole, just compensated by the loss by radiation into surrounding space." (General Strachey, British Association, 1875, sections p. 181.) On the other hand, a scientific gentleman alleges that there are required "9,770,662 million cubic yards of crushed rock to produce the whole" of the fusing, heating, lifting, and wasted work at all the active volcanoes of the earth. Also he says:—"3. Heat wasted and dissipated in steam, &c., at volcanic vents, 5,842,848 millions of cubic yards." Who is correct? The present writer is no astronomer; he is

A GEOLOGIST.

[When Laplace expressed the opinion quoted, he supposed the lunar acceleration fully explained by his investigation. It has since been shown by Adams that about half still remains unaccounted for, unless the earth's rate of rotation is supposed to be diminishing. It was not till a very short time before 1867 that this result became generally known. Strachey's views and Mallet's, which are by no means opposed to each other, have very little to do with the question any way.—ED.]

FLESH FOOD.

[264]—Allow me to reply to "Practical's" note of Dec. 30, 1881. Hear what Dr. W. A. Alcott says on this very subject:—"The only instance which, on a proper comparison, will probably be adduced to prove the incorrectness of these views, will be that of a few tribes of American Indians, who, though they have extremely robust bodies, are eaters of much flesh. But they live also in the open air, and have many other good habits, and are healthy in spite of the inferiority of their diet. But, perfect physically as they seem to be, and probably are, examine the vegetable-eaters among them of the same tribe, and they will be found still more so."

Again, "Practical" must compare men of the same nation and people, and not of different nations; compare Englishmen and Englishmen, and not English and Spanish, as climate varies.

T. R. ALLINSON, L.R.C.P.

[Let the mixed-food advocates and the vegetarians meet on this ground, and make out each a list of, say, one hundred of England's greatest and best belonging to each class. The former might start with Shakespeare, Milton, Spenser, &c., Newton, the Herschels, and so forth, five or six for each class of distinguished men. The vegetarians could then name an equivalent number of each class.—ED.]

WATCH.

[265]—Can you or any of your correspondents explain the following circumstance, and suggest a remedy?

I have a very good watch, with chronometer balance, which on other people keeps admirable time, but will not go accurately when I wear it. Lately, I sent it to be cleaned, and had a similar watch lent me by the maker, which only varied thirty seconds a month. In a fortnight this watch had lost thirty-five minutes! Some people say this is caused by a magnetic condition of the body. Have others similar experience? and what can be done to prevent it?

CHRONOMETER.

THE MINHOCAO.

[266]—The attention of the public is from time to time called to the supposed existence of a sea-serpent of enormous size, and the question of its existence has of late found a place in your columns. Probably few people have heard of the Minhocao, a worm of, according to some accounts, fifty yards length, and five yards breadth, covered with bones as with a coat of armour, and in its burrowings rooting up mighty trees, diverting courses of streams into fresh channels, throwing up heaps of earth, and in its course making trenches about three metres in breadth. The reports of this animal, which has its existence in the highlands of the southern provinces of Brazil) seem well authenticated, and are as marvellous as those of the sea-serpent, if not more so. The accounts, however, as to the size and appearance of the animal are uncertain. It is supposed to be a relic of the race of gigantic armadilloes, which in past geological epochs are said to have been abundant in South Brazil.

The belief in this monster is not confined to Brazil, but is shared in by the people of Nicaragua, where a tradition of such a monster has existed from time immemorial; and as recently as the year 1866 a Nicaraguan *Gazette* gives a circumstantial account of an object much the same as the Minhocao. The accounts, however, of the Minhocao of Brazil are still more recent.

I have read that the Romans in their wars with the Carthaginians are said to have fallen in with a serpent 120 feet long, which dwelt upon the banks of a river and had tough scales.

As the existence of such an animal seems as interesting a subject of inquiry as that of the sea serpent, perhaps KNOWLEDGE may admit inquiries on the subject.

A. T. C.

FOSSILS IN METEORITES.

[267]—In No. XI. of KNOWLEDGE, you give an extract from the *Chicago Herald*, stating that fossils of sponges, corals, &c., have been found in meteorites, which, the extract goes on to say, are doubtless organic remains from an exploded planet. I have no doubt that they originally came from a planet, but the question is, What planet? The aforesaid *savans* say the exploded planet. But I believe it is infinitely more probable that this meteorite from which the fossils were obtained was originally expelled from the earth when first its solid crust was formed. The said fossils belong to the earliest geological epochs, and so far agree with this theory.

I believe Dr. Ball first set forth this theory, and I give nearly his words, extracted from the Editor's "Poetry of Astronomy":—"Meteorites are always angular fragments, even before they reach our air. Many meteorites have a crystalline structure, and, according to Haidinger, this indicates a very long period of formation at a nearly constant temperature—a condition only to be fulfilled in a large mass. . . . . Many meteorites show markings resembling those seen on terrestrial rocks, and caused by the rubbing together of adjacent masses." This, I think, sufficiently proves that they were expelled from some planet, and are not merely "dust of the system," which was, at some time or other, nearly captured by some planet in the process of aggregation.

And now we will attempt to find the most probable body for the origin of the aforesaid meteors.

To take the sun first. The requisite energy is certainly there, but where are the projectiles? The sun's body being certainly not solid, I think we may dismiss him as impossible. Then we find the moon. Here we find two objections. Firstly, if the projectile is given an orbit intersecting that of the earth, it will strike it in its first revolution, and so end its career for ever. If, secondly, its orbit does not intersect the earth, it will revolve round it for ever, and so never touch it. Thus, if we admit the moon to be the source of meteorites, its volcanoes must be still active, which is known not to be the case.

Thirdly, to take the case of the planets. Each planet may eject matter with such force as to pass out of reach of its attraction, but the chances are 50,000 to 1 against any such crossing the earth's orbit. But if we consider that every meteorite the earth expelled must cross the place of its ejection once in every revolution, we see that the chances are that the number of earth-born meteorites which reach the earth exceeds the number of those from other planets infinitely. Attraction of other orbs may, and no doubt does, cause the orbits of the earth and her meteorites to oscillate, but at some time or other they will come into collision. The fossils resemble those of the earth's earliest strata, and this was the surface of the earth in its intensely volcanic era.

To sum up, I consider the chances are that these meteorites were expelled from the earth itself, and carried some organic remains with them.

If there is a particle of evidence in the "exploded planet" theory, I should be glad to hear it. I do not wish to set myself up against these eminent *savans*, nor to contradict conclusions formed by years of toil and labour, but KNOWLEDGE is a medium through which the humblest may express their opinions, and as such an one I offer them.

VIGNOLES.

[It is hardly necessary for me to say that I share in large degree Mr. Vignoles' views, as the essay to which he refers will show. However, we have as yet no evidence of organic remains in meteors. The exploded planet theory, and the theory of two planets smashed in collision, seem to need no discussion. It seemed to me a jest when first advanced, and despite the gravity with which it has been recently urged by Sir W. Thomson, I can only regard it as a jest still. Despite the profound mathematical and physical learning of its author, even the assurance that he was in earnest would not lead me to regard it as deserving discussion. But like Professor Tait's notorious "sea-bird analogy," in explanation of the phenomena of comets' tails, it has not yet been advanced according to scientific rules. No attempt has yet been made to show that it explains observed facts, or that observed facts correspond with it in any definite way.—ED.]

Queries.

[219]—The Atomic Theory.—Where can I find the best account of the atomic theory, and whether any theory has been put forward for the insolubility of certain substances in certain fluids?—Ernest L. R.

[220]—Chemical Analysis.—Could you inform me of the prices of the best books on qualitative, quantitative, volumetric chemical analysis, and food analysis?—Onward.

[221]—Natural Philosophy.—Which is the best "History of Philosophy" for students?—A. Summerson.

[222]—Telephone.—How could I construct a small telephone, and how do you join the wire together, so not to interfere with message sent?—G. H. Mortimer.

[223]—Aniline Dyes.—Could you kindly give me any information on the manufacture of aniline dyes, or tell me where I can learn anything about them?—Ernest L. R.

[224]—Electrical.—Would any kind reader of Knowledge inform me how to make the "rubber" for a cylindrical electrical machine; also whether it is necessary or not that the cylinder should be covered with shellac varnish?—A Greenock Student.

[225]—Ivy Leaves.—Of the various common plants which grow in these islands, ivy seems to me to show a wonderful variety of leaf-forms, depending upon the locality in which it grows. I should feel greatly obliged to any evolutionist who would explain the probable causes which produced these varieties.—E. C. R.—[The query is accompanied by drawings of several varieties, but as they are well known, it does not seem necessary to have the drawing engraved. We have sent it to Mr. Allen.—Ed.]

[226]—Gravity.—Reading the leading article on p. 241, I am quite at a loss to understand the following, taken from paragraph four:—"The most careful observation of the planets' motions reveals no evidence that gravity takes even any appreciable time at all in traversing the spaces separating the various members of the solar system from each other." As we can never have any atom of matter outside the influence of the gravitation of every other atom in the universe, how can the velocity of this influence be measured? I think it would be interesting to the readers of Knowledge if you or some one would favour us with an article explaining how this conclusion has been arrived at.—F. A. L. R. [It can be shown that if gravity occupied a measurable time in reaching a planet, there would be a force constantly hastening the planet's motion. But the matter requires more space than can be given in "Replies to Queries."—Ed.]

[227]—Barometric Oscillations.—Are wave-like oscillations of the barometric column in gales matter of usual observation? My attention has only been lately attracted to the fact. When the atmosphere is in a disturbed state during the passage of a cyclonic system, I set the vernier of the barometer accurately, and watching it closely for five or ten minutes, find that the column rises and falls say the thousandth of an inch, or, if falling, *falls by waves*, like the receding tide on the shore; the intervals between the waves being from thirty to sixty seconds or thereabouts. This may be a fact known to all observers, but, being new to me, I venture to ask the question, and to solicit a physical explanation of these oscillations.—G. R. W.—[If you consider that a barometer indicates atmospheric pressure, and that there must be waves of compression and rarefaction during a storm, you will infer that the oscillation you describe might have been expected. We do not know that it has ever been specially noted before.—Ed.]

Replies to Queries.

[151]—Jordan Barometer.—The inventor gives the sp. gr. of the glycerine as 1·26 at 60° F. (*vide* "The Glycerine Barometer," E. Stanford, Charing-cross, London, price 1s.). The glycerine employed is presumbly pure, as the above specific gravity corresponds to the natural density of the liquid, as stated in Watts's "Chemistry."—Edina.

[155]—Tortoise.—We kept a tortoise more than ten years. When he began to burrow for the winter (moving his body in a rotatory manner) we put him in a wooden box without lid, with a few square inches of carpet over him, and stowed him away under the sofa in the library. He was never deceived by premature fine days in January or February, but when the true spring was coming, he stood in the box on his hind legs, and with his fore-foot tapped to be let out, and we then turned him into the garden for the summer. He became very tame, and I often held him up in the open, and on speaking to him, "Come, Torty," he would put out his head to have his poll rubbed. -Charles Few.

[171]—Choanites.—Having examined many choanites polished in the mass, and cut into thin sections so as to admit of the employment of the microscope, and having compared them with the living sea-anemones, I had come to the same conclusion as Prester W., that the choanite is a silicified sea-anemone. The structure of the tubes is alike in both.—X.

[171]—Choanites.—The answer given to the above query, at the bottom of p. 251 of Knowledge, No. 12, is not satisfactory to me. Probably your correspondent and I do not mean the same thing when we speak of "Choanites." They differ widely from what we call Alcyonites. He may see specimens of the fossil Choanite in the British Museum, where they are described as "silicified sponges, from the chalk" and I think that those specimens show the spiral worm round the cup, but, of course, both the worm and the cup are in the fossil filled with coarse flint, and having been slit by the lapidary, the former appears as spots of flint, as intersected on each side by the wheel, and the latter as a straight, solid body of coarse flint, with which the lateral tubes are connected. If the Choanite, after all, is a sponge, as geologists say it is, it is easy to understand how the worm may be—I had almost said must be—a parasite.—Your obedient servant, Prester W.

[176]—The Blue Streaks in Brick Clay.—These are most probably disintegrated and decomposed sulphuret of iron, which is abundant in the plastic clay.—X.

[177]—Nautilus.—The use of the gut running through the chambers of the nautilus is to secure it to the first-formed chamber of the shell, as it has a periodical slipping of the muscle, which is repeated thirty or forty times during the growth of the animal, each time forming a transverse septum to resist the pressure from without.—C. W. Oldfield.

[185]—Cold Saturated Solutions.—Have you tried taking a *hot* saturated solution of this salt, allowing to cool with constant agitation, and then leaving it to stand over the night, at the temperature you want? Evaporate carefully down to dryness, without spirting, a measured quantity, say 50 grammes (not CO) of the solution, and weigh the residue; the result, multiplied by 2, will give per cent.—A Greenock Student.

[193]—Action of Thunderstorms.—Owing to the formation of ozone, which acts as a powerful oxidising agent. Ozone is formed by the passage of electricity through the atmosphere.—A Greenock Student.

[214]—Colours of Stamens.—I think T. Howse is scarcely fair in testing Mr. Grant Allen's opinion as to the original colour of flowers by plants grown in a conservatory, whose characteristics are so much modified by cultivation. When one speaks of flowers in a botanical sense, one means those grown in a natural state, *i.e.*, wild flowers. If T. Howse examines these, he will find not only that with few exceptions the stamens are yellow, but, in a large number of cases the corolla is yellow also, as buttercup, potentilla, dandelion, primrose, furze, &c.—F. D. H.

Fermentation in Beer. — I see "F.C.S.," in reply to "In Re" (p. 257), recommends him to read some work on brewing, preferably Dr. Graham's lectures, "if published." May I be allowed to state that they are published in the form of a pamphlet. I have not my copy at hand, unfortunately, or would quote publishers. May I also suggest that "In Re" read the articles, "Beer and Brewing," in Ure's "Dictionary of Arts, Manufactures, and Mines" (Longmans & Co.) and "Studies on Fermentation," by Pasteur (English translation published by Lyon, 175, Strand, London)? "On Fermentation," by P. Schutzenberger (International Scientific Series, King & Co., London), is also well worth reading, but the thanks of English brewers are due more especially to Dr. Graham. —W. M.

The limiting depth to which light penetrates in water was some time ago stated to be 40 mètres for Lake Leman, by Prof. Forel, who used albumenised paper in his experiments. M. Asper has recently made similar experiments on the Lake of Zurich by a slightly different method. He used the photographic plates called *emulsion plates* (more sensitive than albumenised paper), and immersed them during the night of Aug. 3, to depths of 40, 50, 60, 70, 80, and 90 mètres. They were brought up after remaining twenty-four hours in the water, and treated with oxalate of iron. All the plates, without exception, were distinctly affected by the light. Thus the chemical rays penetrate in clear water to at least 90 mètres deep.—*Scientific American*.

Answers to Correspondents.

*** *All communications for the Editor requiring early attention should reach the Office on or before the Saturday preceding the current issue of* KNOWLEDGE, *the increase of circulation of which compels us to go to press early in the week.*

Hints to Correspondents. 1. *No questions asking for scientific information can be answered through the post.* 2. *Letters sent to the Editor for correspondents cannot be forwarded; nor can the names or addresses of correspondents be given in answer to private inquiries.* 3. *No queries or replies savouring of the nature of advertisements can be inserted.* 4. *Letters, queries, and replies are inserted, unless contrary to Rule 3, free of charge.* 5. *Correspondents should write on one side only of the paper, and put drawings on a separate leaf.* 6. *Each letter, query, or reply should have a title, and in replying to letters or queries, reference should be made to the number of letter or query, the page on which it appears, and its title.*

C. M. Thomson. Very much gratified by your favourable opinion; our sub-editor (S. D. P.), in particular, thanks you. We note and watch the numerous leaks you mention. J. P. Sandlands. About luck, in article; Huxley meant by extra Christian, outside of, unconnected with, Christianity; you say "this is hardly the usual acceptation of extra"—pardon me, but I think it is (in extra-judicial, extra-ordinary, extra-logical, extra-mundane—in every word compounded with extra this is the sense). "Is it true," you ask, "that science is extra-Christian in this sense?" Surely; if not, it ought to be. What has science to do with Christian doctrines, or Christian doctrine with science? Next, as to Dr. Wilson's "Found Links," you ask how descent of air-breathing from water-breathing animals is proved by the linking of frogs and fishes through the mud-fish. For the life of you, you say, you cannot see how that, because one thing is something like another, it must have been its parent. Well, then, it was not for you Dr. Wilson wrote. Others showed that along a certain line of descent (according to the theory of evolution) links were missing; Dr. Wilson showed that this was natural under the circumstances, and he is now showing that along other lines the links still exist. You raise an entirely different question: for, if similarity of structure is no indication of relationship, there is no way of determining missing links at all, and it is idle to look for them. Geologists might turn up to-morrow the fossil of a creature presenting Simian and human features combined as we might expect to find them in a being midway between man and the supposed common ancestor of men and apes: but of what use the discovery, either way, if similarity is no proof of kinship? Arthur Stradling. Our space does not suit an article on that scale; Ariel, Puck, gnomes, djinns, and pixies, are for regions where there is more dancing room.—G. A. M. Your query otherwise answered; we cannot undertake to forward letters from one correspondent to another.—C. Lloyd Engstrom. See, presently, article on "Fallacies." Alex. Blake. Where there is no struggle for life, there is none. You show this clearly. Then you ask, Why should there be? Why indeed?—J. Horne. Your solution neat; see later reply, with general remarks on the problem.—C. A. E.—I can assure you I have not the slightest intention of allowing astronomy to drive out other subjects of interest. Like yourself, I was not pleased with the change which came over the journal you mention; it was one of the reasons why I ceased to contribute (as I frankly explained to the sub-editor). You need not fear that there will be any change in that respect in Knowledge, so long as it remains in my charge.—Phacopo. I cannot explain why your friend, forty years old and six feet in diameter (I beg pardon, that is the clock), can tell the time by a church clock at a distance of a mile. It seems clear he has excellent eyesight.—Edina. By stitial colures, I presume Mr. Bulley meant the solstitial colure.—Investigator. Having some sympathy for our contemporaries, we feel obliged, in common honesty, to say that the dialogue you quote ought not to be sent to any of them. Those who are likely to be misled by the arguments dealt with are simply those who have no reasoning power. Why reason with them then? I have seen a good deal of the gentry—the teachers are knaves, their followers otherwise. Sirius. You are right; it was the sun and not the earth whose mass Professor Young gave as 2,000 trillions of tons. Ocean. The lecture as given has not been republished—the theory dealt with is given in one of the essays of my treatise, "The Poetry of Astronomy." You say that the "top-gallant forecastle," about which I asked in my "Pleasant Ways in Science," is quite correct. But you define it just as I should define forecastle, and you tell me what the cross-trees are, saying they are topmast cross-trees, not top-gallant cross-trees. I have never heard either expression used, but always simply "cross-trees," and I have known what cross-trees were since I was ten years old. I still think the expression, "top-gallant forecastle," unusual, to say the least. I supposed the writer meant the cross-trees. It seemed to me as absurd as "cutting the water with her taffrail," in the "Red Rover."—W. A. C. I have not elected to vivisect Rover, but I have known of the better fruits of the practice, which, unless directed to such ends, I regard with as much abhorrence as you can. I admit that the argument about breathing is absurd; I did not urge it as otherwise; I only said it might as reasonably be urged as your own. I do not suppose my esteemed correspondent, F.R.A.S., meant one who had been limited to potatoes and turnip-tops; he used a familiar mode of speech. If I found anything in your letter which was more than simply a denial of his view, I would insert it. You cannot say I have not given due hearing to the other side.—Nelumbo. It is absolutely impossible, with our present circulation, to cut the pages. Which would you rather have, four pages more of original matter, or be saved the slight trouble of cutting the paper?

Student. It may be quite safely assumed that the total heat received from the stars is quite insignificant. Pray send account of the dog who inherited kleptomania.—M. Wyatt. The formulæ are practically identical, s being insignificant compared with r. Test the matter, if you doubt this. Thanks for the problem from Newton's "Universal Arithmetic," to which work, however, we have already referred readers.—A. W. D. Will try to find space for your suggestions about sleep and study. Only know of Bain's book against Phrenology.—R. B. Rowlinson. You are right, we should have said 2,700 B.C. not 2,700 years ago. The stars you name will be near the southern pole at different times. My Gnomonic Star Atlas is, I think, convenient for reference in such matters. At least, I always use it myself, and added the longitude lines and circles to make it useful in that way.—D. F. Barrett. Sorry it was not attended to, but the correspondence both with publishers and editor has been literally overwhelming.—Δοκη. You are right; the question is whether vivisection is right or wrong, first, *at all*; and secondly, if under any conditions, then what those conditions are. Opponents are called sentimental; advocates are called brutal. Those who occupy a middle position are called names by both the extreme parties. Of what use is vituperation, anyway?—R. R. We do not know the work. "He combats the views of six of our leading scientists concerning evolution," does not sound promising. Good writers do not go about combating views.—Celt. Your theory that as the pyramid rose, the builders heaped up earth all around to enable them to put on the next layer, and afterwards cleared this matter away, seems only a little less ingenious than the theory that it was built from the top downwards. Have you formed an estimate of the tremendous extra labour the plan would have involved? Builders' measurements would be absolutely ineffective to preserve the accuracy of the orientation. An astronomer would not be content to discontinue the observations, but would make the orientation more and more accurate as the building rose higher.—Ellipse. You seem to overlook the fact that Mr. Thorp's first instrument is meant for ellipses too; it is only *set* for parabola. H. Moulton. Inquire how many papers of large circulation cut their edges. The *Saturday Review*, the *Spectator*, the *Illustrated London News*, the *Graphic*, sixpenny papers, do not. *Punch*, *All the Year Round*, and a host of other papers less cheap than ours, might be cited as not cutting their edges. We never expected to continue the plan, any more than we expected to continue the issue of specimen copies by thousands. If your copy happened to be badly folded, the fault is unusual, so far as we can judge from the examination of a great number of copies taken at random. In the bound volume all irregularities will disappear; and we beg you to notice that the edges of the cut copies must again be cut when they are bound up. If you would but consider what we are trying to do in the way of cheapening science, you would be a little more generous than to ask for what is really only a luxury possible with comparatively dear papers, or with papers having only a limited circulation. We increase the quantity of original matter and the average size of the paper, and with reference rather to our promised than to our (very promising) actual circulation; you, and in all five correspondents, wish us to go back to the arrangements made when we were beginning. We beg to assure you, that except for the question of time, which absolutely prevents our acceding to your request, it would save us considerable expense to accede to it, if we at the same time diminished the original matter to the proportions which it had in Part I. You will see this clearly before the end of the next month.

James Moir. The phenomena are manifestly subjective.—Amateur Botanist. Surely botany has had a very fair share of our space. We could not publish "at extra cost" a supplement of star names and letters. Are not the letters and names given with the name of each constellation in the maps themselves?—J. N. Leigh. No evidence that earth's axis has changed.—R. C. Auld. You have, we trust, now received Part I. (that is, it reached you, we trust, before it was as it now is out of print). What happened was this: Your letter, enclosing stamps, was addressed to "editor"; it was sent to us, and its turn came a week or so after. It was then forwarded to the

publishers; and your later letter, being also addressed wrongly, is opened probably a week after the paper reached you.—H. ROLFE. Do not know Richardson's Conic sections. For analytical conics, Todhunter or Salmon; for geometrical, Drew or Tayler would suit, I think. You require a book suitable for fourth stage, at Kensington. Perhaps some correspondent will help.—ANONYMOUS, SILVERDALE. Darwin's works are published by Murray Lyell's "Student's Geology" and "Principles of Geology" would be good works to begin with; say, first, the cheap edition of the former work published by Murray.—M. S. S Already corrected; but thanks all the same.—A. BLUMENBACH, G. JOHNSON, H. STEINMAN, and others. Nos. 2 and 3 are now out of print. They cannot be reprinted; this has already been done three times, at a serious loss.—FRED. CRAMPTON. Tell us about the monkey, by all means E. MUIRHEAD LITTLE. We did not understand F.C.S. to mean that carbonic oxide is non-poisonous, or a supporter of combustion Everyone with the most elementary knowledge of chemistry is aware that it is more poisonous than carbonic acid or the carbonic dioxide, and that it is no more a supporter of combustion than the latter. What we understood him to mean was that wood gas, after the carbonic acid in it had been converted into carbonic oxide, was changed in character, as described. Coal gas contains both CO and CO_2. Read as you understand it, "F. C. S.'s" communication would describe wood gas as simply carbonic oxide, which is, of course, simply absurd.—SATELLITE. The card of your compass must be badly suspended. Hold the compass horizontally, and if the card evidently "sticks," tap the sides till the card vibrates freely, it will then swing round till the north end points to the magnetic meridian (about 21° west of true north. A "Knowledge Almanac" may be thought of hereafter: at present, KNOWLEDGE occupies all our care. A. J. MAAS. Thanks for snake stories.—T. J. Sun extinguishing fire dealt with in article on "Fallacies." The common idea that sun puts out fire is very different from the theory you deal with, that a fire burns less quickly in a room whose air has been warmed by the sun (after a certain considerable time). Sunlight admitted into a room does not warm the air in the room appreciably for some time; the fire is supposed to go out quite quickly, which certainly does not happen.—H. B. R About tobacco immediately.—CL. A. SEGLER and F. GAUBERT. We knew Zares made the mistake you mention. Our answer was quite correct. There is no need of the isolation you suggest to produce conditions under which the least possible force will move the greatest possible mass. But at what rate? Zares said nothing about that. We know perfectly well where and how he is astray; but it is more useful to let him find his way to the right road, than simply put him on it and leave him *planté là*. J. BROADHURST. American humour is as distinct from English humour as possible, as are American ideas about humour from ours; or, I should rather say, from ordinary English ideas on the subject. The actor approved by English tastes is often not liked in America; and *vice-versâ*, favourite American actors are often regarded with little favour here. Again, if you go with American friends to a play in England, you will find that what the English audience like they do not care for; while they at once select as the best actors those who are not regarded as absolutely in the first flight by most English critics. I must confess it seems to me they often show a more refined taste than ours,—perhaps because I find their views in agreement with my own. For instance, I have always regarded Rutland Barrington as one of the very best of our histrionic humorists, but his quiet humour seems very little appreciated compared with the more grotesque fun of other actors on the same boards. Now I find Americans quite at one with me in this view. The oftener they see him the better they like him. It seems to me, by way that so consummate a judge as M. Got, of the Comédie Française, was of the same opinion, even when he saw Barrington in a part so comparatively ungrateful as that of Captain Corcoran. Americans say that ninety-nine Englishmen out of a hundred fail to understand American humour at all. I remember an English fellow-passenger on board an American Pacific steamer, to whom the most outrageous absurdities were addressed with a certain grave calm (not solemnity), which should have been as suggestive of fun as Barrington's manner in telling us of Teazing Tom; but he took them in as confidently as he received the announcement of the day's run and the ship's latitude and longitude, and for aught I know, entered them in a big book about his travels which he was writing. He was "a perfect gold-mine of fun," a Californian said.—MUSICUS. You may be right. Conversation when music is going on may set the performer at his ease. He might be still more at his ease if the audience stopped their ears with cotton wool, and still more so if they all went out of the room. But the object in view when any one is invited to play, is not to set him or her at ease, but to hear sweet sounds discoursed. If a player is so inferior as not to be at ease, common sense suggests that he should not be invited to play. If, on the other hand, he plays well, you may depend he will not be set at ease, but very much the reverse, if he is not paid the compliment of silence. But to say the truth, no one who knows what music is either talks when others play, or does what you seem so to desire to do, talks when playing himself. I have heard many musicians speak of the habit some unmusical persons have of talking when music is in progress, and they have one and all denounced the practice as an offence to the audience and or insult to the performer.—P. F. A. Your assertion that a strong artificial light will put out a fire would entirely dispose of C. T. B.'s explanation, which yet you say is the only one you can arrive at. It is very easy to make a few experiments either on the effect of sunlight, or of strong artificial light. First get a good, steadily, blazing fire, with the shutters of a south-facing room closed, on a sunny day. Open the shutters for a quarter-of-an-hour, and note how during all that time the fire appears dull and languid. Close the shutters, and note that as soon as the eyes have become accustomed to the change, the fire appears as bright as ever. As you say, it does not appear so at once; but that is simply because the effect of sunlight on the eyes does not pass off at once. I do not myself agree with F.R.A.S. in regarding the idea that sunlight puts out a fire as a mere vulgar superstition, but rather as a very natural illusion. Moreover, there can be no doubt that solar heat, admitted long enough into a room to appreciably increase the temperature of the air, does, to some degree, diminish the activity with which a fire burns. This is no more a superstition than the perfectly correct idea that fires burn brightly in frosty weather. But you must remember that F.R.A.S.'s remark was altogether impersonal; it was not applied to the belief of the querist, for none was indicated: it expressed only F.R.A.S.'s view respecting the idea about which N. inquired.—J. F. The earth's axis is inclined 23° 27′ 16·60″ from a perpendicular to the plain of her orbit. The equality of action and reaction does not—exactly—*mean* "that if a man in a boat pushes against another boat ten times the weight of his own, the heavier boat would go one tenth of the distance of the lighter one"; though something like this would follow from the law: it means, more generally, that whatever pressure, strain, or action is exerted by one body on another, excites an exactly equal pressure, strain, or action in this other body, acting in the opposite direction.—M. J. HARDING. I have taken the liberty of forwarding your suggestion to the publishers of my "Easy Star Lessons." But surely it would be rather hard if an author were regarded as responsible for the pictures put on the binding of his book. As to the other work, considering who honoured the author in the way you mention, and the absurd remark he made, I should attach no weight at all to his opinion. How on earth could the writing of a scientific treatise be regarded as equivalent to a course of education specially fitting a man for ministerial duties? It was the hard hitting, I should imagine (and infer from the title of the book), which pleased the bishop. The works attacked seem to me perfect models of what scientific works should be, presenting accumulated knowledge, attacking none, courteous to all, even to opponents. A book attacking such works, and having for its title what implies that those attacked are godless and wicked men, carries its own condemnation on its title-page. That it should have run to the twelfth edition would show that there are many who enjoy such attacks, but would prove nothing as to the scientific value of the treatise.—R. LEE. We cannot find space for *all* the titles of Mr. Dallinger's essays. We fear the original query should not have been inserted. We might fill our whole number with replies, if many such questions were asked.—A. LUMMERSON. Have inserted one of your queries; as to the other, the person named is considered a dreamer in his own country, and has no scientific standing either there or here.—W. G. S. Certainly you heard the 800 puffs in nine seconds less time than was required to produce them. In the case of an approaching train, you always hear the sounds in quicker succession, and in a receding train, in slower succession, than if the train were at rest, or if you were a passenger by it.—S. H. W. Nay if a cannon-ball were sent round the earth without any initial rotational motion, it would in each circuit present all parts of its surface towards the earth not constantly the same face. The other passage seems carelessly written. If a mass of air came from the pole, in a moment, to our latitudes, nothing could bear the brunt of passing through it. But north-east winds come only from higher latitudes gradually to our own, and the deficiency of their original rotational motion is gradually made up by the frictional action of the surrounding air, earth's surface, &c.—T. S. V. P. Read Wallace's book on modern miracles if you want the sort of faith you refer to. We cannot admit the subject here until it has, or some part of it has, a scientific standing.—JAS. DEVLIN. Yes; we quite mean that the builders of the Great Pyramid could not possibly have oriented it so perfectly

as they did without telescope, and, unless they had employed such methods as we find they actually did employ. The work was much more difficult than you seem to think.—Hugh. Every reflecting telescope (except those of the large Herschelian type) has two reflectors, which are not meant to be used separately. Telling me that the eye-glasses are both coloured red, one lighter, the other (as I might almost have inferred) darker, does not enable me to understand what is wrong. You should get an optician to look at the instrument.—Onward. Your first question out of our line.—W. A. Sadler. We think not; but if you will describe any experiments showing *how* sun rays might build up a planet, we shall examine them with exceeding interest.—Reporter. No room for shorthand discussions.—Ernest Groth. Much obliged. Have already, however, inserted a reply similar in effect to your own very complete one.—G. H. Mortimer. What can you mean? What *is* the substance of a light? In a gas-flame, the light is due to glowing carbon, the heat coming from the combustion of hydrogen.—W. E. B. F. C. S. Did not meet Mr. Severn in Australia; but he was well-remembered there, as were his lectures on astronomy. Do not know his address. Probably Mr. Ellery (Melbourne) may know.—E. B. V. I considered most of the phenomena you refer to in an article which appeared fifteen years ago or so in the *Intellectual Observer*. I may deal with it shortly in these pages, but so many subjects crowd in upon Knowledge, that it is hard to know which to take first. The apparent changes in brightness are only subjective, not objective, phenomena: they depend on the different brightness of different parts of the background. The shadow of IV. ought to be larger (including penumbra) than that of III., IV. being so much further from the planet.—J. A. L. R. How would your explanation apply to the moon? The enlargement certainly is an optical illusion, as measurement shows. The climate question will be raised in articles on Precession. Sun's proper motion wants an article for its proper elucidation. In my "Essays on Astronomy" it is discussed rather fully. The velocity has not yet been measured, and cannot be. Otto Struve's reasoning was quite unsound. As to the Vectors question, there is an obvious misprint. Is it not rather unfair to ask questions of that kind, leaving us either to occupy a portion of space for the query and replies, besides giving trouble to readers and to us in reading their answers, or else to send us to look up the book, which may or may not be ready to our hand? If all our readers who encountered such difficulties sent us queries, our whole space and more would be wanted for them.—T. W. Yes; the guillotine cutter works easily enough when fed; it is the feeding takes the time. The extra expense, when we are doing our best to give as much extra matter as we can provide for out of our scanty margin (or, rather, looking forward for the margin we hope to have when full grown) counts, of course, for nothing. The "nice little job" you have "cut out for our readers as well as ourselves too" we beg respectfully to decline. In an article such a matter may be discussed, not in replies to queries.—H. Snell. Thanks. Another letter pointing out the same mistake is in type.—Eolus and Boreas. It was your own joke about storms being sent us out of a gun. Did you think we "took you" seriously? Your questions too wide for the kind of answers you want.—J. A., L. M. B., A Carthusian, and others. Thanks for various solutions of the four fours problem, or approximations thereto.—Guinois. Yes, the errors are there, but one is an obvious misprint, and neither affects the result.—P. B. You are right: that "was what we intended to convey, officially."—W. B. Corrected already.—P. A. MacMahon, T. R., and others. The problem is not difficult with the Dif. and Int. Cal. "No analyst" wanted a solution he could follow. Thanks, however, for your solution.—J. Hammard. Much obliged for your solution of the messenger problem. We had already sent a geometrical solution to the printer's. At present we have scarcely room for mathematical essays; two have been waiting for room since the fourth number. We agree with you about leaving mathematical "problems" for a week, but not mathematical questions, simply because querists may be anxious for early reply. The simultaneous equations later.—E. J. Kennedy. Your solution will not do; the "by symmetry" assumption is unsound. Note, that you get, besides the solution, an equation of condition. Why should this equation hold?

The Future of Solar Physics. The fundamental problems now pressing for solution are, "first, a satisfactory explanation of the peculiar law of rotation on the sun's surface; second, an explanation of the periodicity of the spots, and their distribution; third, a determination of the variations in the amount of the solar radiation at different times and different points upon its surface; and fourth, a satisfactory explanation of the relations of the gases and other matters above the photosphere to the sun itself—the problem of the corona and the prominences."—*The Nation.*

Notes on Art and Science.

The Earliest Date of a London Fog.—What the earliest record of a London fog may be I am unable to state; but since Mr. Hales has mentioned Evelyn's name in connection with a fog noticed by him in 1684, it will undoubtedly have struck more than one of your readers that twenty-three years previous to this, the delightful old diarist had published his "Fumifugium, or the Inconveniencie of the Aer and Smoak of London dissipated, together with some Remedies humbly proposed by J. E., Esq.," &c. "Published by His Majestie's command," MDCLXI. The warmth of expression used in this little tractate, now very scarce, would certainly point to the long-settled existence of the fog and smoke nuisance in the metropolis; indeed, the energy of the attack of this original Fellow of the Royal Society, his notice of the injury done to the health of the inhabitants, to the public buildings of London, and the furniture and "movables" generally, singularly coincide with the sentiments expressed about us in the present day. In the midst of our boasted civilisation and advance, the words of this accomplished gentleman of the seventeenth century are as much to the point on the question of the London fog and smoke nuisance as they were 221 years ago. Evelyn mentions ("Diary" Dec. 15, 1670) "the thickest and darkest fogg on the Thames that was ever known in the memory of man."—F. Carew Martin.—*Athenæum.*

Chloroforming during Sleep.—The possibility of chloroforming a person in sleep, without waking him, having been disputed in a recent murder trial, Dr. J. V. Quimby, of Jersey City, was led to test the question experimentally. The results were presented in a paper before the section of Medical Jurisprudence at the meeting of the Medical American Association, a few days ago. Dr. Quimby made arrangements with a gentleman to enter his room when he was asleep and apply chloroform to him. This he did with entire success, transferring the person from natural to artificial sleep without arousing him. He used about three drachms of Squibb's chloroform, and occupied about seven minutes in the operation. The second case was a boy of thirteen, who had refused to take ether for a minor operation. Dr. Quimby advised the mother to give the boy a light supper and put him to bed. She did so, and Dr. Quimby calling when the boy was asleep, administered the chloroform and performed the operation without awakening the boy. The third case was a boy of ten years suffering from an abscess, and the same course was pursued with equal success. Two important inferences may be drawn from these cases, Dr. Quimby said:—minor surgical operations may be done with perfect safety and much more pleasantly than in the ordinary way; and, secondly, a person somewhat skilled in the use of chloroform may enter a sleeping apartment and administer chloroform with evil intentions while a person is asleep. Hence the use of this drug in the hands of a criminal may become an effective instrument in the accomplishment of his nefarious designs.—*Medical Advance.*

Crustaceans and Light.—In a recent paper communicated to the Paris Academy M. de Merejkowsky describes experiments in which he sought an answer to the question, "Do the lower crustaceans distinguish colours?" His answer is that it is exclusively the quantity, not the quality, of light that affects them. Larvæ of Balanus, a cirrhipede crustacean, were employed, and some of the experiments were repeated on certain marine Copepoda. In a vessel that is quite dark these crustaceans are dispersed in all directions, but if daylight be admitted through a slit they collect about the entering beam. This occurs, too, when light of only one colour is admitted (a coloured liquid being put before the slit). These crustaceans do not seem to be blind for any colour (a result corresponding to what M. Bert observed in the case of Daphnides). But to find out whether they perceive colours as well, the following experiments were made:—Two slits being used for a beam of white and of coloured light respectively, the crustaceans preferred the former beam, all clustering round it if the coloured light was deep red or violet, and a large majority if that was of a brighter tint (yellow, green, or bright red). Comparing a beam of darker colour, as violet, with one more luminous, as yellow, the crustaceans always preferred the latter. With two slits admitting coloured rays of equal brightness, the crustaceans divided into two equal parts, whatever the colouration (the author thus compared bright red with yellow, green, and blue, yellow with green and blue, and green with blue); but directly the brightness was rendered unequal the groups of crustaceans became unequal, too. M. de Merejkowsky concludes, then, that in perception of light there is a great difference between the lower crustaceans and man, and even between them and ants; "while we see different colours and their different intensities, the lower crustaceans see only a single colour in its different variations of intensity. We perceive colours as colours; they only perceive them as light."—*Times.*

Our Mathematical Column.

MATHEMATICAL QUERIES.

[23]—A messenger M starts from A towards B (distance a) at a rate of v miles per hour, but before he arrives at B a shower of rain commences at A and at all places occupying a certain distance z towards, but not reaching beyond, B, and moves at the rate of u miles an hour towards A. If M be caught in this shower, he will be obliged to stop until it is over. He is also to receive for his errand a number of shillings inversely proportional to the time occupied in it, at the rate of n shillings for one hour. Supposing the distance z to be unknown, as also the time at which the shower commenced, but all events to be equally probable, show that the value of M's expectation, in shillings, is

$$\frac{nv}{a}\left\{\frac{1}{2}-\frac{u}{v}+\frac{u(u+v)}{v^2}\log.\frac{u+v}{u}\right\}$$

[Let the distance A be divided into p equal parts, each equal to δ, so that $p\delta=a$; then

$\frac{\delta}{v}$ = time in which M passes over space δ.

$\frac{\delta}{u}$ = time in which shower passes over same space.

Suppose the distance Z successively equal to $\delta, 2\delta, 3\delta \ldots p\delta$, and *in each case* suppose successively that the shower commences after a time $\frac{\delta}{v}, \frac{2\delta}{v}, \frac{3\delta}{v}, \ldots\ldots \frac{p\delta}{v}$ from M's starting, so that there are p^2 cases in all to be considered. Thus M's time is as follows in the following cases:—

. . . .

$z=\delta$; in p cases, $\frac{a}{v}$;

$z=2\delta$; in $(p-1)$ cases, $\frac{a}{v}$; in 1 case, $\frac{a}{v}+\frac{\delta}{u}$

$z=3\delta$; in $(p-2)$ cases, $\frac{a}{v}$; in 1 case, $\frac{a}{v}+\frac{\delta}{u}$; in 1 case, $\frac{a}{v}+\frac{2\delta}{u}$

. . . .

$z=p\delta$; in 1 case, $\frac{a}{v}$; in 1, $\frac{a}{v}+\frac{\delta}{u}$; in 1, $\frac{a}{v}+\frac{2\delta}{u}$; in 1, $\frac{a}{v}+\frac{3\delta}{u}$; &c.

in 1, $\frac{a}{v}+\frac{(p-2)\delta}{u}$; in 1, $=\frac{a}{v}+\frac{(p-1)\delta}{u}$

Thus, the sum of the amounts to be received, according to the conditions, is—

$$n\left\{\frac{p}{\frac{a}{v}}+\frac{p-1}{\frac{a}{v}}+\frac{1}{\frac{a}{v}+\frac{\delta}{u}}+\frac{p-2}{\frac{a}{v}}+\frac{1}{\frac{a}{v}+\frac{\delta}{u}}+\frac{1}{\frac{a}{v}+\frac{2\delta}{u}}+\cdots\right.$$

$$\left.+\frac{1}{\frac{a}{v}}+\frac{1}{\frac{a}{v}+\frac{\delta}{u}}+\frac{1}{\frac{a}{v}+\frac{2\delta}{u}}+\cdots+\frac{1}{\frac{a}{v}+\frac{(p-1)\delta}{u}}\right\}$$

$$=n\left\{\frac{p(p+1)v}{2a}+uv\left[\frac{(p-1)}{au+\delta v}+\frac{(p-2)}{au+2\delta v}+\frac{(p-3)}{au+3\delta v}+\cdots\right.\right.$$

$$\left.\left.+\cdots+\frac{(p-r)}{au+r\delta v}+\cdots+\frac{1}{au+(p-1)\delta v}\right]\right\} \quad (A)$$

Now
$$\frac{p-r}{au+r\delta v}=\frac{1}{\delta}\left[\frac{(p-r)}{pu+rv}\right]=\frac{1}{\delta v}\left[\frac{pv-rv}{pu+rv}\right]$$

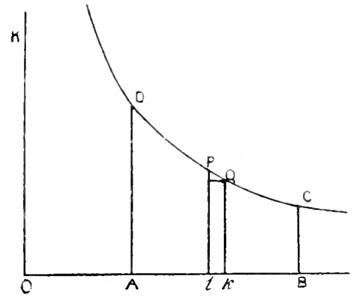
$$=\frac{1}{\delta v}\left[\frac{pv-pu-(pu+rv)}{pu+rv}\right]=\frac{p}{\delta v}\left[\frac{u+v}{pu+rv}-1\right]$$

$$=\frac{p^2}{av}\left[\frac{u+v}{pu+rv}\right]-\frac{p^2}{av} \qquad \left(\text{since } \frac{1}{\delta}=\frac{p}{a}\right)$$

Thus series $(A)=n\left\{\frac{p(p+1)v}{2a}-\frac{p^2u}{va}\right.$

$$\left.+\frac{p^2u(u+v)}{a}\left[\frac{1}{pu+v}+\frac{1}{pu+2v}+\cdots+\frac{1}{pu+(p-1)v}\right]\right\}$$

Call the series withing the square brackets S, then the general term of $\frac{1}{pu+rv}$; or the general term of pS is $\frac{1}{u+\frac{r}{p}v}$, and we have to find the sum of this series when r has all values from 1 to p, p being made infinite; or which comes to the same thing when $\frac{r}{p}$ varies through all values from 0 to unity. Now, supposing we know nothing of the differential calculus, we should, probably, at once see how this was to be done by using the well-known property of the rectangular hyperbola that the rectangle having asymptotes as sides, and a line joining centre and a point on the curve as diagonal, is of constant area. Thus, suppose we take OK, OB as asymptotes of a rectangular hyperbola DPC,



$OA=u$, $AB=v=AD$, and $Ok=u+\frac{r}{p}.v$, then we know that

$$\text{rect. } Ok.Qk=\text{rect } OA.AD=uv$$

$$\text{so that } Qk=\frac{uv}{u+\frac{r}{p}.v}$$

Hence, if we take $lk=\frac{v}{p}$ and complete rectangle Ql, we have

$$\text{rect. } Ql=\frac{v}{p}\cdot\frac{uv}{u+\frac{rv}{p}}=\frac{uv^2}{pu+rv}=uv^2\text{ (general term of } S)$$

When p is made infinite, so that such a rectangle as Ql becomes indefinitely narrow, and the sum of all such rectangles between AD and BC is the area $ADPCB$, we have

$$uv^2(S)=\text{area } ADPCB=OA.AD\log.\frac{OB}{OA}=uv\log.\frac{u+v}{u}$$

$$\text{So that } S=\frac{1}{v}\log.\frac{u+v}{u}$$

Now the value of M's expectation is the total payable on all the possible events, divided by the number of events; or is series (A) divided by p^2 when p is made infinite.

$$=\frac{n}{p^2}\left\{\frac{p^2v+pv}{2a}-\frac{p^2u}{va}+\frac{p^2u(u+v)}{va}\log.\frac{u+v}{u}\right\} \text{ when } p \text{ is infinite.}$$

$$=\frac{nv}{a}\left\{\frac{1}{2}-\frac{u}{v}+\frac{u(u+v)}{v^2}\log.\frac{u+v}{u}\right\}$$

Of course, the solution thus given depends on the principles which underlie the differential and integral calculus. It does not seem worth while to master in each such problem the difficulties which result from avoiding the actual use of the calculus, except in this respect, that before the student begins to use the calculus he should so far accustom himself to deal with such problems as the above, that the real meaning, as well as the real value of the calculus, may be recognised. In dealing with the above problem we should simply get the general term of the series S, writing x for $\frac{rv}{p}$, and since $\frac{v}{p}$, when p is made infinite, is dx, we get

$$S=\frac{1}{v}\int_0^v\frac{dx}{u+x}=\frac{1}{v}\log.\frac{u+v}{u}$$

as by the geometrical method used above. Ed.]

[24]—1. Who introduced the symbol π? 2. What is the origin of the name "Courbe du diable," as applied to the locus $y^4-96a^2y^2+100a^2x^2-x^4=0$? 3. What is the origin of the name, "Witch of Agnesi"?* 4. Where can one find the best discussion of "Fourier's Series"? 5. How may an angle be trisected by means of the cissoid of Diocles?—W. W. Beman.

25.—I borrow £100 from a Building Society, and repay principal and interest (compound) by 120 monthly payments of £1. 3s. 1d. What rate of interest am I paying?

---

* Agnesi says herself, vol. I., p. 381, "Equazione alla curva da descriversi, che dicesi la *Versiera*."

[Let r be rate per pound per month, so that at the end of a month £1 becomes £$(1+r)$. Then at end of first month £$\frac{6}{5}$ is due; at end of second £$\frac{6}{5}+(1+r)$£$\frac{6}{5}$, at end of third £$\frac{6}{5}+(1+r)$£$\frac{6}{5}$ $+(1+r)^2$£$\frac{6}{5}$, and finally at end of twenty months

$$\{1+(1+r)+(1+r)^2+\ldots\ldots\ldots+(1+r)^{19}\}£\frac{6}{5}$$

$$=\frac{(1+r)^{20}-1}{1+r-1}£\frac{6}{5}=\{(1+r)^{20}-1\}£\frac{6}{5r}$$

Now, £100 at rate r per £1 per month, compound interest should be equal in value at end of twenty months to £$\frac{6}{5}$ paid monthly, as above, for twenty months. That is

$$(1+r)^{20}£100=\{(1+r)^{20}-1\}£\frac{6}{5r}$$

$$(1+r)^{20}(6-500r)=6$$

Hence $20 \log.(1+r)+\log.(6-500r)=\log. 6.$

We can find r tentatively from this equation. It is clear r cannot be greater than $6\div500$. —ED.]

[26] What will £10 annually at r per £1 per annum compound interest amount to in twenty years?—J. R.—[We presume this is our correspondent's question. The method applied to previous question gives for this amount at the end of twenty years

$$\{(1+r)^{20}-1\}(£10\div r)\text{—ED.]}$$

[27] The value of a diamond varies as the square of its weight. A diamond falls and breaks into three pieces; what proportion does their probable value bear to that of the original diamond?—P. A. MACMAHON.

[28]—EQUATION.

$$\frac{a+x}{\sqrt{a}+\sqrt{(a+x)}}+\frac{a-x}{\sqrt{a}-\sqrt{a-x}}=\sqrt{a}$$

MATHEMATICUS NOVOCASTRORUM.

[The Equations of "Rueverte" in our last seem to have been made "pie" of. We very carefully corrected the second (the first was rightly given), writing out in margin the left side of the equation afresh, but now the right side has entirely disappeared, and the first equation has gone wrong. It should be, I think,

$$\frac{x^2}{a}+\frac{y^2}{b}=\frac{a^2}{x}+\frac{b^2}{y}=a+b$$

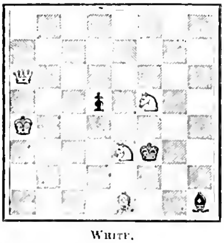
I cannot recollect what was on the right side of the other equation. Will "Rueverte" oblige by repeating it? Readers must not imagine that formulas which appear wrongly have been carelessly written or corrected. When I note that I had to correct several sheets of my book on Cycloids five or six times, and that even then errors appeared, they will see how difficult it is to secure correctness in the printing of mathematical matter.—ED.]

Our Chess Column.

THE following problem gained the first prize in the late Problem Tournament of Design and Work. It is taken from the *Chess Players' Chronicle*.

By B. G. LAWS.

BLACK.



WHITE.

White to play and mate in three moves.

GAME AT ODDS.

First game in a match between Messrs. H. Erskine and W. Mead at the odds of Pawn and two moves.

Remove Black King's Bishop's Pawn

	WHITE. Mr. W. Mead.	BLACK. Mr. H. Erskine.
1.	P. to K.4	
2.	P. to Q.4	P. to Q.3.
3.	B. to Q.3.	B. to K.3. (a)
4.	Kt. to K.B.3.	B. to B.2
5.	Kt. to Kt.5.	Kt. to K.R.3
6.	B. to K.3.	P. to K.4.
7.	P. to Q.B.4. (b)	B. to K.2.
8.	P. to K.R.4	Kt. to Q.2. (c)
9.	Kt. to Q.B.3.	B. to B.3.
10.	P. to Q.5	P. to Q.R.3. (d).
11.	Kt. to K.6. (e).	B. takes Kt.
12.	P. takes B.	Kt. to B.sq.
13.	B. takes Kt.	P. takes B.
14.	Q. to R.5. (ch.)	Kt. to Kt.3
15.	P. to K.Kt.3	P. to B.3.
16.	Castles Q.R.	Q. to K.2.
17.	P. to B.4.	Castles Q.R. (f).
18.	P. to B.5.	Kt. to B.sq.
19.	Q. to K.2.	Kt. takes P. (g).
20.	P. takes Kt.	Q. takes P.
21.	K. to Kt.sq.	K.R. to Kt.sq
22.	Q. to K.3.	K. to Kt.sq.
23.	Q. to Kt.6.	R. to Q.2.
24.	P. to B.5.	B. to Q.sq.
25.	Q. to Kt.4.	P. takes P.
26.	B. to B.4. (h).	Q. to Kt.5. (i)
27.	Q. takes B.P.	R. takes R. (ch.)
28.	R. takes R.	R. to K.sq.
29.	B. takes R.P.	P. takes B.
30.	Q. takes B.P.	Q. to Kt.3.
31.	R. to Q.6.	Q. to B.2.
32.	R. to Q.7.	Q. to B.8. (ch.)
33.	R. to Q.sq.	Q. to B.2.
34.	Q. takes R.P. and White won.	

NOTES BY MEPHISTO.

(a) The intention of bringing the B. to B.2. is good enough, but we hardly think that Black can afford the necessary time for this manœuvre, we should prefer P. to K.3. followed by Q. to K.2., a line of play favoured by Mr. Potter. We, however, freely confess some ignorance as to this most cruel of all openings. A modern treatise on these and other odds would be welcome. In a game between Mr. Wayte and Mr. Hooke, at Leamington, the following occurred:—

P. to K.4.	P. to K.B.4.	Kt. to K.B.3.	P. to K.5.
—	P. to K.3.	P. to Q.4.	P. to Q.B.4.

this line of play resulted unfavourably for the attack.

(b) White's attack relaxes a little, he ought to have *Castled* and then played P. to K.B.4.

(c) Black has emerged fairly from the first attack, and Castling at once would have been his best course. For, considering the odds given, he ought not to dread White's attack on his King's side, with the King in comparative safety.

(d) Sad loss of time.

(e) Well played. Should Black play Q. to B.sq., White would also obtain a strong position.

(f) Black misses a good chance of somewhat equalising matters. He ought to have played 17. P. takes P., followed by B takes Kt. and Castles Q.R.; he would then have either remained with Bishops of opposite colours or with Kt. against a B., and would have had a fair chance of drawing, *e.g.*, 17. P. takes P. 18. P. takes P. (P. to K.5. would not be good play.) B. takes Kt. if 19. P. takes B. / Castles Q.R. or if 19. P. to K.B.5. / Castles Q.R. 20. P. takes Kt. / P. takes P. 21. Q. takes P. / B. to K.4. with a good game.

(g) A desperate course, but there seemed no possibility of extricating the Knight from its helpless position.

(h) White conducts his game with good judgment.

(i) Black could not gain anything by playing his Queen away from his King's side.

"Synopsis of the Chess Openings." By William Cook. Third Edition. (London: W. W. Morgan, 23, Great Queen-street.)

The author of this treatise, who is himself a strong chess player, presents in this book the best forms of play according to latest practice, giving us the benefit of games played as late as 1880. We must give Mr. Cook great credit for his laborious collection of the best openings. What makes this book specially valuable is its practical tabular form. Every Chess player can at a glance see the line of play or variation he wishes to consult.

We extract the following as specimens of style:—

Cook's Synopsis of the Openings.

Table LXXVI.—Allgaier Gambit.

1.	P. to K.4. / P. to K.4.	2.	P. to K.B.4. / P. takes P.	3.	Kt. to K.B.3. / P. to K.Kt.4.
4.	P. to K.R.4. / P. to Kt.5.	5.	Kt. to Kt.5. / P. to K.R.3.	6.	Kt. takes P. / K. takes Kt.

7.	P. to Q.4.(1) / P. to Q.4.		
8.	B. takes P. / P. takes P.(2)		
9.	B. to B.4.(ch.) / K. to Kt.2.		
10.	Castles(3) / Kt. to K.B.3.		or B. to K.5.(ch.) / Kt. to K.B.3.
11.	Q. to Q.2. / Kt. to B.3.		Q. to Q.2. / Kt. to B.3.
12.	Kt. to B.3.(4) / B. to Q.3.(5)	or Q. takes P.(ch.) / Q. takes Q.	Q. to B.4.(4) / B. to Q.3.
13.	Kt. to K.2(6) / B. takes B.	Kt. takes Q. / B. to K.5.	Kt. to B.3. / Q. to K.2.
14.	R. takes B. / Kt. to K.4.	Kt. to B.4. / Kt. takes P.	Kt. to Q.5. / B. takes B +
15.	Q. to B.3. / Kt. takes B.	B. to K.2. / R. to B.4. +	
16.	Q. takes Kt. / R. to B.sq.		
17.	Kt. to K.3. / Q. to Q.3. +		

(1) Mr. Thorold's attack.

(2) If 8. B. to Kt.2. 9. Kt. to B.3. 9. P. takes P. 10. B. to B.4.(ch.) 10. K. to Kt.3. 11. Kt. to Q.5.

(3) If 10. Kt. to B.3. 10. Kt. to K.B.3. 11. Q. to Q.2. 11. B. to Q.3. +.

(4) Mr. Freeborough's variation.

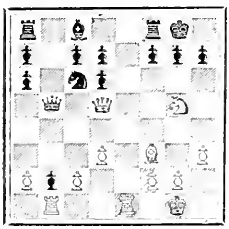
(5) Mr. Potter's defence.

(6) If 13. B. to K.3. 13. Kt. to Q.R.4 +.

Ending of an actual game played by Mephisto, illustrating the dangers to be shunned by weaker players.

AMATEUR.

BLACK.



WHITE.

MEPHISTO.

In this position White played.

R. takes P.	Q. takes R.
Q. takes B.P.(ch.)	R. takes Q.
B. to K.8.(ch.)	R. to B.sq.
B. to Q.5.(ch.)	K. to R.sq.
R. takes R. mate.	

Another specimen of the Chess skill of the late Mr. S. S. Boden *Select Reprint.* Bird's "Masterpieces," 66 (originally in Horwitz and Kling's "Chess Player"; and played soon after 1851). From the *Chess Players' Chronicle*.

(PHILIDOR'S DEFENCE.)

WHITE. Mr. Schulder.	BLACK. Mr S. S. Boden.
1. P. to K.4.	1. P. to K.4.
2. Kt. to K.B.3.	2. P. to Q.3. (a)
3. P. to B.3.	3. P. to K.B.4.
4. B. to B.4. (b).	4. Kt. to K.B.3.
5. P. to Q.4.	5. P. takes K.P.
6. P. takes K.P.	6. P. takes Kt.
7. P. takes Kt.	7. Q. takes P.
8. P. takes P. (c)	8. Kt. to B.3.
9. P. to B.4.	9. B. to Q.2.
10. B. to. K.3.	10. Castles.
11. Kt. to Q.2.	11. R. to K.sq.
12. Q. to B.3.	12. B. to B.4. (d)
13. Castles Q.R.	13. P. to Q.4. (e)
14. B. takes P.	14. Q. takes P.(ch.)
15. P. takes Q.	15. B. to R.6. mate.

Notes.

(a) Mr. Boden was partial to this defence. It was a characteristic of his style that he did not mind a close position to begin with; he was not easily to be hemmed in.

(b) White's opening, though unscientific, is not without ingenuity. He intends of course, in answer to P. takes P., to sacrifice the Kt.

(c) He should have taken with Queen, and played as best he could for a draw.

(d) Not only preventing the advance of the Kt., but also doubtless shrewdly divining that his opponent intends to Castle on Q side, and preparing a "concatenation accordingly."

(e) Winning a piece at least, if White has by this time discovered the danger to his King. This game has been selected as a specimen of Mr. Boden's felicity of combination in his lighter encounters.

*** The notes are by the Rev. W. Wayte.

A. J. MARTIN AND J. P. You are right; 1. K. to K.3. is the key move to Problem 5. We answered from recollection only of the problem, which was sent to us by Mephisto (with, of course, the correct solution). On turning to the position, we see that if Black checks with Bishop, R. covers, disclosing check; and after King has moved B mates, not R. as you suggest. In future, please address Chess Editor. We have not had a moment to open a chess board during the last three weeks, nor spare brain energy to go through games, or study positions without the board. ED.

E. H. J. All your solutions right. ED.

DUNSTABLE.—Correct.—ED.

W. GODDEN.—No. 12 (wrongly called 11) is correctly given.—ED.

A. MACDONNELL.—All incorrect. ED.

Our Whist Column.

BY "FIVE OF CLUBS."

IT may now be convenient to sum up the various leads, in such a form that they can be readily studied at a glance and easily remembered. We would invite those who have tried to retain in their recollection the multitudinous leads given in the books heretofore published, to note how simple the whist leads are when viewed as we have presented them. We venture to say indeed, we *know*, having tested the matter—that a more perfect knowledge of the leads at whist can be gained in a week by considering when to lead Ace, King, Queen, and so forth, than in two months at least by the usual method of considering what card to lead from each of the numerous combinations which the cards may present. Moreover it is found in practice that a learner who has followed our method at once picks up the habit of interpreting the leads of others, whereas one who followed the other method is often a long time in passing from a knowledge of what he should lead to the ready recognition (instant recognition, it should be, after a little practice) of the meaning of any given lead.

SYNOPSIS OF THE LEADS IN PLAIN SUITS.

Lead Ace, from Ace, with four or more others, not including King: from Ace, Queen, Knave, with or without others; from Ace, two others (not including King), if you have reason to believe that you

partner has strength in the suit; and from Ace one other, whatever this other may be. The last two cases are, of course, forced leads.

After leading Ace, from Ace four or more, follow * with lowest, (unless you adopt Drayson's plan of playing lowest but one if there are more than four others). After leading Ace from Ace, Queen, Knave, follow with Queen if you have not more than one small one of the suit, otherwise follow with Knave. When you lead Ace from Ace two others (forced lead) follow with highest. Lead Ace from Ace, King, and others, when you have trumped another suit, lest your partner should trump your King, to establish a cross ruff.

Lead King, from Ace, King, and others; from King, Queen, and others (unless these others, being more than two, include the Knave); from King two others (forced lead), if you have reason to believe that your partner has strength in the suit; and from King one other (forced lead), whatever that other may be.

After leading King from Ace, King, and others, follow with Ace, unless you hold Knave, in which case you may sometimes—if the state of the score seems to render it advisable—change suit, that you may be led up to and finesse the Knave. After leading King from King, Queen, and others, if King makes, follow with small one, unless you hold Knave also, when follow with Queen (not with small one, because Ace may have been held up). When you lead King from King two others (forced lead) follow with highest.

Lead Queen from Queen, Knave, Ten, with or without others; from Queen, Knave, and one small one (forced lead); from Queen two others, not including Knave (forced lead), if you have reason to believe that your partner has strength in the suit; and from Queen one another (forced lead) whatever that other may be.

After leading Queen from Queen, Knave, Ten, follow with Knave, unless you have five or more, when follow with lowest of the Queen, Knave, Ten sequence. After forced lead from Queen two others, if Queen makes, follow with highest.

Lead Knave from King, Queen, Knave, and not less than two others† (not including ten); from Knave, ten, nine, with or without others; from Knave and two others (forced lead), and from Knave one other (forced lead).

After leading Knave from King, Queen, Knave, &c., follow with King if you have two small ones, with Queen if you have more. After leading Knave from Knave, ten, nine, lead ten if there is only one card below the nine, the nine if there are more. After leading Knave from Knave two others, wkatever they may be, follow with highest.

Lead Ten from King, Queen, Knave, ten, with or without others; from King, Knave, ten, with or without others; from ten two others, or ten one other (forced leads). After leading ten from King, Queen, Knave, ten, follow with King if you have no small cards, otherwise with Knave. After ten from King, Knave ten, play a small one. After forced lead of ten, play your highest.

Lead nine from King, Knave, ten, nine; and in case of forced lead, from nine two others.

Lead a small card from all suits not considered in the above synopsis. Lead the lowest from four cards, the lowest but one from five or more (the lowest but two from six or more, if you care to adopt Drayson's rule); the highest from three or two small cards.

Note that it can scarcely ever happen that playing the lowest but one or two for the purpose of indicating length, can be mistaken by your partner for a forced lead from two or three small cards, or *vice versâ*.

We have already considered concisely, yet fully, the distinction between trump leads and leads from plain suits (see No. 12).

Observe that, short as the above synopsis seems, considering the multiplicity of Whist leads as usually presented, it would be very much shorter if it dealt only with original leads. For these one may say that all the beginner need learn is summed up in the following:—

Lead Ace from Ace and four others, following with small one; and from Ace, Queen, Knave, with or without others, following with Queen, if you have not more than one small one, otherwise with Knave. Lead King from Ace, King and others following with Ace; and from King, Queen and others, following with small one. Lead Queen from Queen, Knave, ten, following with Knave, unless you have five or more, when play lowest of head sequence. Lead Knave from King, Queen, Knave, and two or more, from Knave, ten, nine, with or without small ones. Lead ten from King, Queen, Knave, ten, and from King, Knave, ten, with or without others. Lead nine from King, Knave, ten, nine. In other cases lead a low one, the lowest if you have only four cards, the lowest but one if you have more.

Let the learner combine with this the general rule, that if he is obliged to lead from a weak suit, he always plays the best card of it, unless he has either Ace, King, or Queen, with two small ones. He now knows nearly all that he need know about leading from plain suits. All that he need at first notice about leading from trumps, is, that he can more safely play a waiting game in that suit, as his good cards in it cannot be lost by trumping; also that he must consider the trump card. The play in trumps is also apt to be modified by considerations depending on the state of the score, the position of the cards in other hands, and so forth.

* When we thus speak of second round, we do not wish the reader to forget that the first round may show it to be unadvisable to continue the suit; it may seem better to leave your own suit and lead your partner's, or to lead trumps, &c.

† In our last we inadvertently wrote "with or without others," instead of "not less than two others."

Whist Problem.—For the study of advanced Whist players we give the following from the "Westminster Papers":—

Z turns up Spade 7. A leads.

B's hand.
- *Spades.*—Ten, nine, six, five.
- Hearts.—Ace, Queen, four, two.
- Diamonds.—Queen, six.
- Clubs.—Ace, ten, eight.

First four rounds:—

A	*Y*	*B*	*Z*
C 6	C Kn	C Ace	C 3
H 9	H 5	H 2	H 10
D 8	D Kn	D Q	D 4
S Kn	S A	S 5	S 7

After these four tricks, B can place every card left in the players' hands, if they all play according to the rules usually followed. Show how he can do this.

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PLAINLY WORDED—EXACTLY DESCRIBED

LONDON: FRIDAY, FEBRUARY 10, 1882.

CONTENTS OF No. 15.

THE EYE AND THE MICROSCOPE.

BY HENRY J. SLACK, F.G.S., F.R.M.S.

THERE is a notion prevalent that using the microscope injures the eyesight, but this is really not the case; it is only the abuse of the instrument that has such an effect. It does not hurt the eye to look at anything which is plain and easy to see, and neither in too strong nor too feeble a light. The art, then, of using the microscope consists in exhibiting small objects so that they shall be seen as larger ones are without any instrumental aid. To do this the microscope must be a good one, and a few simple rules followed until practical skill is gained. Whether the microscope is a binocular or not, *both* eyes must be kept open. This is of absolute necessity if the sight is not to be damaged. Many persons find it difficult to look down a tube with one eye without shutting the other. A remedy for this, and one useful in other respects with a monocular instrument, is to cover a piece of cardboard nine inches long and five wide, with black cotton velvet, cut a hole in the middle, and insert the eye-piece through it into the tube of the microscope. With this screen in front, everybody finds it easy to keep *both* eyes open, and look at the object with either one, if they are both alike, or with the best if they differ. Persons whose eyes agree in focus, and otherwise correspond, derive the greatest benefit from binocular instruments, but the use of one eye does not fatigue it if the object is properly focussed and suitably illuminated. A common fault with beginners is to use too high power, with which it is impossible to see the object they want. For many most interesting sights of live objects a four-inch objective is extremely useful, and the highest power a beginner is likely to employ with advantage is a one-inch, with a couple of eye-pieces, the highest giving a linear magnification of 50 or 60, with the English length of tube. The proper use of this power should be mastered before more magnification is attempted.

It requires considerable practice to pay attention only to one part of an object that may be shown plainly, and take no notice of other parts that from any reason are not clear. With low powers, an object need not be quite flat for the whole to be fairly in focus at the same time: with higher powers, great flatness is indispensable, and an extremely slight irregularity only permits one portion at a time to be clearly seen. This state of things is very uncomfortable to a beginner, and the eye is sure to suffer from it. The size of an object that can be viewed as a whole with any power depends upon several conditions, which need not now be discussed, but the higher the magnification the less it is, and with an enlargement exceeding 200 linear it becomes exceedingly minute.

As soon as the student has attained to some dexterity in the use of the instrument, he should acquire the habit of paying exclusive attention to what he can see plainly, and take no notice whatever of things out of focus, or from any other cause not even focally visible. With some exceptions, it is most agreeable to the eye that an object should not occupy the whole field, but have a fair margin round it, which should not be over-flooded with light. Eyes differ very much in sensitiveness to light, and when, as in noticing the actions of live objects, prolonged attention is required, the intensity of the illumination should be nicely regulated to suit the individual case. Light passing through a piece of foreign post paper, saturated with spermaceti, is often the most pleasant.

By attention to such directions as have been mentioned, there need be no fear of devoting a considerable time every day to microscopic investigation, and many observers could be mentioned who have done this for years without any detriment to their visual powers. On the other hand, those who torment their eyes in attempts to see the most difficult diatom markings, or the closest of Nobert's ruled lines, suffer from their folly, without any compensation in the shape of useful knowledge.

ABOUT FALLACIES.

BY THE EDITOR.

SEVERAL correspondents write about the question of luck as we considered it in No. 11, some asking whether the evidence does not show that some men really are exceptionally lucky (so that their luck in matters of pure chance may be relied on); others asking whether, if a coin had been tossed a great number of times with the same result (head or tail) in one set of trials, it would not be more likely to show the other side (tail or head) oftener in the next set of trials; while yet others consider that the ideas of men of science about fallacies generally are erroneous—that, in fact, the so-called fallacies are real truths.

Taking the last first, I may note that the rule of science in all those cases in which specific results are popularly supposed to follow from specified actions, or the like, is simply to inquire whether there can possibly be any relation of cause and effect in such cases. When a housemaid says, for instance, that putting the poker across a fire makes the fire burn up, the student of physical laws is able at once to see that the supposed influence is antecedently most improbable. Here in a grate are certain more or less combustible materials, and certain quantities of matter already burning; combustion is going on, though indifferently; the air is nourishing this slowly burning fire, but inefficiently; on the whole, it seems likely that the fire will go out. In what way shall I do any good if I stick a rod of iron from the fender across the top bar? I thus add a certain quantity of cold metal to the space across which the air has to come to the fire. Do I increase the draught? On

the contrary, so far as I produce any effect at all on the draught, I must diminish it. For the draught depends in the main on the diminished density of the warmed air in the neighbourhood of the fire, and the cold metal must to some degree increase the density of this air by cooling it. The effect may be very slight, but, such as it is, it is unfavourable. But here is a correspondent who tells me that whether theoretically the poker should make the fire burn up or not, as a matter of fact it does. Repeatedly he has tried the experiment, and after exhausting in vain every art he possessed to make the fire burn up, he found the poker put across the top bar, immediately, or almost immediately, produce the desired result. Science is bound to listen to evidence of this kind, for science deals with phenomena, and even when phenomena seem to point to something which appears utterly incredible, science has to inquire into the matter. Well, in this case, what are the facts? Someone tells us that he has repeatedly tried in vain to make a fire burn up, but when he put the poker across it, the fire presently became clear and bright. Multitudes of contrary cases might no doubt be cited, but let us suppose that none could. Are we therefore to infer that in these cases the poker drew the fire up? A new law of nature would be indicated, if this were so; and a new law of nature is worth learning. But when due inquiry is made, it appears that there is no such law—as, unfortunately, we might have expected. Our correspondent, who found that when he put the poker across the fire it drew up, is unquestionably but an unskilful fireman. He puts on coals, and pokes and stirs the fire, unconscious of the fact that this is just the way to put a fire out. When the fire is all but hopelessly reduced by his unskilful measures, he puts the poker across the top bar. According to old fashioned superstitions, he makes the sign of the cross across the fire place, and the fire, in which until now there seemed to have been some evil spirit (that is what people mean when they say "the devil's in the fire"), is purified from the unclean presence and begins to burn up. That would have been the old-fashioned interpretation of the change; unfortunately, science takes another view of the matter. It sees reason to believe that the change took place simply because the disturbance to which the fire had before been exposed was bad for it. Putting the poker across the top bar meant letting the fire alone, and giving it a chance to burn up.

Singularly enough, I had occasion, when the last sentence was just finished, to leave my study. When I came back, an hour later, I found that my fire, which in the meantime must very nearly have gone out, had been recoaled—and the housemaid, or whoever had attended to it, had, after the fashion of her tribe, put the poker across the top bar. The fire was not burning very brightly—on the contrary, it seemed inclined to go out. Yet, rashly daring, I put the poker down—from scientific principles I object to seeing bright metal smoked and dulled—and went on with my work, intending, if the fire went out, to call someone in to light it again. However, it so chanced that after the poker was put down, the fire began to burn pretty brightly, and as I write there is every promise of a good fire. Am I to infer that taking the poker from across the top bar made the fire burn up? Of course, the real fact was, that when the fire seemed dull it was really making steady progress, and whether I had taken down the poker, or supplemented its salutary action by putting another poker across the top bar, would have made not one particle of difference.

That our domestic servants should consider the poker across the top bar a specific for making a dull fire burn up is very natural. Their manner of treating fires is unscientific in the extreme. A Cambridge Fellow, who knew very little about the fair sex, except what he might gather from the ways of "bed makers" and his recollections, perhaps, of domestic servants at home, used to define woman as "an inferior animal, not understanding logic, and poking a fire from the top." Most servants do this. They also have two utterly erroneous ideas about making up a low fire—first, that the more fuel is put on the better; secondly, that after putting coal on it is desirable to stir the fire. As a matter of fact, when a fire is low, the addition of fuel will often put it out altogether, and the addition of much fuel is almost certain to do so; and in every case the time to stir the fire (when low) is before coals are put on, not after. Generally it is well, when a fire is low, to stir it deftly, so as to bring together the well-burning parts, and then to wait a little, till they begin to glow more brightly; then a few coals may be put on, and after awhile the fire may again be stirred and some more coals put on it. When a low fire has been unwisely treated by being coaled too freely, and the fresh fuel uselessly stirred, it is generally the case that the only chance for the fire is leaving it alone. Susan does this when she puts the poker across the top bar, and unconsciously she retains the old superstition that by thus making the sign of the cross over the fire, she sends away the evil beings, sprites, or whatever they may have been, which were extinguishing it.

That letting the sun shine on a fire puts it out is not, like the other (in its real origin, at any rate), a superstition, but simply an illusion. A correspondent writes that it is believed in by nine persons out of ten; but in this it is like all other wrong beliefs. Scientific methods of inquiry and reasoning are followed by fewer than ten in a hundred; and although now-a-days the views of science are accepted more widely than in olden times, this is simply because science has shown its power by material conquests.

I do not think that my friend Professor Tomlinson's experiments on the burning of candles in sunlight and in the dark would be regarded by all as decisively showing that sunlight does not interfere with combustion, though, rightly apprehended, they go near to prove this. But *à priori* considerations show conclusively that though by warming the air around a fire the sun's rays may, in some slight degree (after a considerable time), affect the progress of combustion, they cannot possibly put the fire out in the sense in which they are commonly supposed to do so; in fact, a fire would probably burn somewhat longer in a room well warmed by a summer sun than in a room from which the solar rays were excluded. (The difference would be very slight.)

NIGHTS WITH A THREE-INCH TELESCOPE.

By "A Fellow of the Royal Astronomical Society."

TO-NIGHT we will avail ourselves of the Zodiacal Map, on p. 225, and examine some of the objects it contains which have not yet been described in these papers. Before commencing our stellar work, though, we will have a look at Mars, now travelling quickly away from us. He is apparently becoming rapidly smaller, as he is receding from the earth, and a good deal of the detail which would have been visible in the instrument we are employing at the end of last December, has now become imperceptible. With a power of 150 or upwards, though, the planet at moments of the best definition will be seen as in Fig. 17.

What is technically called the gibbous appearance of Mars will at once strike the observer's eye. In other

words, the outline of the planet will be seen to differ sensibly from that of a circle, a portion of such outline or "limb" being seemingly cut off by a curved line on the side opposite the Sun. This gives a somewhat hump-backed effect. (Lat. *Gibbus*, humped.) Hence the name.

Fig. 17.—Mars.

While in this region of the heavens, we may notice one or two objects in Taurus, which we omitted on p. 221, in the absence of means for their identification. They will be found in the Zodiacal Map, which we are now employing. The first is Piazzi V., 20 Tauri, which will be noticed in the second square to the left of, and a little higher than Aldebaran; it is marked 20^v. This will prove a very severe test of the excellence of the observer's instrument, and will require a fine night and the highest power at his disposal to be made out properly. 118 Tauri, again, is a beautiful small pair; it lies on the parallel of declination below β on the map. In noticing the nebula to the N.W. of ζ Tauri, we omitted to add that ζ itself is situated in a rather pretty and curious field.

Above Taurus lies the constellation Auriga, to the examination of which we proceed to devote ourselves. We will begin with 14, marked T (for triple) in the map; but we shall only be able to see it as a double star, the components being of a yellowish tint, and about half as far again apart as those of γ Arietis. A very pretty pair will be found in ω Aurigæ. This does not appear by name on the map, but is so close to the star at the top marked 4^v as to incline us to believe that they are intended for the same object. It is represented in Fig. 18.

Fig. 18.—ω Aurigæ.

θ Aurigæ, as a close and very unequal pair, will tax both the instrument and the eyesight of the observer to the uttermost to see it properly. When best seen it will appear as in Fig. 19.

Fig. 19.—θ Aurigæ.

5. Aurigæ (to the north of ω, or 4, just out of the map, on p. 225), is another star in which the diversity of size of the components and their proximity render its observation decidedly difficult. The student will see both these objects better with a high power than with a lower one. 26 (N.E. of β Tauri in the map) is a pretty star, from the contrasted colours of its components, and is very easy from their distance. The comparison is almost horizontally to the left of the larger star. Σ 772 is an equally easy pair. It will be found just to the left of the solstitial colure in the map. 225 P. v. Aurigæ, to the N.E. of 26, must be found by fishing, as it is invisible to the naked eye. When in the field of the telescope, however, it will be found to be a close and extremely pretty little pair.

We may conclude to-night's work by a glance at two or three of the most striking clusters of stars in the constellation under review. And first, M. 38 (north of ϕ Aurigæ) forms a beautiful field, the main cluster assuming a cruciform aspect. The telescope may be moved about in this neighbourhood, which is a rich one. M. 36 (nearly due E. of ϕ) is also very fine. M. 37 (N. of the double star 225, previously described) is a glorious field, even with such an instrument as that which we are employing. In regarding a nebula or cluster, no light should be suffered to enter the eye for some little time before it is applied to the telescope; and the observer should gaze steadily at such an object until the eye becomes accustomed to it, after which hitherto imperceptible detail will flash up. Another rich field will be found in H VII. 33 (marked 33 at the very top of the map).

Next week we will devote to Gemini and the constellations south of it.

Erratum.—Page 221, col. 2, line 22, ξ Tauri should be ζ Tauri.

THE ELECTRIC TELEGRAPH.

By W. Lynd.

The Battery.

BEFORE describing the instruments used by the Government and the Telegraph Companies for the transmission of news and private telegrams, a brief account of the generators of the electric current is necessary.

The simplest form of apparatus for producing electricity by chemical action is called the Voltaic cell, named in honour of the inventor, Professor Volta, a celebrated Italian philosopher. The cell is easily made, and the cost of the materials is trifling. Two strips of metal—one zinc and the other copper—a glass cup, nearly filled with water, to which a little dilute sulphuric acid is added, two bits of copper-wire, and the apparatus is complete. The wires must be soldered to the metallic plates. When the metals are immersed in the liquid and the extremities of the wires placed in contact, chemical action begins. The currents of electricity are produced at the expense of the zinc and the acid. The zinc is consumed and the water is decomposed. The zinc has a very strong affinity for oxygen. The chemical action going on in the cell when the metals are in contact is rendered visible to a certain degree. The liberated hydrogen may be seen collecting in bubbles on the copper plate. The zinc is acted upon by the acid. The oxygen, liberated upon the decomposition of the water, combines with the zinc, forming an oxide of zinc. The copper is not acted upon.

"The materials of an ordinary voltaic cell," Professor Thompson says, "may be regarded as the fuels of electric currents, just as coke and coal are the fuels of steam power. Like those fuels, they represent a store of energy. In the voltaic cell, the flow of electricity continues so long as the wires are joined and the direction of the current is from the zinc plate through the acidulated water to the copper, and from the copper through the wire back to the zinc. A single cell such as I have here described would

be of little or no use for telegraphic purposes, but a series of cells would produce a current sufficiently strong for the working of a needle or Morse instrument. The first battery that came under my notice was constructed as follows:—In a gutta-percha trough about 2½ ft. long, divided into compartments, were placed alternate plates of zinc and copper. The cells were filled with fine sand, over which dilute sulphuric acid was poured. This form was known as the Cruikshank or sand battery; it is now entirely superseded, for it was not constant, and required "refreshing" frequently, or the signals became so weak that it was a very difficult matter to read off a message on the double or single-needle instruments. The form of battery now adopted is called the two-fluid battery, and consists of a wooden trough, lined inside with a resinous composition, which prevents the action of the acid upon the wood. The trough is divided into ten or twelve water-tight compartments insulated from each other. In these cells stand porous earthenware pots, containing a solution of sulphate of copper (blue-stone), and surrounded by a semi-saturated solution of sulphate of zinc. Plates of zinc and copper are connected together by a band of copper, rivetted to each, and bent to allow the copper plate to be in one division and the zinc in the next. The coppers are immersed in the blue-stone solution with which the porous cells are charged, the zincs in the sulphate of zinc. The last copper plate is called the positive pole of the battery, and the terminal zinc the negative pole. When this battery is in action, the copper of the solution is precipitated on the copper plate. If all the solution were consumed, hydrogen would be deposited on the copper, and the current would lose its constancy. To maintain the solution in a saturated condition, and to prevent the accumulation of gas bubbles, crystals of sulphate of copper are placed in the porous cells. The zinc plate is consumed and the copper increased by the precipitation of the metal held in solution. The sulphuric acid produced by the decomposition of the sulphate permeates the porous cylinder, and tends to replace the acid used up by its action on the zinc. The quantity of sulphuric acid decomposed in the solution of copper is regular; the action of the acid on the zinc is regular also, and thus a constant flow of electricity is produced. Professor Thompson represents the chemical action as taking place in two stages

Z_n	+	H_2SO_4	=	Z_nSO_4	+	H_2
Zinc	and	Sulphuric acid	produce	Sulphate of zinc	and	Hydrogen.

and then—

H_2	+	C_uSO_4	=	H_2SO_4	+	C_u
Hydrogen	and	Sulphate of Copper	produce	Sulphuric acid	and	Copper.

It will be seen that the zinc plate is destroyed and converted into a sulphate, and the copper plate actually gains by the action of the voltaic circuit. In the old sand battery, the current was enfeebled by the accumulation of hydrogen at the copper plate, and the zinc was consumed. In the battery I have just described, which is a modification of the cell contrived by the late Professor Daniell, the hydrogen bubbles are avoided and the current is constant, but the consumption of zinc goes on just the same. Now, the student will be able to comprehend one of the greatest discoveries of modern times—that the voltaic cell is *reversible*. "To every action there is an equal and contrary reaction." The application of Newton's words is not limited to mechanical reaction—it reaches down into other departments of science. In the science of electricity and magnetism, the same fundamental principle holds good.

Dr. Sylvanus Thompson, to whom we are indebted for the very best definition of the action of the voltaic battery, says:—"To separate an atom of zinc from one of oxygen requires energy to be expended. When thus separated, they have the chance of doing work in *re-uniting*, this work generally appearing in the form of heat. When a piece of coal is burned—that is to say, is permitted to unite chemically with oxygen—its store of energy runs down and manifests itself in the evolution of heat. A piece of coal represents a store of energy; so does a bag of hydrogen gas; so does a piece of zinc, for zinc can burn directly and give out heat, or may burn indirectly by being dissolved in sulphuric acid, also giving out heat. A Daniell's battery represents a store of energy. A pinch of gunpowder also represents a store of energy. The amount differs, it is true, and the rate at which some of these stores can be made available for use also differs widely in the different cases. An ounce of coal represents an amount of energy which, if entirely expended in doing work, would raise 695,000 pounds one foot high against the force of gravity, or would do 695,000 foot-pounds of work. In an ounce of gunpowder is stored about 10,000 foot-pounds of energy. An ounce of zinc represents a store of only 113,000 foot-pounds. An ounce of copper represents a store of about 69,000 foot pounds only. An ounce of hydrogen gas will yield, by combining with oxygen, 2,925,000 foot-pounds of work. Joule first showed us how to make use of facts like these in calculating by its mechanical value the electric power of voltaic cells. Let us apply these considerations to the storage of energy in any ordinary voltaic cell—say, for example, the Daniell's cell used in telegraphy. In this cell we have certain liquids containing zinc and copper chemically dissolved in sulphuric acid, and into these liquids dip a plate of zinc and a plate of copper. The zinc plate slowly dissolves away, and, at the same time, metallic copper is gradually separated out of the solution, there being about 1 1-20 oz. of zinc consumed for every ounce of copper deposited. Now, to separate an ounce of copper from its solution in sulphuric acid, requires 69,000 foot-pounds of energy to be spent upon it, and as 1 1-20 oz. of zinc represents a storage of 118,650 foot-pounds, the consumption of this weight of zinc is enough to provide the 69,000 foot-pounds needed to separate the copper and to leave a surplus of 49,650 foot-pounds. It is this surplus which goes to maintain electric currents in the circuit and do electric work. But, as we have remarked, the voltaic cell is reversible. If we could take such a cell and by means of some superior electromotive force drive electric currents back through the cell, the whole action will be reversed. Copper will be dissolved, and zinc will be deposited. The copper in dissolving will help the process by giving part of the necessary energy, and our currents will thus once more give us back pure zinc, and so separating out the zinc, we do work and actually store energy." To sum up, a telegraphic battery is a box divided into compartments containing plates of zinc and copper alternately arranged, and immersed in solutions of sulphate of copper and sulphate of zinc. The wires at the terminal plates are called electrodes. When the wires are joined together, the battery is said to be "in circuit." How the electric current is transmitted to distant places, and the instruments employed for the recording of signals will be explained in a subsequent article.

PRESERVING FRUIT FOR THE WINTER.—Dry sand of all substances is found, from the experiment of P. Saraner, to be the best in which to preserve fruit for the winter. The germs of mould attack the rough portion of fruit packed in paper, with great avidity, through the openings in the silk paper. If packed in straw, the least dampness of the straw imparts a musty flavour to the fruit. Sand has another advantage, which is that the damaged specimens do not infect their neighbours. Choose the most perfect fruit with the waxy covering perfect. Leaving the stalks on makes the fruit shrivel up quickly.—F.C.S.

THE GREAT PYRAMID.

By the Editor.

BEFORE considering the characteristics of the Great Gallery in detail, I must note one peculiarity which seems to me very significant.

Regarded as a sort of architectural transit instrument, the Great Gallery would, of course, have to be carried up to a certain height, and there open out on the level to which the Pyramid had then attained, the sides and top being carried up until the southernmost end of the Gallery was completed with a vertical section like that shown in Fig. 2 (further on). This would be the "object end" of the great observing tube. The observer might be anywhere along the tube, according to the position of the object whose transit was to be observed.

Now notice that the most important object of transit observations is to determine the time at which the objects observed cross the meridian. Either the observer has to determine at what time this happens, or, by noting when it happens, to ascertain the time: in one case, knowing the time, he learns the position of the celestial object in what is called right ascension (which may be called its position measured around the celestial sphere in the direction of its rotation) in the other, knowing the position of the object in right ascension, he learns the time. But whether the observer is doing one or the other of these things, he must have a time indicator of some sort. Our modern astronomer has his clock, beating seconds with emphatic thuds, and he notes the particular thuds at or near which the star crosses the so-called wires in the field of view (really magnified spider lines). We may be tolerably certain that the observer in the Grand Gallery had no such horological instrument. But he *must* have had a time indicator of some sort (and a good one, we may notice in passing), or the care shown in the construction of the Gallery would have been in great part wasted.

Now, whence could his time sounds have been conveyed to him but from the upper end of the Gallery? A time-measure of some sort—probably a clepsydra, or water clock—must have been set there, and persons appointed to mark the passage of time in some way, and to note also the instants when the observer or observers in the Great Gallery signalled the beginning or end of transit across the Gallery's field of view. These time indicating persons, with their instruments, would have occupied the space where now are the floors of the so-called Antechamber and King's Chamber—then, of course, not walled in (or the walls would have obstructed the view along the Gallery). These persons themselves would not obstruct the view,

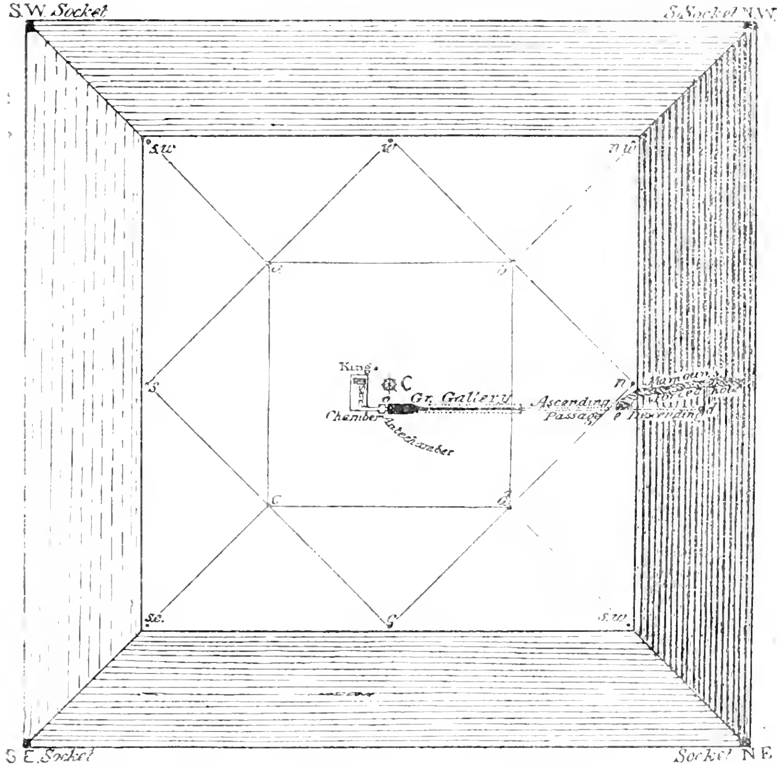

FIG. 1.—SHOWING THE FLAT SQUARE SUMMIT OF THE PYRAMIDAL OBSERVATORY. [illegible] page.

unless they came too near the mouth of the Gallery. Or they might be close to the mouth of the Gallery at its sides, without obstructing the view.

But now, notice that if the place they thus occupied (the future King's Chamber (perhaps, as the region in or near which all the observations of the heavenly host in culmination had been made)) were in the centre of the square top of the Pyramid as thus far built, they would be very much in the way of other observers, who ought to be stationed at certain special points on this horizontal top, to observe certain important horizontal lines, viz., the lines directed to the cardinal points and to points mid way between these. An observer who had this task assigned him should occupy the very centre of the square top of the, as yet, incomplete Pyramid, so that the middle point of each side would mark a cardinal point, while the angles of the square would mark the mid-cardinal points. Also this central point ought not only to command direction lines to the angles and bisections of the sides, but to be commanded, without obstruction, by direction lines from these points.

Thus the upper end of the Great Ascending Gallery should not be exactly at the centre, but somewhat either to the west or to the east of the centre of the great square summit of the incomplete Pyramid.

Let us see how this matter was actually arranged:

Fig. 1 shows the incomplete Pyramid, as supposed to be viewed from above. The four sockets, *s.w.*, *n.w.*, *n.e.*, and *s.e.*, were supposed, until quite recently, to mark the exact position of the four base angles of the Pyramid. It turns out, however, that they are rather below the level of the real basal plane of the structure, which is, therefore, somewhat smaller than had been supposed.

Fig. 1 is, however, chiefly intended to show the nature of the square platform, which formed the top of the pyramidal frustum when the level of the floor of the gallery of the King's Chamber had just been reached. We have a horizontal section of the Pyramid, in fact, taken through the floor of the King's Chamber and Antechamber—that is, through SD, in the figure at p. 266. The bottle-shaped black space, near *O*, gives the section of the slanting gallery, beginning on the southern side at its widest part, reaching a narrower part somewhat to the north of *O*, and thereafter narrowing towards the north, till the section of the uppermost or narrowest part is reached. The dotted lines show where the Grand Gallery and the narrow ascending passage (ascending for one passing towards the King's Chamber) pass downwards into the structure of the Pyramid: at e is the place where descending and ascending passages meet. The position, also, of the entrance-hole, forced in by Al Mamoun, at about the level of the angle e, is indicated.

At *O* is the centre of the square surface, which then formed the top of the structure. If posts were placed at the angles *n.w.*, *s.w.*, *s.e.*, *s.w.*, and also at *n.*, *e.*, *s.*, and *w.*, an observer stationed at *O* would have the cardinal and the mid cardinal points exactly indicated. Now the point *O* is about eight and a half paces from the middle of the southern opening of the Grand Gallery; so that, if there were an assistant observer at *o*, he could communicate time signals readily both to the observers in the Gallery and to the observer at *O*. All such observations as the easting, southing, westing, and northing of heavenly bodies would belong to the observer at *O*, uprights of suitable height being erected at *n.*, *e.*, *s.*, and *w.* He could also observe when heavenly bodies passed the mid-cardinal directions, *n.e.*, *s.w.*, *s.e.*, and *s.w.* It will be noticed that if we suppose the Grand Gallery completed, which would carry it to a height of about 28 ft. above the level of the floor at *o*, the slant of the Gallery would yet be such that the observer at *O*, supposing him to observe by means of an instrument raised a few feet above the level of the floor, would be perfectly well able to look along the horizontal direction line from *O* to *w.* (Most of his observations would, of course, be directed to points above the horizon.)

But I think if I were planning such observations on the square surface *n.e.*, *s.e.*, *s.w.*, *n.w.*, I should wish to have several observers at work in thus taking azimuths (directions referred to the cardinal points) and altitudes, just as several transit observers were manifestly provided for in the construction of the Grand Gallery.

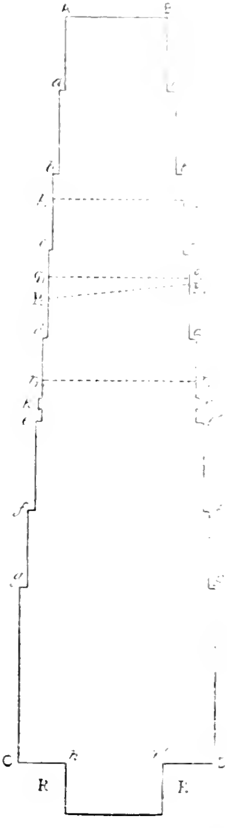

Fig. 2.—Vertical Section of Grand Gallery.

I should set an observer at *n.*, to observe in directions *n.-n.w.*, *n.-w.*, *n.-s.* (that is, *n.O.*), *n.-e.*, and *n.-s.w.*; another at *w.*, another at *e.*, and another at *s.*, to observe in the corresponding directions belonging to their stations. Observers at *n.w.*, *s.w.*, *s.e.*, and *s.e.* could also do excellent work. In fact, they, between them, could take the horizontal cardinal and mid cardinal directions better than the observer stationed at *O*, though his would be the best station for general work with the astrolabe.

Yet again, for observing heavenly bodies at considerable altitudes, stations nearer to the uprights at *s.w.*, *w.*, *n.w.*,

&c., would be useful. Where else could they be so well placed as at the points *a*, *b*, *c*, *d*, where the lines *w.s*, *w.n.*, *e.s.*, and *e.n.* intersect the diagonals of the square surface of the pyramidal structure? Note, also, that these observing stations would be at convenient distances from each other. The sides of this square surface would be roughly about 175 paces long, so that such a distance as

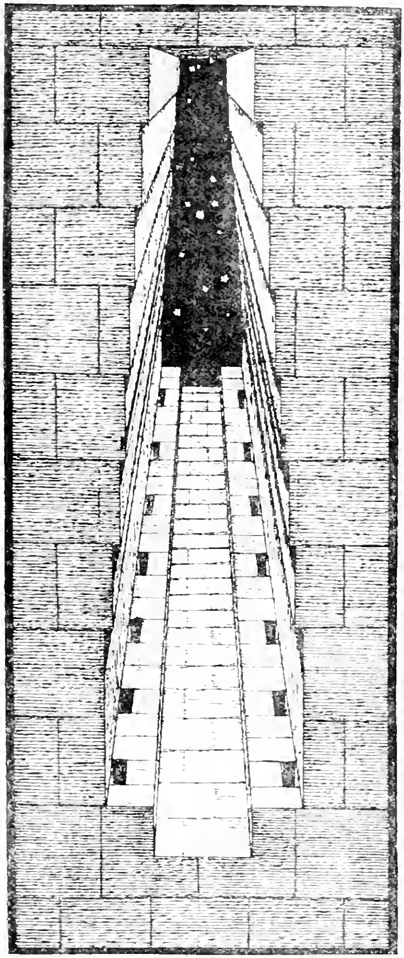

Fig. 3.—A Perspective View of the Upper Fourth of the Great Gallery.

a.w., or *a.O* would only be about 62 yards (the length of the Grand Gallery being about 52 yards).

Thus there would be thirteen observers of azimuthal directions and altitudes, whose work would be combined with that of at least seven transit observers along different parts of the length of the Great Gallery with its seven transit widths (as shown by its section, Fig. 2). Twenty observers in all, the transit workers, provided with the great fixed transit instruments in the Gallery itself, the others armed, perhaps, with astrolabes, armillary spheres for reference, direction tubes (or rings carrying rods) would be able to make observations only inferior in accuracy to those made in our own time with telescopic adjuncts.

Fig. 3 is intended to show something of the structure of the interior of the Great Gallery. The stones outside are supposed to be seen in section, only one fourth of the Gallery being given. For correct perspective, six or seven more layers of stone should have been shown below the lowest in the picture. But this would have given to the illustration an inconvenient shape. It will be seen that a section of the southern sky, very convenient for observation, would be seen from the interior of the Grand Gallery. The central vertical through this section would (as seen from the middle of any of the cross seats) be the true meridian. But the moment of transit might be equally well observed by taking the moments when a star was first seen (from the middle of a cross seat) on the eastern edge of the vertical sky space, and when the star disappeared: the instant midway between these would be the true time of transit. By combining the observations made by several "watchmen of the night," stationed in different parts of the Grand Gallery, a very close approximation to true sidereal time could be obtained.

I apprehend, however, that astronomers who had shown themselves so ingenious in other respects, would not have omitted to note the advantage of suitably-adjusted screens for special transit observations; and it seems to me likely that the long grooves shown in section at *k* and *k'*, Fig. 2, might have been used in connection with such a purpose, and not *merely* (though that was probably one of the objects they were intended to subserve) to carry a horizontal sliding cross-bar, by means of which the altitude of a celestial body at the moment of transit could be more readily determined. We must not forget that transit observers have to determine what is called the declination of a star (its distance from the equator), as well as what is called the right ascension, or distance measured parallel to the equator from a certain assigned point on that circle. For this purpose the horizontal lines *a a'*, *b b'*, &c., (Fig. 2) would be useful, but not sufficient. I incline to think that the method used to obtain accuracy in observations for determining declination involved a very practical use of the grooves *k k'*. Possibly a horizontal bar ran from *k* to *k'*, carrying vertical rods, across which, at suitable distances, horizontal lines were drawn (or, better still, horizontal rods could be slid to any required height). The horizontal bar could be slid to any convenient position, the vertical rods adjusted, and at the time of transit the horizontal rods could be shifted to such a height as just to touch a star as seen by an observer in the Gallery at the moment of mid transit.

If a telescopist in our own time will try to plan out a method of determining the declinations and right ascensions of stars (say, for the purpose of forming a trustworthy star chart or catalogue), without using a telescope, by using such an observing place as the Great Gallery, he will see how much might be done, so far as equatorial and zodiacal stars were concerned; and they are altogether the most important, even now, and were still more so in the days when the stars in their courses were supposed to rule the fates of men and nations.

THE CRYSTAL PALACE ELECTRICAL EXHIBITION.

FIRST NOTICE.

IT is greatly to be regretted that this exhibition, like most others, is so slow in attaining to anything approaching what one may justly consider should be its proper dimensions. We are, of course, aware that there are many causes retarding the completion of the preparations, and that it is, generally speaking, to the interest of the exhibitor to get his show in a position fit for inspection as speedily as possible. It is, nevertheless, unreasonable to suppose we can endorse the unduly laudatory opinions expressed by the majority of our scientific critics. We shall see, as we go on, something of the great state of unpreparedness which pervades nearly every department, and although there is even now plenty of work for those who intend to thoroughly study the mines of information as they are laid open, it cannot fail, for some two or three weeks to come, to engender very great disappointment in the minds of those who can only pay one or two visits to Sydenham.

It is our intention this week to give simply a brief description of the general arrangements, and hereafter to describe in detail the various collections of apparatus, not forgetting to keep our readers posted up in the additions made to the Exhibition week by week.

The exhibits are divided into fourteen classes, some of which, however, may, so far as we are concerned, be regarded as sections of larger and more important classes. The class which commands the greatest amount of attention is that which embraces the various systems of electric lighting, while telegraphy appears to rank next in interest. There is one section of the exhibition, not favoured with a class to itself, which has great attractions for many of our readers, namely, that which comprises the various displays of apparatus for teaching the science of electricity. These will all be described in their turn. Commencing at the north end of the Palace, we enter first the machine-room of the Anglo-American Brush Electric Light Company. It is a spacious room, and has been floored with a layer of concrete, to give the necessary firm foundation for the machines. There is a goodly display of dynamo machines, all fixed in position, and most of them waiting for the arrival from the contractor of four out of the five engines required! The one engine is driving two dynamo machines, one of which supplies sixteen arc lamps, and the other a number of incandescent lamps of the Lane-Fox type.

There is also fitted up on one base a small steam-engine, working direct on to a small dynamo machine, capable of maintaining three arc lamps. The machine makes 1,100 revolutions per minute, and presents altogether a very compact and commendable appearance.

The arc lamps of this company are working well and steadily, and no doubt, when all of them are lighted, will receive universal admiration. The Lane-Fox incandescent lamps are tolerably good, but do not yet seem sufficiently steady, nor do they all appear to be equal in resistance, as some are much brighter than others. Leaving this company's exhibit, which is located in and about the Tropical Department we come into the nave, where, right away to the orchestra, there is a good display of arc lamps. The first are those of the Electric Light and Power Generator Company (Maxim and Weston systems). This light is very steady and very brilliant. It seems at present to leave nothing wanting in this direction. The company has also started a number of Maxim incandescent lamps, very tastily fitted into a kind of candelabra. They, however, are not uniform in brilliancy, for while some are equal to over twenty-five candles, others are little more than dull red.

Next is the display of the British Electric Light Company (Brockie system). These lights, nine in number, are very powerful; but it is to be feared they will never be quite free from momentary jerks, so to speak, which are, no doubt, due to the fact that perfectly homogeneous carbon-rods are unobtainable. Turning into the Egyptian Court, we find a sweetly-pretty collection of glass chandeliers and table-lamps of various designs (some of the latter several feet high), and fitted with the British Company's incandescent lamp. The glass is manufactured by Messrs. James Powell & Sons, Whitefriars Glass Works, and it is expected that they will all be ready for lighting in the course of a day or two. In the North Nave are also the stands of the Post Office, War Office, &c., including the almost empty cases of the School of Submarine Telegraphy. The Handel Orchestra is lighted by Messrs. Rowatt & Fyfe, with six Pilsen lamps, the same firm having ten Joel lamps in the Pompeian House. The latter at present appear very irregular. In the South Nave are four of Messrs. Strode & Co.'s arc lamps (Mackenzie system), which oscillate considerably. Messrs. Siemens & Co. and other houses should light the remainder of the nave, but they are all very backward. It is said that some firms fixed their plant on to the wooden floor. The immediate result can be conceived as easily as described. The concert room is well lighted with 250 of Edison's incandescent lamps. They are extremely steady, and, owing to a peculiarity in the manufacture, are said to be very durable. By far the most attractive display is made by his exhibit in the entertainment room, where he has 250 lamps arranged in all sorts of imaginable designs. In the centre of the room is a large brass chandelier, heavily laden with lamps placed in elegantly-tinted glass shades. The effect is grand in the extreme, and wins immediately the entire sympathies of all who enter the apartment. No amount of gas could give the brilliancy, for the quantity required would be much more than the air contained in such a room could support. To judge of the electric light, we must, of course, divest it of its gorgeous decorations, but even then we shall find it excellent.

Various exhibitors occupy the floor of the nave, while the costume-room is beautifully adorned by Messrs. Hammond & Co. (Brush System), who also light very effectively the corridor leading to the low level station. The gallery, which should be almost entirely occupied by exhibitors, is comparatively empty. This state of things is no doubt greatly due to the entire absence of electric light, and consequent nearly total darkness. On the ground-floor, Mr. Edison, The British Electric Light Company, and the Electric Light and Power Generator Company have their engines and dynamo machines, all well-fitted on solid foundations, but neither of them in a state of completeness.

Next week, some of the exhibits will be dealt with in detail, and the main principles of the more important apparatus explained.

NATURAL RUBBISH HEAPS.

By JAMES GEIKIE, LL.D., F.R.S.

IN a paper recently read before the Perthshire Society of Natural History, Dr. Geikie gave some account of the various accumulations of rock-*débris* which are now taking place in this country. He described the appearance presented by many of the mountain-tops and slopes in our hilly regions. The rocks were often more or less concealed below masses of coarse angular fragments of all shapes and sizes. The hill-tops frequently looked as if they had been subjected to the battering action of some mighty hammer, which had smashed and shattered the rocks to a considerable depth; so that, if we wished to get at the solid and undisturbed parent-mass, we should first have to clear away many feet, and even sometimes many yards, of more or less loose *débris*. The slopes of such *débris*-capped mountains were invariably clothed with long sloping taluses of similar fragments, which swept down at a high angle to the valleys, and at the base of these slopes large blocks and isolated masses of rock were of common occurrence. No one who should examine these phenomena could for a moment doubt that they owed their origin to the action of the atmospheric agents. Dr. Geikie then described, in a somewhat detailed manner, the mode in which the rocks were broken up by the disrupting force of ice. Water found its way into the crevices of the rocks, and, being frozen there, the joints were gradually widened by the expansion of the ice, again and again repeated. When this action took place on a flattish hill-top the rocks were simply disrupted, and the separate fragments pushed asunder. But upon the verge of precipices, and upon steep slopes, the disrupted fragments were shot downwards, as soon as thaw set in. There were other ways in which rock-*débris* or natural rubbish-heaps were formed. Strata were often undermined by the action of water, and large masses of rock, deprived of their support, tumbled down in ruins. This could be seen at the base of sea-cliffs, and along the margins of streams and rivers. Then, again, some kinds of rock which were more or less soluble in water were liable, under certain circumstances, to be disjointed and broken up. Limestone, for example, was dissolved by the action of acidulated water working its way downwards through the natural fissures of the rock. In process of time these fissures were widened by this solvent action, and converted into irregular channels and tunnels. This was the origin of most of our limestone caverns. Water continuing to percolate down into such caves, gradually loosened the limestone that formed the roof, and now and again large and small fragments of the rock, losing cohesion, fell to the ground. Another cause for the origin of rock-*débris* was to be found in the peculiar geological structure of certain masses of strata, which were so arranged as to render them liable to sudden and wholesale demolition. When a mountain was built up of a series of porous and non-porous strata, arranged in alternate layers, dipping into the valleys at such a low angle that the edges of the beds were exposed upon the mountain-slopes, such a mountain might at any moment be destroyed. Dr. Geikie then referred to several remarkable examples of such catastrophes. In the case of the Rossberg, in Switzerland, the destruction was due to the

fact that long-continued rains, soaking down through porous beds above, were arrested by beds of non-porous clay, which, however, became softened to such a degree that the mountain-mass of strata that rested upon them slid forward upon them, and rushed down into the valley. After describing yet other modes in which natural rubbish-heaps were formed, Dr. Geikie went on to remark that all the phenomena referred to were more or less exceptional, and that the agent which effected the greatest results was frost. Some of the other agents he had described could only work under certain geological conditions; — others, again, were somewhat limited in their action, and tended to remove the rubbish-heaps which they themselves had accumulated. But the action of frost in a country like ours was, he might say, general. It affected every part of the land, but of course the amount of work it performed was very variable. Its results were most conspicuous in mountain regions, where frosts were not only more frequent, more intense, and more prolonged, but where the physiographical conditions of the surface lent their aid in the most effective manner. The rock-*débris* gathered to the greatest thickness upon slopes at the base of a rocky precipice. This was natural, for the steep rocks above, shattered by frost, showered their *débris* downward. But on flat hill-tops the time must come when the formation of rock-*débris* must terminate. The rock would only be acted upon to as great a depth as the frost could penetrate. Some account of the frost-riven *débris* of other countries was then given, more especially of the Swiss Alps, and northern regions of Europe and North America. It was remarkable that many parts of our own country were covered with sheets of *débris* which had apparently long ago ceased to accumulate, and these sheets occurred not only upon comparatively low ground, but even in mountain regions. The angular fragments were grown over now with lichen and heath, and even with natural wood, and in every feature betrayed the marks of great antiquity. And not only so, but they occurred in positions to which loose blocks detached from the rocks at higher levels could not possibly have rolled. It was hard enough to account for the presence of such sheets of ancient angular *débris* in a country like Scotland, but it was more difficult still to explain the presence of similar sheets of angular *débris* at low levels in the South of England, in Northern France, in Southern Spain, and at many places upon the borders of the Mediterranean. After giving a description of the so-called "Head" of Devonshire, Cornwall, &c., and the similar accumulation upon the coast of Normandy, Dr. Geikie went on to give some account of the clay-with-flints of the Paris Basin and the great consolidated *débris*-heaps or *breccias* of Gibraltar. By means of sections across the Rock he showed the position of these *breccias*, and explained how they had been formed at two different periods, separated by a considerable time, during which the Rock of Gibraltar was submerged for some hundreds of feet. After remarking upon the fact that similar *breccias* occurred in Corsica, Sardinia, Malta, Italy, Cyprus, and other places, he proceeded to explain the mode in which they, the "Head" of Cornwall, the ancient *débris*-heap of Scotland, and similar formations elsewhere, had been accumulated. The angular fragments had been dislodged from the rock of which they once formed a part by the action of frost. But they could not have rolled to their present position upon the low grounds by the mere impetus acquired by them when they were disrupted from the rocks above. They would naturally come to rest upon the low grounds at the base of the cliffs, unless some other force than the mere impetus of their fall had been urging them forward. We now meet with them at distances of many hundred yards away from the foot of the cliffs and steeper slopes, and to have reached their present positions they have travelled over a surface-slope not greater in many cases than 5°, or even 3°. The *débris* speaks not only to the action of hard frost, but of heavy snows. It was the melting of the latter and the saturation of the *débris*-heaps which caused the rubbish to flow as it were outwards from the base of the cliff, and doubtless this action was still further favoured by the alternate freezing and thawing of the water-soaked masses. It might seem strange to speak of snows and hard frosts in the islands and along the borders of the Mediterranean, but the evidence of former colder conditions was not by any means restricted to ancient *débris*-heaps or *breccias*. In a few words Dr. Geikie then sketched the broad results which had been arrived at by glacialists as to the former extent of the European snow fields and glaciers during the Glacial Period, and he showed that these, taken in connection with the evidence furnished by organic remains, both animal and vegetable, abundantly confirmed the conclusions to which the phenomena of the ancient rubbish-heaps appeared to point. The climate of all Europe had been greatly affected; not only did an enormous ice-sheet, extending from Scandinavia and burying the British Isles, creep southward over the plains of Northern Germany, but all the mountain-tracts became centres of glaciation. The present glaciers of Switzerland were the degenerate successors of great icefields which now meet with their nearest analogues in the Arctic Regions. And many hilly districts in France, Spain, and Eastern and Southern Europe, which were now destitute of glaciers, were formerly the seats of extensive snow-fields and glaciers of no mean size. While in other places, such as the low grounds of Southern England and France, and hilly regions bordering on the Mediterranean, where the conditions were not favourable to the formation of glaciers, considerable snows fell, and hard frost ruptured and shattered the rocks. It was to this period of cold that most of those great accumulations of rock-*débris* belonged — those natural rubbish-heaps which had now ceased in many places to accumulate. They thus bore strong evidence to the former extent and intensity of ice-action during the Glacial Period.

DR. CARPENTER ON VACCINATION

AT a monthly conference of the London Society for the Abolition of Compulsory Vaccination, held at the Steinway Hall (Dr. Andrew Clark in the chair), an address was given by Dr. W. B. Carpenter, C.B., on the increase of small-pox mortality in London during the year 1880. He pointed out the inadequacy of the objection that a system of compulsory vaccination outraged the rights of individuals, contending that in health, as in education, it was the paramount duty of the State to secure, as far as possible, the public advantage. The State, in his opinion, was morally bound to intervene in such a matter between the parent and the child, for the good both of the child and of society at large. He proposed to speak with special reference to the outbreak of small-pox in 1880, which, he understood, was specifically mentioned in the resolution that was to be moved in the House of Commons by Mr. P. A. Taylor. That outbreak, according to his view of the case, afforded grounds, not for the repeal of the Act, but rather for making its operation more complete and stringent. It was necessary first to consider the history of small-pox, with regard to which very important statistics existed in the bills of mortality for the last 200 years. In the case of other exanthemata – scarlatina, for instance – doubts might have been cast on the accuracy of the earlier figures; but small-pox had always been clearly recognised and distinguished from other diseases, and no such doubts could therefore be entertained. Now, from 1660 to 1678, the general mortality of the kingdom was 80,000 in every million of living persons, and the small-pox mortality was 4,170; in 1728-57 the general mortality was 52,000 per million, and the small-pox mortality 4,260; in 1771-80 the general mortality was 50,000, and the small-pox mortality 5,020 – a slight increase, which was probably due, as Dr. Heberden said long ago, to inoculation. However, the average small-pox mortality in the period from 1660 to 1800 was about 4,000 per million. It was noticeable that at that time the disease periodically appeared in its worst form, and was the terror of all classes. Thus Louis XV. died deserted by all except Madame du Barry, and the priests who chanted mass in the Chapelle Ardente were said to have been "condemned" to do so. And in 1750 Horace Walpole wrote, "Lord Dalkeith is dead of the small-pox in three days." These, of course, were instances in which the disease appeared in its greatest intensity, and attacked the rich, who in these days would ordinarily have little to fear from it. He could scarcely suppose that an outbreak of small-pox—say, in Pimlico—would deter her Majesty from visiting Buckingham Palace. For the decade 1801-1810 the general mortality was 29,000 per million, and the small-pox mortality 2,040. In 1831-35 the general mortality was 32,000 and the small-pox mortality had fallen to 830. At that time he had himself seen as many as 100 cases of blindness from small-pox in unvaccinated persons, and it was probable that in the last century two-thirds of the patients at the eye hospitals were blind from the same cause, while the proportion now was only 5 per cent. In 1840 the Legislature provided the means of vaccination, and the result was that the mortality fell to 100 per million. Then came compulsory vaccination in 1853, and the small-pox mortality in the decade 1851-60 was only 278 per million. In 1861-70 the number was 276. He now came to the years 1871-80, which period was unquestionably exceptional. The mortality in these years among unvaccinated persons was so extraordinarily great, and the disease itself was so violent, as to suggest the notion that it might be indeed the Black Death of the Middle Ages. Yet, as far as he knew, no person who bore the evidences of vaccination had died of small-pox in the last year. In 1871 the disease was severe everywhere in Great Britain, but especially in Scotland, where compulsory vaccination had not been then adopted. Since that time, however, vaccination had been made compulsory in Scotland, where it was now enforced more effectually than in England, the result being that for the last five years there had not been twelve deaths a year in that country from small-pox.

London, on the other hand, thanks to the efforts of the Council, there was an unvaccinated residuum which kept the disease alive. The epidemic had come to us from France, and had arisen there from the unsanitary condition of the French soldiers during the late war. Having examined the circumstances of the epidemic and from a study of epidemics in general, he had no hesitation in saying that the period 1871-80 was altogether exceptional, and that the rate of small-pox mortality during that decade afforded no basis for an argument against vaccination. He need only make one more observation. His opponents would doubtless urge that such places as Dewsbury, Leicester, and Keighley, where the anti-vaccinationists were strong, had had a comparative immunity from small-pox. But the truth was, that the disease had already died out in those towns, and that the mere disuse or neglect of vaccination did not reproduce it. As an illustration of the fact that no sanitation would suffice to exclude small-pox, the case of San Francisco might be cited. In the Chinese quarter of that city a smouldering fire of small-pox had existed for some time, but there had been no considerable outbreak since the autumn of the year 1879, when nearly 150 cases occurred in the best and richest parts of the city, in spite of the fact that, as the very low annual death-rate showed, the sanitation of the city was singularly good. Of the children, however, all of whom had been vaccinated, many from heifer lymph, only two or three took the disease.

THE MOON'S BIRTH BY TIDAL EVOLUTION.

In response to the wishes of many readers of Knowledge, we had intended to prepare for these pages a paper on the views to which Mr. G. Darwin has been led, and which Dr. Ball has eloquently expounded, respecting the birth of the moon by tidal evolution. It occurred to us, however, when our essay was nearly completed, that our readers might like to hear Dr. Ball himself on the subject; and we now have much pleasure in announcing that next week a paper from the pen of the Astronomer Royal for Ireland on the moon's birth by tidal evolution will appear in these pages; to be followed by another on the astronomical consequences of such evolution.

The Editor.

THE MENACING COMET.

Readers of Knowledge who have followed my remarks on the various predictions which have been made respecting the approaching end of the world, must have been inclined to exclaim:—"Is Saul also among the prophets?" when they heard that, as the *Spectator* tells us, I had definitely indicated the year 1897 as one in which the world would, in all probability, come to an end. I have carefully read over the essay in my recently-issued "Familiar Science Essays," to see whether it should suggest these startling anticipations; and I find nothing there which does not seem in perfect accordance with observed facts and scientific deductions therefrom. All that is there said I certainly adhere to still. How far it can be regarded as threatening the end of the world in 1897, I shall give the readers of Knowledge an opportunity of inferring next week, when a short article on the comet which is thus thinks the *Spectator* to bring the world to catastrophic end will appear, illustrated by a picture of that menacing object. Possibly after reading that article, those who have urged me to reconsider my verdict may find that some chance is still left for our good old earth.

R. A. Proctor.

Errata.—Lines 9, 11, 12 and 13, p. 270, for "wood" read "woad." Line 37, "nitril" for "nitrite." Lines 39 and 19, for "ortho-nitro phenyl glyoxalic" read "ortho-nitro-phenyl-glyoxylic."

Letters to the Editor.

The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with their writers. All communications should be as short as possible, consistently with full and clear statements of the writer's meaning.

All Editorial communications should be addressed to the Editor of Knowledge; *all Business communications to the Publishers, at the Office, 74, Great Queen-street, W.C.*

All Remittances, Cheques, and Post-Office Orders should be made payable to Messrs. Wyman & Sons.

⁂ All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.

All Letters or Queries to the Editor which require attention in the current issue of Knowledge, *should reach the Publishing Office not later than the Saturday preceding the day of publication.*

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

Our Correspondence Columns.

OUR LETTERS, QUERIES, AND REPLIES.

[268]—Letters, queries, and replies reach us in such numbers, that not only are we unable to find room for a third of them, but they involve a tax on our time seriously interrupting the progress of more important matters. We are obliged, therefore, to adopt the following rules:—

(I.) Letters to have a chance of appearing must be concise; they must be drawn up in the form adopted for letters here, so that they may go untouched to the printers; private communications, therefore, as well as queries, or replies to queries (intended to appear as such) should be written on separate leaves.

(II.) Queries and replies should be even more concise than letters; and drawn up in the form in which they are here presented, with brackets for number in case of queries, and the proper query number (bracketed) in case of replies.

(III.) Letters, queries, and replies which (either because too long, or unsuitable, or dealing with matters which others have discussed, or for any other reason) cannot find place here, will either be briefly referred to in answers to correspondents, or acknowledged in a column reserved for the purpose.

We beg that correspondents will consider how the matter stands. It is essential that each number should contain a certain portion of original matter, illustrations, notes on science and art, short extracts from home and foreign journals, mathematics, and so forth. We cannot yet enlarge Knowledge more than we have already done; to compress our correspondence into the space left open for it, by abridging, extracting, and putting the matter so arranged into proper form for the printers, would require either the whole time of the editor, or the assistance of a staff of sub-editors. Our correspondents must, therefore, do the work of abridgment and arrangement themselves; at any rate, they must not be annoyed if, failing this, their communications are wasted.

To ninety-hundredths of our readers no apology for this notice need be given. On the contrary, some explanation may be due to them for the way in which correspondence has threatened to interfere with the proper work of Knowledge.

The Editor.

FLESH FOOD.

[269]—I trust you will allow me to point out in your columns the extreme unfairness of the comparison you suggest between vegetarians and flesh-eaters. You ask vegetarians to produce a list of famous men of their persuasion equal to that which can be made out by their opponents. Now this would be an excellent and perfectly fair test, if exactly half the civilised world had always been vegetarians; but as matters actually stand, it is grossly unfair. You might as well ask the Quakers to make out such a list, or the Albinos, and the test would be just as valuable. The list of famous men, if it is to be of any use, must be made out with due regard to the proportion between vegetarian and carnivorous candidates for fame, and from this test the vegetarians have no cause to shrink.

Your instance of Newton is not altogether a good one, as he lived on vegetarian principles while he wrote the "Principia," from the desire to keep his mental faculties unobscured.

J. L. JOYNES.

[Our correspondent does not seem to notice that we were saying jestingly what he himself gravely (and of course correctly) points out.—ED.]

THE MOON AND THE WEATHER.—INTRA-MERCURIAL PLANET.—THE ICE-AGE IN BRITAIN.—ACTION OF THUNDERSTORMS.

[270]—Mr. Bulley (p. 247) seems to me to be one of that numerous body who mistake assertion for proof. I will deal very shortly first with his science, and next with his facts. Now, *in primis*, he repeats a very old fallacy indeed, when he says that "it must be clear to every tyro in natural science, that if it be rational, and in accordance with the verification of science, to assert that the moon's influence acts upon the waters of the ocean, it cannot be foolish and irrational to hold that the same influence affects the waters of the clouds and the air in which they float both ponderable bodies, equally subject to the laws of gravitation." Just so. The late Professor Daniel conducted an elaborate series of barometrical experiments on the summit of Box Hill, in Surrey, and showed conclusively that there is actually a semi-diurnal *tide* in the atmosphere. But what then? If the gravitational action of the moon on our atmosphere affected terrestrial meteorology, the weather ought to change twice a day! Does it? We are told, though, that atmosphere changes occur when the moon either crosses the equator or attains her greatest north or south declination. Let us try this theory for the last three months. Writing with my own daily Meteorological Register and the *Nautical Almanac* open before me, I will see how far Mr. Bulley's theory holds for this part of England. On Nov. 22, 1881, at two a.m., the moon was on the equator. The cold cloud and damp of the previous day continued; from nine a.m. to nine p.m. the barometer rose exactly 0·002 inch; and, I may add, precisely the same weather (with, however, a rise in temperature) prevailed for about a week. The moon was again on the equator on the 29th, and, once more, nothing happened. Nov. 8 (when she attained her greatest north declination) was foggy. I am ignorant whether, according to Seleno-meteorology, great north declination of the moon should bring fog. On Nov. 22, when she attained her greatest south declination, the wretched wet weather from which we suffered, both before and after that date, persisted without change; and one of several gales which visited us towards the end of the month, blew in some parts of the country.

In December the moon reached her greatest declination north at 1 a.m. on the 6th; and here again the wet weather which had persisted (and subsequently persisted) from the beginning of the month, underwent no alteration whatever. On the 13th, however, when the moon was on the equator at noon, it actually ceased raining, or practically so, for twenty-four hours. The moon reached the most southerly point of her orbit at 10 a.m. on December 20; but the wet which distinguished the month continued. At 1.30 in the afternoon of December 26, the moon was once more on the equator, and here again nothing whatever was noticeable save the great height of the barometer, though this endured from the 23rd to the 29th. On the 18th there was a tremendous gale, and an almost equally heavy one on the 20th. On the occasion of the former and fiercer storm of the two, the moon was at some considerable distance from her "stitial colure." If we turn now to 1882, the moon attained her greatest north declination at 8 a.m. on the 2nd, and again we had a heavy gale with rain. At 8 p.m., on the 9th, she was in the equator, and again it blew, as it had done on the 5th, 6th, 7th, and 8th. At 9 p.m., on the 16th, the moon reached her greatest south declination, but not the slightest change took place in the fog and calm, which begun on the 11th, and lasted eleven or twelve days. Lastly, our satellite was on the equator once more at 10 o'clock last night (22nd), the high barometer of the last twelve days persisting, and one or two temporary peeps of clear sky being vouchsafed; otherwise, no change whatever occurred. Now, what are we to say to all this? Here we find all sorts of weather occurring when the moon has great north declination, great south declination, and no declination at all. Mr. Bulley makes certain assertions, but assertion is not proof; and the crucial test of any such theory as his is for every meteorological observer to institute such a comparison as I have attempted here. His concluding paragraph about "the chemical rays" of bodies whose diameters subtend angles of 40″, 30″, 16″, and so on, scarcely merits any serious reference.

Mr. Jones (query 188, p. 255) may possibly be thinking of the utterance of Le Verrier, in the *Comptes Rendus*, for Dec. 21, 1874, thus oddly translated in the R.A.S. *Monthly Notices*, vol. xxxv., p. 155:—"There exists in the neighbourhood of Mercury, doubtless between the planet and the sun, a matter (sic) as yet unknown. Does it consist in one or more planets or in more minute asteroids, or even in cosmic dust? The theory tells us nothing on this point. On numerous occasions trustworthy observers have declared that they have witnessed the passage of a small planet over the sun, but nothing has been established on the subject ('on n'est parvenu à rien coordonner à ce sujet')."

Referring "Arachnida" (query 192, p. 255) to that most interesting book, Geikie's "Great Ice Age," for full details, I may say here, that the stones frozen into icebergs and glaciers are polished and striated in a most striking and unmistakable way, as they are pushed over the rocks by the motion of the masses of ice in which they are imbedded, and that such polished and striated stones are found over nearly every part of the United Kingdom. Moreover, where the beds belonging to their geological horizon are fossiliferous, their contained fauna is arctic.

I think that what "F.A.S." (query 193, p. 255) apparently regards as a fact is at least questionable. The great heat and approximate saturation of the atmosphere which generally precede summer thunderstorms supply the most favourable possible conditions for acetous fermentation; and both beer and milk not infrequently turn sour during very hot weather without any thunder at all. Acetous fermentation is, though, a process of oxygenation, and the abundance of free ozone in the air during a thunderstorm may proximately or remotely affect liquid organic compounds, although, if this were the case, it is hard to see why no acidification takes place during winter thunderstorms. I am inclined myself to regard the belief that lightning turns beer sour because it does not know how to conduct itself as a popular delusion.

A FELLOW OF THE ROYAL ASTRONOMICAL SOCIETY.

INTELLIGENCE IN ANIMALS.

[271]—Some years ago, a small terrier belonging to a neighbour, having shown a propensity to come and chase my cats about my orchard, I repelled his visits with a stick, in flinging which I calculated the distance he would have run by the time that the missile reached the ground. I thought he had learned the lesson that I intended to teach him. Instead of which, he had thought of a dodge. I saw him one evening approaching the house, when my weapon flew as before. To my surprise, instead of bolting right away, he rushed towards me several yards, and then turned sharply round, and was off. The stick, therefore, instead of dropping objectionably near to his heels, flew high over his head, as he had calculated that it would. Thus was I outwitted by a dog—to my great delight.

Bards are often set down as but a nonsensical, visionary sort of people; but the following lines are among the many that might be quoted from the writings of poets, to show that they often display evidence of the possession of more common sense than can be boasted of by some of those who think themselves to be scientific philosophers:—

"Then vainly the philosopher avers
That reason guides our deeds, and instinct theirs.
How can we justly different causes frame,
When the effects entirely are the same?
Instinct and reason how can we divide?
'Tis the fool's ignorance and pedant's pride." *Prior.*

As an illustration of a dog's capacity to distinguish between the characters of man and woman, I may mention that once, when a farm-labourer was telling me that a certain big, vagabond dog made himself a nuisance in the neighbourhood by entering the cottagers' rooms as he pleased, on my asking why they did not order him out again, he replied, "Ay, but he won't go out *for a woman*." Which feminine trait gave him this boldness to please himself in the matter?

F. RAM.

INFLUENCE OF SEX UPON MIND.

IV. DAILY EXPERIENCE.

[272] The common opinion founded on observation, that woman's reasoning and reflective powers do not equal man's, is not confined to physiologists deciding from head-forms and organic structure; but is held by the vast majority without theory, judging solely from experience and practical knowledge. Whately* defines woman as "a creature incapable of the exercise of reason, and that pokes the fire from the top." It is a colloquial axiom that you cannot argue with a woman. I have heard many lady-lecturers; not one argued consecutively; from beginning to end it was declamation! They

* Whately, was it? Like most other stories, it has been told of others, and in my college term it was told of one of our most profoundly logical dons.—ED.

began by [illegible] as that the [illegible] position they should have time to prove, jumped to a conclusion logically unattainable, praised all who agreed with them as *friends*, and savagely scolded all opponents as *enemies*." "Female disputants, learned or unlearned, seldom know how to reason. They assert and declaim, employ wit, eloquence, and sophistry to confute, persuade, or abash adversaries, but distinct reasoning they neither use nor comprehend" (Miss Edgeworth's "Letters for Literary Ladies"). Woman's forte is certainly not argument; as well expect her to chop wood as logic. (I have seen American women chop wood!) In generalisation, arrangement, and concentration of ideas, she cannot compete with man. "They never see, whether for good or bad, more than one side of any question, and always the one which first presents itself" ("Oliver Twist").

Inability to argue is no mark of woman's absolute inferiority or non-development. Sexual equality advocates admit woman's lack of reasoning faculty compared with man. But (deceived by their hypothesis) they declare the power exists *latent*, and could be developed to the same extent as in man by a masculine education. They might as well argue that the power to equal man in size and strength exists latent in woman. On their assumption, inability to argue is a defect in woman. I maintain it is the result of natural organisation; therefore, no more a defect than woman's lack of manly size, strength, shape, beard, and complexion. Plato wished to submit the sexes to the same physical training, but even *he* declared woman in every respect weaker than man. Her mind corresponds with her body. Some men grow impatient with female relatives for inability to argue. Some despise the sex for this peculiarity. The "strong-minded" libel the "weak-minded" sisterhood as poor, stunted, distorted, mentally-arrested creatures! (*Victoria Magazine*, May, 1879.) All are wrong who adduce absence of reasoning power as a defect in woman. The obvious error is to gauge woman's mind by a masculine standard, and to expect the astounding absurdity that woman can, and should, possess all man's mental qualities in addition to her own! Wiseacres blurt out that woman is stupid, that her education has been neglected, because she cannot write like Locke, Bacon, Newton, Shakespeare, and Milton. Inability to argue—which would be a defect in man—is a characteristic and valuable *quality* in woman!

It is a most irrational conclusion that woman is mentally undeveloped, and claims our pity, and masculine, mental, and physical culture, to enable her to hold her own. Does man's pre-eminence in reasoning power give him an unfair advantage over woman? It would do so, if woman were—what sexual equality advocates misrepresent her—man's disappointed rival, an undeveloped younger brother, with a long lee-way to make up. "Woman is not undeveloped man, but diverse" (Tennyson). Two sexes constitute humanity. To tell women to copy man is a gross insult. She was no more intended to argue with, than to fight with, man. Very serious consequences would result from the sexes having minds constituted alike. Suppose that woman could generalise like man, could ascend to principles, could think as profoundly, and reason as correctly; and that man had woman's intuitive powers, and capacity for details. Woman would then become man's rival, instead of his help-meet. Each sex being able to dispense with the other's mental qualities, man and woman would live in perpetual discord. But at present, in spite of woman's alleged mental defects, harmony reigns between the sexes. There is constant reciprocal need of the male and female mind supplementing one another. All tends to mutual inter-dependence and happiness. Each sex, in turn, follows the other's impulse, listens to the other's advice; each influences in his or her respective province; each obeys, and both rule.

J. McGrigor Allan.

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[273]—At the beginning of his letter of the 13th, Mr. McGrigor Allan makes an assertion which, although true in the abstract, cannot logically be advanced as an argument for the mental inferiority of women. It may be that in the past men have practically monopolised the control of human thought and human institutions; but this circumstance is no more a voucher for their intellectual superiority than the defeat of the Romans was proof of the mental supremacy of the Huns. It has been largely a question of physical force, the assertion of which is proportionate in despotism to the ignorance or degradation of the male community.

What authorities can Mr. Allan cite for his statement that savage life shows the nearest approach to physical equality of the sexes? The hardships to which savage women are accustomed from their infancy are certainly such as could not safely be imposed upon civilised females. But are we to suppose that savage life has not increased male robustness in an equal proportion?

Mr. Allan incidentally remarks that "among savages woman is a slave." This is a fatal admission; for savages do but give physical scope to the spirit of overbearing which animates scoffers at women. Name a nation where women are debarred from social influence, and you have named one which is proportionately backward in liberty and knowledge. But, to be consistent, Mr. Allan should agitate against the part which women already take in state affairs. He cannot surely resign a large share in such an important function as the education of future generations into the hands of those who, he declares, do not possess "sufficient development of the abstract principles of justice, morality, truth, &c., to hold society together for one week!"

It is a remarkable fact that Mr. Allan's school, although it has ever impeded and opposed, as it still does impede and oppose, the higher education of women, always seeks to make an argument of their small achievements in invention and philosophy.

Mr. Allan hath it that "the eternal subordination of woman is conclusively exemplified in her exaggerated admiration for the male prerogatives—strength and intellect." Your readers must judge as to the soundness of the proposition that admiration—whether exaggerated or not—for strength and intellect is evidence of inferiority; but I have always heard that a profound appreciation of talent was the special characteristic of the world's greatest men.

For the rest, Mr. Allan's letter makes a series of pompous and sweeping assertions, supported by an extract from a novel, and so spiced with illiberal flippancy as to contrast strangely with his complaint of female injustice.

L. Burke.

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[274]—As "Only a Woman" considers the philosophy of Shakespeare conclusive on the subject of "women possessing justice," may I call her attention to a few things that the subtle understanding of the immortal bard has given forth to the world. He says, "Frailty, thy name is woman!" "Be it lying, note it, the woman's; flattering hers, deceiving hers;" "Women are frail as the glasses where they view themselves;" "Even to vice they are not constant." About their logic, he says, "I have no other but a woman's reason." Satirical view of their constancy: "Constant you are, but yet a woman; and for secresy, no lady closer;" "How hard it is for women to keep counsel."

So it will appear that Shakespeare does not represent woman as "infallibly faithful;" and I think it is only just to bring forward his opinions, since they have been courted. Some of his female characters are actuated by the most selfish and vicious motives that can possibly be conceived.

R. C. Fraser.

[To say that Shakespeare makes certain of his characters express these views, would be nearer the mark. What Shakespeare himself thought cannot be judged in this way.—Ed.]

ASBESTOS PAINT AND THE SAFETY-LAMP.

[275]—Upon reading the very interesting description in Knowledge of the successful experiments recently carried out at the Crystal Palace with the asbestos paint, I was led to infer that another important application of it—namely, to the wire-gauze of the ordinary safety-lamp—might be adopted. For, if this gauze were so protected, it would not, I conceive, even under the most unfavourable circumstances, be raised *above* incandescence, and, therefore, could never, while entire, give rise to an explosion, Sir Humphrey Davy, in his treatise on the safety-lamp, having declared "that even red-hot gauze of the proper degree of fineness will abstract sufficient heat from the flame of carburetted hydrogen to extinguish it." In fact, on account of the very low conducting property of asbestos, and the consequent difficulty of raising it to a high temperature, I ventured to propose, some years since, the substitution of an asbestos gauze or netting for the iron-gauze cylinder of the "Davy," but do not know whether the suggestion was ever experimentally realised.

Should you deem this brief communication worthy a place in your valuable pages, I shall feel gratified.—Yours, &c.,

W. H. O.

VEGETARIANISM.

[276]—Permit me a few lines of comment on part of a letter relating to the above subject, published in your last number, and signed "A Fellow of the Royal Astronomical Society."

If stories be really valuable in controversy, I can produce scores, the moral of which is exactly contrary to that cited by your correspondent. But my experience of "stories" is this, that they are seldom related with scientific exactness, and that minute investigation generally reveals some detail which has been either wilfully or ignorantly suppressed in narration, and which invalidates the whole point it is sought to establish. Personal observation of facts in one's own immediate sphere, constitute, in my opinion, the most valuable kind of statistics. Some five years ago I had very severe symptoms of tubercular phthisis, a disease hereditary in my family. The physicians whom I consulted recommended me cod-liver oil,

raw meat, and what is commonly called "good" living. They were, however, of opinion that these means would but ameliorate my condition temporarily, my fate being sealed. As I was a vegetarian, and had begun to study medicine, I did not put into practice the advice given me. Instead of the raw meat, I took cold porridge made of oatmeal and milk, maccaroni, and other farinaceous foods, with as much fruit as I could get. I used hygienic means also, with the details of which it is unnecessary to trouble you. But I took no drugs, and no fish-oil. Instead of dying, I recovered my health, and shortly afterwards returned to my hospital course in Paris. Four years afterwards, I took my degree, and it is now my custom to recommend to my patients the dietary which saved me from death. I have found several of my patients greatly improved in health by following my example, and I have never found one the worse for it.

As I am "fanatic" enough to be quite sure I am right, I can afford to meet objections to my mode of life with equanimity, knowing that they proceed only from insufficient consideration or pardonable ignorance of the true bearings of the question.—Yours, &c. ANNA KINGSFORD, M.D.

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[277]—Having inserted a letter (207, p. 251) in which an altogether wrong construction is put upon the principles of vegetarianism, I hope you will allow the other side a little space for reply. Vegetarians are not such fools as to say that those living on a moderate amount of animal food, and temperate in other respects, cannot have health, though we think that a total exclusion of flesh, with the substitution of suitable vegetable products, would give yet better health and greater longevity. We can point to hundreds of cases where weakness and constant sickness has, after the adoption of a proper vegetable diet, given place to comparative health and perfect freedom from sickness. "A Fellow of the Royal Astronomical Society" gives an instance of greatly-increased mortality and illness resulting to some prisoners who were fed "principally on white and grey peas and lentils with bread." This is not vegetarianism: these results are only to be expected from such a concentrated highly nitrogenous diet, particularly as they were prisoners and, I presume, not doing any hard work. Many, upon making up their minds to try vegetarianism, think they must eat twice as much (many old vegetarians, however, only have two meals a day), and that, too, of the richest and most concentrated food—peas, beans, &c. As a natural consequence, they find themselves gradually getting worse. They and their friends, therefore, decry the system as a delusion. I could name some of the greatest thinkers and hardest workers who have been vegetarians.

A FELLOW OF THE CHEMICAL SOCIETY.

ANIMAL LANGUAGE.

[278]—From what I have read and seen, I have always taken it for granted that animals have languages of their own. But Arachnida evidently thinks that proofs are wanting to show that such is really the case.

Thoreau says that the language of birds may even, to a certain extent, be understood by man. His passage of the birds trying to fill up the hole in the roof (the chimney, wasn't it?) is very amusing, and at the same time full of interest to the reader.

Sir Samuel Baker, speaking of the monkeys on the banks of the Nile, says, that by watching them constantly he, by degrees, began to understand the meaning of some of their noises and signs, *i.e.*, their language. I forget if he uses the word *language*, but I think we may take it for granted that *he* thinks the animal kingdom has its various languages, just the same as we have.

If Arachnida has a cat who has a kitten, he will, by watching and listening carefully, find out that the old cat has a language by which it speaks to its kitten. For instance, I have noticed that by making a certain noise the mother will *call* its kitten to her, whereas another time she will, perhaps, make a different noise, which will be answered by the kitten, when the mother will run to the kitten, who will stay where she is—instead of running to its mother as in the former case. I have noticed this over and over again, as we have had a good many kittens, and I take an interest in watching them, and have tried them, as they have grown up, with *looking-glasses*, &c., to see the difference in their *mental* powers; some, being very sharp, finding out the deception very quickly, while others, as with us, have been stupids. But this is a digression.

I think there ought to be no doubt about the fact of an animal having a language of its own.

Your able article on "The Intelligence of Animals" has very nearly brought me round to believe in the *abstract* power of an animal to reason.—Yours &c., F.C.S.

Queries.

[228]—MICROPHONE.—Will you or any of your readers kindly give me full particulars as to the construction of a dry pile suitable for using with the microphone? Have looked up Guthrie's "Magnetism and Electricity" and Ganot's "Physics," the only works I have at hand on the subject, but although they give the material and arrangement of such a pile, they do not give the size and number of the elements.—G. B.

[229]—HAIR.—Is it possible for a person's hair to turn white *instantaneously* from fear, or other causes? If so, how is it accounted for? Have any well-authenticated cases been known?—PERCY B. DODD.

[230]—TELESCOPE.—Will "A. F. M." (letter 238) kindly inform what kind of black paint he used to darken the cartridge paper he used to make the tube of his Astro-telescope? Whether it was oil paint; and if so, of what kind? And also if he used any kind of dryer in the paint, as turpentine? And also if he can inform me whether or not he put into the tube diaphragms or not?—DALLIH.

[231]—CHEMICAL PROBLEM.—Would any reader of KNOWLEDGE inform me how the co-efficients of any chemical equation may be calculated, the full equation being given, except the co-efficients? Thus given—

$$Cu + HNO_3 = Cu\,(NO_3)_2 + H_2O + NO$$

to find the numbers, or co-efficients, 3.8.3.1.2.:—

$$3\,Cu + 8HNO_3 = 3\,Cu\,(NO_3)_2 + 4\,H_2O + 2\,NO.$$

A method appeared in the *Chemical News* some years ago, which I cannot get now.—G. H. MAPLETON.

[232]—CHEMIST.—Would it be possible for a young man, nineteen years of age, having no knowledge of it, to be able, after severe study, but at not too great an expense of money, to pass the several examinations needed to set-up as a chemist? What books would you advise to commence, and also to proceed with? The probable expense of passing? The probable time?—W. A. FYSON.

[233]—BIOLOGICAL.—Will Dr. Wilson, or any of your contributors, kindly state—(1) Whether the difference between the highest ape and the lowest man is any greater than between the lowest man and the highest man? (2) Of what organ among the lower animals is the thyroid gland believed to be a rudiment? I cannot clearly understand from Haeckel's description in "Evolution of Man." (3) Whether any instances are on record of children being born with the caudal vertebræ projecting so as to form a rudimentary tail? Also whether it is true that a race of men exists with the projecting vertebræ? If so, who are they, and where is their country? (4) What is the brain capacity, general form and appearance, and relativity to man of the Neanderthal skull? What geological formation was it found in? No works to which I have access throw much light on this subject, and a short article would be very useful.—JOHN HAMSON.

[234]—DAISIES.—Mr. Grant Allen will serve some hundreds of us lawners if he will tell us how to free our lawns from daisy beauties.—A LADY FLORIST.

[235]—ANIMALS' FOOD.—Can Prof. A. Wilson give me any fact or theory showing that all animals were at one time herbiverous? I am often inclined to think so, and that carnivorism is an acquirement.—T. R. ALLINSON.

[236]—THE POLAR SUN.—At the North Pole, how many minutes does the sun's disc occupy in rising? and along how many degrees of the horizon does the sun move in the same time?—R. W. I.

[237]—PHYSIOGRAPHY.—Can you tell me of any text-book of physiography suitable for the advanced stage of the science and art examinations? Huxley's by no means covers the whole ground indicated in the syllabus.—GRADATIM.

[238]—ELECTRIC.—What is the "co-efficient of induction" between two inductors—a term which Maxwell frequently uses but nowhere defines? Also, is there any electric force outside a galvanic circuit?—O. A. BRIDGE.

[239]—SPANISH BOTANY.—Can any of your readers tell me if there is any work on the botany of Spain similar to "Hooker's Students' Flora of the British Isles," written in either English, French, Italian, or Spanish?—T. HUCKLEBRIDGE.

[240]—MEDIEVAL.—What was the "luntes yelde," or "luntis yeld," or "luntys gylde," or "luntis yield" collected by churchwardens from the parishioners in 1505?—PHILO.

[241]—"FAURE ACCUMULATOR."—(1) Are all the tongues to point one way? (2) Which are joined together? (3) How is one cell connected with another? (4) Is there any limit to the amount a cell can hold; if so, what? (5) Ought the cells to be open or closed; if the latter, how? (6) Would five small Bunsen's cells be enough to charge it? (7) Should the lead be lifted out of the acid while not in action? (8) Is the force of the battery equal to that of the cells by which it is charged?—ECCENTRIC-CHUCK.

[212]—PROSE COMPOSITION.—Will you kindly do me the favour of saying what work you consider to be the best on English prose composition?—ANNISTAS.

[213]—AIR PUMP.—(1) I have a small air pump with one barrel. I have tried to filter mercury through a little cane filter sold for the purpose, but without success. Would some reader of KNOWLEDGE kindly give me a reason for this? Does the experiment require a stronger pump? (2). Would some reader mention a book containing experiments with the air-pump?—X. X.

[214]—LECTURES.—I wish to give a few scientific lectures to young people, and should be grateful if some one would suggest good subjects, tell me where to get information, and how to make simple instruments for experiments? Where can I get information for a lecture on "The Atmosphere," and what experiments could be made by one who is unable to buy costly apparatus?—X. X.

[215]—MOON'S BRIGHTNESS.—The sun's light striking moon more obliquely, when near new moon, one would expect brightness per unit of surface to be greater. Is this so?—C. T. B. [Our correspondent surely means less, not greater. It is theoretically less, and actually less.—ED.]

[216]—BAROMETER.—What is the mean height to mercury barometer at the equator?—C. T. B. [29·853 inches; though I should, for my own part, be disposed to doubt about the last decimal figure.—ED.]

[217]—WARMTH AT NIGHT.—I am a slight sufferer from chronic asthma, accompanied, chiefly at night, with a few attacks of coughing; and friends have urged the use of gas, conveyed through tubing to a stove filled with "asbestos," to warm the apartment. Is it conducive, or not, to one's health to maintain the warm temperature all night?—J. M. J.

Replies to Queries.

[152]—"The Art of Electro-Metallurgy," by G. Gore, LL.D., F.R.S. (Longmans) 1877. I succeed perfectly where C. T. B. fails.—W. VAN EYS.

[179]—FAURE ACCUMULATORS.—Other things being equal, the power of Faure cells are to one another as the superficial area of the plates; but by using several plates in one cell, and coupling the alternate plates together, there is some gain, inasmuch as you use both sides of the plates. It appears to me that it is erroneous to call the Faure cell an accumulator. The Faure cell is, when charged, a mere Voltaic cell; and the action that goes on in the cell, when discharging, is similar in its nature to that which takes place in an ordinary Voltaic cell. The same remark applies to all secondary batteries.—H. B. T. STRANGWAYS.

[188]—LONGEVITY OF THE TORTOISE.—The late Professor G. Pryme, of Cambridge, mentions having paid a visit at the palace of the Bishop of (I think) Peterborough, and being there introduced to a tortoise, said, by tradition, to be then 200 years old. Going to the same place some ten or twenty years later, he found that the patriarch was defunct. The passage occurs in a very interesting biography of the Professor, written by his daughter.—E. D. G.

[191]—ANIMAL LANGUAGE.—Probably animals possess means by which they express their feelings one to another, be those means movement, looks, or even articulation. But considering their social state, we have no reason to expect that the Almighty endowed them with powers of speech as the human race. The members of the brute creation, living lives almost independent of each other, while the very existence of the human race depends on the mutual help, and therefore on the mutual intercourse of its members, what language may exist between animals is of a kind vastly inferior to that between man and man. But one finds, on studying the animal world, that this language is more perfect in the higher than the lower orders, some of the latter being entirely mute; the neighing of horses and ponies, the cawing of rooks, the greeting of dogs, and the language of birds being illustrative of the former, and the lowing of kine and the silence of fishes of the latter. Apart from the language which Æsop and La Fontaine put in the mouths of animals, and the whistle which some other individual placed in the mouth or the shell of the "Oyster of Drury-lane," we may conclude that some inferior means of intercourse exist, and that to a greater extent of perfection in the higher than the lower orders.—HERBERT R. WELLER.

[192]—ICE AGE IN BRITAIN.—That an ice age or glacial epoch has existed in Great Britain, we may infer from the facts: that in some parts the eminences are all precipitous towards the west, the result of some powerful agent wearing away that side; that in the intervening valleys, boulder clay (a blue clay, in which rounded and waterworn pebbles are imbedded) is found; that deep furrows across the country and striations (scratches) on the rocks, are observable, the striations and furrows being all parallel to themselves; that all the eminences are rounded at their summits, the result of some powerful agent passing over them.—HERBERT R. WELLER.

[192]—ICE AGE IN BRITAIN.—In answer to "Arachnida," p. 255, as to what proofs exist, showing that there ever existed an ice age in Britain, reference to any geological work of any pretensions would have showed him the country fairly teems with proofs, from the Thames to Cape Wrath. The three great witnesses of glacial work are (1) the transportation of erratic blocks; (2) the smoothing and scratching of the valleys through which the glaciers travelled; and (3) the presence of arctic fossils in glacial deposits. As typical instances, Staffordshire contains erratic blocks transported from the Cambrian group; rocks from the Grampians are found 60 and 100 miles south of these mountains; while in Wicklow an immense block of granite is perched up 650 ft. above sea-level, and ten miles from the nearest granite. Valley scratchings are typically exhibited in the neighbourhood of Snowdon. Among the arctic fossils are *Trophon clathratus*, *Astarte borealis*, and *Pecten Islandicus*.—W. G. ROLLT.

[195]—QUICKSILVER NON-POISONOUS.—It may interest "F. C. S." to know that in former times liquid mercury was given in large quantities, even pounds, for obstruction of the bowels. As to its efficacy, I cannot speak. The fact is, that ordinary liquid mercury passes through the digestive tract without being absorbed, and, therefore, without producing any effect upon the system. If, however, it be reduced to a state of very fine division (as is really the case in grey powder, blue pill, and some other preparations of mercury), it is absorbed, probably because its fine state of division enables it to be easily converted into oxide.—F. W. G. [In parts of Ireland in former years a dose of small shot used to be given for a similar purpose.—ED.]

[195]—QUICKSILVER NON-POISONOUS.—For the sake of F.C.S., I quote the following words from "Taylor on Poisons":—"Although liquid mercury is not in itself poisonous, it is liable to be converted into poisonous compounds in the body," p. 360, 3rd Edition.—ROBERT MACPHERSON.

[197]—Messrs. J. & E. Hall's last designs are by far the best in the market, not only for efficiency, but on account of their simplicity and the small space they occupy.—GAY.

[200]—LEASES.—"James Gregg" should consult an actuary, or study Sir Isaac Newton's "Tables on the Value of Leases," or the modern "Tables" of Inwood, or both of these authorities.

[200]—LEASES.—To determine the value of a lease, even when the conditions *are* specified, is not always an easy matter. To the following seemingly simple problem, I have received a score of answers, all varying. I would be glad of an authoritative solution. A. pays for a fourteen years' lease, £1,050; the rent for the first ten years is to be £250, and £300 for the last four. At the end of ten years he agrees to sell the lease to B. at a proportionate price to what it cost him. What is the amount B. must pay A.?—W. CAHILL.

[201]—MINIMA OF ALGOL.—G. M. T.

	H. M.		H. M.		H. M.
Feb. 16,	4 33 a.m.	March 11,	3 1 a.m.	April 3,	1 13 a.m.
,, 19,	1 22 a.m.	,, 13,	11 53 p.m.	,, 5,	10 21 p.m.
,, 21,	10 10 p.m.	,, 16,	8 42 p.m.	,, 23,	3 17 a.m.
,, 24,	6 59 p.m.			,, 25,	0 6 a.m.
				,, 28,	8 55 p.m.

Professor Pickering's observations, however, at Cambridge, U.S., show that in 1880 the true time of minimum preceded that of the ephemeris by about 37 minutes. "L." would do well, therefore, to look out for the minima about three-quarters of an hour before the times given above. Algol begins to diminish about 4 hours before the actual minimum, and does not regain its full lustre till 5½ hours after the epoch of faintest light. Curiously enough, Algol has a companion at about 82″ distance, which is also variable, but in some long period. It would take up too much space to answer "L.'s" second query adequately. An east wind has sometimes a very curious effect, rendering the discs of bright stars triangular, or causing them to be apparently accompanied by a faint, close companion. I found the latter effect very markedly visible on two occasions in the early part of 1874. Webb had noticed the same thing. Perhaps the Editor would say if he has ever heard of the "triangular" effect having been noticed in America.—H. SADLER.

[206]—OPIUM.—In chronic lead poisoning, the constipation, which is usually very obstinate, is due to a tonic, *e.g.*, a continued contraction of the muscular coats of the small intestine. If opium were administered in such a case, it would act as a purgative, by overcoming this spasm.—ROBERT MACPHERSON.

[207]—HEAT.—The hand can be put into molten iron. The perspiration induced by fear provides a cushion of vapour, as in spheroidal state. If tried too often the experimenter has got

burnt. Wetting with ammonia acts better. A ring worn has caused burning.—C. T. B.

[210]—HOMER.—Both the "Iliad" and the "Odyssey" are published in prose in Bohn's Classical Library, price 5s. each. C. J. C.

[213]—ORGANIC COMPOUNDS.—"Siqnis" should read Berthelot's "Chimie Organique fondée sur la Synthèse," Paris, 1860. See also "Indigo," &c., in last number of KNOWLEDGE. Alizarin (chief colouring matter of madder) got by Graebe and Liebermann from anthracene. This can be built up from its elements. If "S." will write, will gladly show how.—C. T. B.

[218] Tennyson. "In Memoriam," poem 87

"And over those ethereal eyes
The bar of Michael Angelo."

Whose brow was straight and prominent, the sign of intellectual power. *Ibid*, poem 91:—

"The sea-blue bird of March,"

The kingfisher, which like other birds, puts on its best plumage in early spring.—*Vide* "Key" to "In Memoriam," by Alfred Gatty. D.D.—P.B.F.

[218] "The bar of Michael Angelo" refers to the peculiar contraction of the forehead, forming a wrinkle, seen in the old portraits of Michael Angelo. Arthur Henry Hallam, referred to in the quotation, had this same mark on his brow.—JOHN CRAIG, JUN.

[218] "SEA-BLUE BIRD OF MARCH."—Kingfisher ([illegible]).

"These fields made golden with the flower of March,
The throstle singing in the feathered larch.
* * * *
And down the river, like a flame of blue,
Keen as an arrow flies the water-king." —OUTIS.

[219] THE ATOMIC THEORY. Daubeny's "Introduction to the Atomic Theory," Oxford, 1850, is the best English work. There is no theory to explain insolubility: bodies of similar chemical constitution dissolve each other. C. T. B.

[220]—CHEMICAL ANALYSIS.—The most complete book on Qualitative Analysis is by C. R. Fresenius, translated into English, and published by Churchill, at 12s. 6d. The methods, however, are longer, and take up more time than most students can spare, but they are the best and most trustworthy. A very good book on the same subject is "Practical Chemistry," by Jones (Macmillan, 2s. 6d.). In "Quantitative Analysis" (inorganic), by far the best and most complete is Fresenius's (Churchill, 15s.): the large number of methods and quantity of matter is, however, confusing to the beginner, unless under the guidance of a teacher. On the same subject, Thorpe's is very good (Longmans, 4s. 6d.). If agricultural chemistry is wanted, there is Church's Laboratory Guide (Van Voorst, 7s. 6d.); the first part is qualitative, the second quantitative. The standard work on Volumetric Analysis is Sutton's (Churchill, 15s.). On Commercial Organic Analysis we have Allen's, of which only the first vol. is published (Churchill, about 15s.). "Practical Chemistry," by Blyth, is an excellent work on foods, drinks, and toxicology (Griffin, about 12s.). Wanklyn has written separate small treatises on the analysis of water, milk, tea, coffee, and cocoa (Trubner); "Water Analysis"—Frankland (Van Voorst); "Portable Water" — Ekin (Churchill); "Butter" — Hehner (Churchill, 3s. 6d.); "Commercial Handbook of Chemical Analysis," by Normandy, is in dictionary form (Lockwood); "Select Methods of Analysis, by Crookes (Longmans, 12s. 6d.); "On Microscopic Analysis of Foods," Hassell's is the best (about 21s.). There are a number of important articles on food analysis in the *Analyst*, a monthly magazine (Baillière), which every food analyst should see. Do not confine your attention to any one book on water analysis; it is necessary to read Wanklyn's, but analysts do not follow it throughout. There are so many works on qualitative analysis that it is difficult to say what are the best.—A FELLOW OF THE CHEMICAL SOCIETY.

[229]—MORTALITY FROM CANCER.—H. A. Everest will find in Haviland's work on "The Geographical Distribution of Heart Disease, Cancer, and Phthisis, in England and Wales," illustrated by coloured maps, the facts proving that cancer is influenced by locality. The above work was published in folio in 1875. It is now out of print, but can be obtained at second-hand booksellers, and seen at the Library of the British Museum.

[Letter 220]—THE HEALTH OF NAVVIES.—In No. 12 of KNOWLEDGE, I find betel-nut chewing given as a specific against fevers. The Javanese and Sundanese are inveterate betel chewers, and yet they have been dying literally by thousands during the past few years from fever. When I left Samarang, Java, in September last, the natives were dying by hundreds of fever. My experience, extending over nearly four years in various parts of Asia, has been, that the natives who universally chew betel (with lime, and the leaf of the sirih-pepper-plant), are much more easily affected by fever than are Europeans.—EDWIN SACHS.

Answers to Correspondents.

**** All communications for the Editor requiring early attention should reach the Office on or before the Saturday preceding the current issue of* KNOWLEDGE, *the increasing circulation of which compels us to go to press early in the week.*

HINTS TO CORRESPONDENTS. *1. No questions asking for scientific information can be answered through the post. 2. Letters sent to the Editor for correspondents cannot be forwarded; nor can the names or addresses of correspondents be given in answer to private inquiries. 3. No queries or replies savouring of the nature of advertisements can be inserted. 4. Letters, queries, and replies are inserted, unless contrary to Rule 3, free of charge. 5. Correspondents should write on one side only of the paper, and put drawings on a separate leaf. 6. Each letter, query, or reply should have a title, and in replying to letters or queries, reference should be made to the number of letter or query, the page on which it appears, and its title.*

S. S. G. See Dr. Ball's paper in our next. C. RONALD W. Kindly put query in concise form. WALTER W. It would save much trouble if you would either put your queries in form, with heading, &c., or head your letter properly. Does moonlight really make the planets and their satellites more distinct? That three-inch is a splendid telescope. Having seen Rigel, as described in "Half-hours with Telescope," know that it can be so seen. Possibly the objects you refer to were observed under unfavourable conditions. A new edition of Webb's "Celestial Objects for Common Telescopes" has been published lately. JOSEPH DAVIES. There is Rodwell's "Science Dictionary," Moxon's, price 10s. 6d., I believe.—E. W. Propose to re-write the articles on the "Differential Calculus," with sundry improvements which have occurred to me. ZARES. When did we say that a tangentially (horizontally) moving mass has no energy? If a fly pushed against a mass a ton in weight so suspended as to be perfectly free to move in the direction in which the fly pushed, he would communicate to the mass just so much momentum as corresponded with the force wherewith he had pushed it. Or conceive half a ton of matter connected with another half ton by a strong but weightless cord passing without friction over a pulley, and let a fly light on one of them. Let his weight be one-ten-millionth of a ton. Then the weight on which he lighted would immediately begin to descend, the accelerating force being one ten-millionth part of gravity. In 10,000,000 seconds, or 115 days, 17 hours, $6\frac{2}{3}$ minutes, the velocity communicated would be 32 feet per second—that is, the same velocity which gravity communicates to a falling body in one second. It would take rather more than $3\frac{1}{2}$ days to communicate a velocity of one foot per second. As to your offer of £5 prize to determine what force would overcome the inertia of a pound of iron, we are much obliged to you, but must decline. We repeat your other query; we might safely offer a prize ourselves to any correspondent who can understand what you mean. You require "the summation of the infinite number of infinitesimal differences between 0 and 1, of which 0·5 is the intermediary or $\frac{1}{2}$." The sum of the differences would simply be 1. You do not mean that, however, for your original query asked for the sum of the series 0 to 1, when the number of terms is infinite. *That* sum, if there were such a series, would be infinite. You say I "dare not say" two ships unequal in mass, moving with equal velocity, could both be stopped with the same resistance: I dare say not. F. BLAKE. Your method already given. But for the present we have done with magic squares.—GRANVILLE SHARP. You are more sharp than generous or reasonable. Before you spoke of my "foolish utterances" in the *Spectator*, you should have made sure they were mine. As it chances, they are not, nor do they in the slightest degree represent my views. Seeing how rashly you rush to conclusions, I am almost pained to think you ever thought well of my work. How sad to think that your past liking may have been as ill-founded as your present disfavour!—H. A. B. Statement insufficiently exact; you say nothing of size of building and height; you do not define your "very near."—G. E. R. Science knows of no way of inflicting pain on persons at a distance, "notwithstanding such obstacles as closed doors and windows, without any visible means." The stories about such action at a distance are generally thought to illustrate the influence of imagination.—F. E. B. The writer of "Brain Troubles" has some singular experiences to relate respecting music in the ears; the phenomenon is unquestionably subjective. O. DAWSON. Pray define *inferiority* (in the Man *versus* Woman question), *man* (in the Descent debate), *love* (in the Phrenology discussion), and *justice*—in your own way. If you had any conception of the value of time, you would understand what our definition of *injustice* might be. We have no time for hair-splitting, and it is unjust to expect us to follow you in yours. We could find space for a short letter giving your definitions, and commenting (at reasonable length only) upon what you regard as the inexactness of others. CLERICUS. We receive letters from America which are printed by some new form of type-writer, probably the one to which you refer. Perhaps some of our readers can give us information about the new, cheap

(beginners') type-writer recently brought out in America. E. S. Dr. Ball (whom we have invited to write for us on the subject and who has kindly done so) is quite right; the energy of a body in raising tidal waves varies directly as the mass, and inversely as the cube of the distance; that is why the lunar tide exceeds that raised by the sun (which, were the ratio that of the inverse square, would be the greater). W. P. Thanks; we have touched on the point in our article on "Fallacies," now in type. Edmund Hunt. Before we go to the expense of engraving your diagram, we must have some idea of the nature of the relations you propose to deal with. As it stands, it looks like "a beast." We give it without a figure, and in our own words, in the Mathematical Column; but we should not care to attack it unless we saw our way to some useful result. H. A. Bulley. Let us rest, after magic squares, before we begin on magic circles. W. R. Many thanks; your algebraical solution very neat. A. N. Somers-Vates. Thanks; but the projection not likely to interest many, and space runs short. The polar aspect of the heavens appears — A. T. C. When we wrote "any rectangle," we meant, as usual, that the solution must be applicable to any rectangle of whatever dimensions, not that you might take a rectangle of any particular dimensions which seemed most convenient. The question related to I.H.S., put on tombstones now, not to the ancient inscription in Greek letters, IHΣ. C. J. C. May shortly describe a very simple instrument, by which the place of Venus in the day-time may be found.—E. D. G. Quite unable to find place for what you rightly describe as a flood of notes. Brown-Sequard was the name of the physiologist you refer to; but he did not write the article; he was quoted in it among other authorities. I wrote the article myself. The difficulty about recommending books is that the practice is open to abuse. — J. Whitley. Depends what size field you require. It is impossible to answer questions so vague.—R. Staveley. Thanks. But you get the wrong equation to escribed circle. The negative sign under the first radical is incorrect.—W. G. Rolfe. Astronomers are not at issue with geologists as to the earth's interior volcanic ashes carried by winds. In my article on earth-born meteors, I have shown how meteors sent beyond the earth's control would still travel on paths intersecting the earth's orbit.—A. M. K. T. You are quite right. I have written repeatedly to show that the only danger was for the comet, which, in such an encounter, would be like the "coo" of the elder Stephenson. —A. Jellivan. Never heard of any astronomer so named, but Prof. Pritchard may be writing about the stars.—R. E. J. That is just what I did take into account. If facing a windmill, the left arm *always* goes down, it follows that if the windmill faces the north, and you, facing it, look southwards, the eastern arm goes down. On the other hand, if you look northwards in facing it, the western arm does down. Why the left arm should be made to go down and the right arm up, instead of the reverse holding, I do not know.—Philo. Brackets are far too useful to be limited to the use to which Whately put them.—G. B. Thanks, but first set of hogs sufficiently attended to.—A. Gaubert. The Chinaman said he was a member of a class of trained computers, and not one of the most skilful. G. M. T. Bates. It seems as though, since there are 462 combinations of six girls, and each set of five out of the six appears six times — viz., with each of the remaining six of the eleven, there must be one-sixth of 462 sets fulfilling the conditions, or seventy-seven.—G. M. If you wait till I prophesy, you will have to wait more than fifteen years.—Winter. The time of Venus's brilliancy is to be calculated, not taken from the "Nautical Almanack." You have not got the answer quite right to the watch question. The watch loses five minutes in a day of twenty-four hours, and the interval from noon at one place, to noon at a place 1¼° due west of it, is more than one day.—J.C.L., G.F.H., Geo. Brown, Araitchens, C.T.B. and others. Thanks for notes on the quicksilver matter.—Insomnolens. Your night-watching calculation of the sun's restraining force on earth, measured by tensions of sun-attached wires, is excellent. Wish we could find room for it. There is no doubt now on the general subject of light. Sound ceases to be perceptible when the vibrations are very rapid, and a very wide gulf separates the most rapidly vibrating sound-waves from the most slowly vibrating light-waves. The waves are also different in kind. To say truth, one might almost as well ask, Why, since water-waves rock ships, do not sound-waves also rock them? as your question, Why, since vibrations of a certain velocity produce sound, light-waves are not also heard?—Hesketh. A theodolite would not help you much to find a star or planet from the R.A. and Dec. in the *Nautical Almanac*. You can calculate by spherical trigonometry the altitude and azimuth for a given epoch; but a fresh calculation would be wanted after the epoch was past. The best way is to use an equatorially-mounted telescope. Of course, if the object is conspicuous, you can mark down its place in a star chart, and then readily identify it in the constellation wherein it is. Scientiæ Amicus. Query why birds scratch their heads, rather too suggestive of Lord Dundreary's "Why doth a dog waggle hith tail?" Churchman. That is just one of the questions we do not wish asked of the students of science to whom they refer. What they think of the origin of man is clear. If they think this inconsistent with other views, we do not wish them to say so; therefore, we do not wish others to ask them if they think so. Jas. Grundy. When a person who has had small-pox, or has been vaccinated, is again attacked, it is not the revival of the former disease germs which is in question, but the introduction (in some way) of new germs into the system. Vaccination is supposed to act as a perfect protective for about seven years. Germs, however, may lie dormant for years, as we see in such cases of infection as are discussed in Tyndall's book on "Dust and Disease."—Ursa Major. The rule is — number of feet fallen in t seconds, equals 16 1 × square of t; so that in 6 seconds a body falls through 6 times 36, or 216 feet.—J. Sharp. Observations such as you cite are not of sufficient exactness to admit of scientific discussion. It is no proof of spontaneous generation to ask, If such and such living creatures were not evolved in the places where they are found, whence came they. In many cases, where the origin seemed more perplexing than in those you cite, Pasteur and others have traced the complete chain of linked existences.—Gravity. The difference in the range would not be so great as the parabolic theory would suggest, the resistance of the air greatly affecting the range; but there would be a decided difference of range. If both guns were fired at the same time, then, when the lower projectile had descended to the ground, the upper would have attained as great a horizontal distance, and be still 100 ft. from the ground; it would not be descending vertically, the range continuing to increase, though more and more slowly, till this projectile in turn reached the ground.—A. E. S. The question was answered on page facing the answer to other A. E. S. —J. A. S. B. Too big a question. All perturbations have to be considered, besides mathematical relations of a complex kind.—H. R. Weller. Thanks, but room for only one answer to the question.—C. E. H. By a *sine quâ non* is signified something without which a certain course will not be followed; the words mean "without which not."—I. J. Simpkin. Thanks for the corrections. The article was too technical for the "reader" and for our readers. It was sent to the printers by mistake for another.—Rev. W. J. W. Many thanks.—Constans. Nay, the poker, if—as usual—cold, in the first instance will do the reverse of what you suggest. If finally it gets warm, its warmth has been taken from the fire which it is supposed to nourish. In the other case, we know why less light reaches the eye in full sunlight. Stay in the dark awhile, with a small mirror in your hand, facing the blind which darkens the room, and while a friend draws up the blind, look at the pupils of your eyes as seen in the glass, and you will see the reason clearly enough. Can see no reason why with spring tides the sky should always become overcast; though, of course, there is then a wider water surface for evaporation.—John Sanders. The points of light are simply the images of Jupiter itself seen after double reflection at the front of the glass as well as the back. Children have been suffocated by cats sleeping over their face, not by cats drawing their breath. The secret of such criticism as that in the *Manchester Sporting Chronicle* is to be found in my condemnation of sporting rascaldom, not in my predictions of unpleasant things. It is very well known I have predicted only such things as meteoric showers, &c., which have usually occurred as predicted —unlike what sporting prophets predict; praise from sporting prophets would be like the contempt of honest folk.—J. P. Sandlands. Surely you are a little unreasonable. Certain readers ask for intermediate forms, saying that if there is descent, such forms should exist. Dr. Wilson describes some. You then say they do not prove descent; and I reply, naturally, that he wrote in response to those who thought such evidence as Dr. Wilson supplied essential to the development theory, and not for one who, like yourself, considered that it proved nothing. Now you say that Knowledge, being intended for those who want information, contributors should write for those who, like you, want information. Are we, then, to answer no questions until we are assured that every reader of Knowledge wants the information asked for by some of them? It would interest me to know how my article on "Fallacies about Luck" touched on religious questions. In one sense, everything almost that could be said here might be regarded as touching on religion. The statement that two and two make four, involving, as it does the inference that two and one make three, might be regarded as verging on a reference to the diverse doctrines held by Trinitarians and Unitarians; remarks on Brain Troubles might be considered to refer, more or less directly, to theological interpretations of "possession by spirits;" Dr. Carpenter's discussion of Food questions might be held inconsistent with the Bible narrative of the basketsful of fish; and Mr. Foster's articles on Illusions might be

regarded as intended to explain away miracles. We expect fairer treatment than this, however, from our readers, whatever their religious views may be. The word "Simian," when referring to the species, always has a capital. As for "extraordinary," *does* it mean extra-ordinary in your sense—that is, exceptionally ordinary? Always thought it meant something outside of what is ordinary.—W. Greenwood. If the air in bladder is much compressed, there will be a slight excess of weight; otherwise none, unless weighed in vacuo.—F. Cowley. You are right in saying that we do not see a star where it actually is, if the star is in motion, though, owing to the much more rapid motion of light than of any celestial orb, we always see a star very near (apparently) to its true place. But you are quite mistaken in supposing that owing to the earth's rotation, the star's true place might be in the southern celestial hemisphere when we see the star in our northern skies. The earth's rotation has nothing to do with the matter—at least nothing in the way imagined. Compare the earth to a twirling globe in a room, against which a number of small shot are shot from a distance. The globe may have made several twirls while the shot were travelling, and the side turned towards the gun when it was fired may be on the opposite side when the shot arrives, but the shot will reach the side which is at the moment of arrival towards the gun: and, in like manner, rays from a star reach that side of earth which is towards the star, so that rays coming from a star really in the southern celestial hemisphere cannot possibly reach a part of the earth turned towards the northern celestial hemisphere. In other words, wherever a star's rays reach the earth (that is, wherever the observer may be), they will seem to come from the direction in which the star lies, apart from the slight corrections due to aberration, &c. The suggestion that expensive telescopes and microscopes might be let out on the three years' system seems worth considering.—F. F. Read article by "Fellow of Astronomical Society," in No. 10.—B. Riley. Your method of computing the moon's distance from the force of gravity, as calculated for the moon, combined with moon's known period, is simply working Newton's problem backwards. It is indicated at p. 21-2 of my Treatise on the Moon.—T. A. The lecturer was quite right, so far as astronomy teaches. I have for several years given a lecture bearing the same title and treating the subject in the same way. Matter may be infinite, but it does not follow that each sun in space can draw to himself an infinite quantity.—Vice-Admiral, F. A., &c. Thanks. The efficacy of oil as a sea-calmer has been warmly advocated by Mr. W. Chambers, in *Chambers' Journal*. Believe the matter still remains in doubt. Have not room for the long extracts sent both by F. A. and Vice-Admiral.—J. J. Your method already considered in the earlier numbers of Knowledge.—Farmer Will. Sorry to hear you say you have a "fossilised mind," but your belief in Noah is scarcely of scientific importance. Permit me to quote one sentence of your letter:—"Before Noah's time we read that there was a certain tree" the taste of whose fruit brought trouble. "In abusing knowledge we eat of that fruit." Will adverse critics, if such there are, remember?—J. H. H. I can answer from my own observation that all the objects you mention can be seen with the smaller apertures, powers, &c. Probably the faintness of green or blue stars may be due to the quality of your glass. The diffraction rings not being complete circles does not indicate very serious defect. Wish your letter were a little more compact, or your queries put separately, in proper query form.—B. M., F.R.C.S. You write under evident misapprehension as to space at our command. But thanks all the same. Surely the Osborne sea-serpent case did not occur so many as "eight or nine years" ago. Which questions would better suit the *Lancet*. We do not at all want medical questions; but unless you call the quicksilver question medical, none such have appeared.—Arthur Twort. Light from each point in the small triangular space gives circular image of sun, and these overlapping, combine into a single circular image, in which the triangularity of small aperture is lost.—A. J. Martin. It was a slip of the pen on the part of "Five of Clubs," which he corrected in the next number.—E. J. Wilson. Do not know any better work on trigonometry than Todhunter's.—J. A. Crawley. Thanks for quotation from Tacitus, showing that Tiberius was of the same mind as author of "Brain Troubles": "Solitusque eludere medicorum artes atque eos qui post tricesimum ætatis annum ad internoscenda corpori suo utilia vel noxia alieni consilii indigerent."—E. D. G. Thanks for numerous replies and notes. If Knowledge could but be expanded weekly to 48 pp. or so! Replies not mentioning number of query are useless to us.—J. H. Garfit. Fear I can only say the article was too long. When any circumstance assures us that an article will not suit, our attention is naturally turned at once to "the next article."—J. Rae. Whoever reaches the Pole will probably have to winter there. For him there are many ways.—J. J. Henderson wants titles of books on chemical analysis of alcoholic stimulants.—J. W. C. That would be right, if you have correctly determined focal length of the two glasses. But a 2-inch aperture would not readily bear such a power.—W. Baxter. Ferguson's tables no longer of any value. Try Johnson's book on eclipses.—Gradatim. The paradox comes in before the Pole is reached. The time is finite, but the number of convolutions infinite; how, then, can the particle be said to reach the Pole (along what course, I mean)? Thanks for other matter, but no space except for query.—R. W. Dr. Brewer's explanations amusing. "Why does sun put out fire? Because the chemical action of sun's rays is detrimental to combustion! Why does a poker across a fire revive it? Because the poker concentrates the heat!" Just so. This sort of science-teaching might go on for ever. As thus, why do the planets travel in ellipses? Because the tendencies of planetary motion are elliptical. Why do comets bring disaster to nations? Because of the disastrous tendencies of cometic apparitions.—A Student No. 1. You ask us to reconcile a statement made by Dr Ball with another made by the Astronomer Royal for Ireland. Dr. Ball says in one lecture, there is no water in the moon; the Astronomer Royal for Ireland, in another, says tidal waves checked the moon's rotation. But Dr. Ball said in the latter lecture that such waves would exist whether there was water or not in the moon; and in the former lecture the Astronomer Royal for Ireland said that formerly there may have been water in the moon; while finally you are not to suppose that Dr. Ball says one thing as Dr. Ball and another as Astronomer Royal for Ireland. This you will have opportunity of recognising shortly in an article which he has written at our request for Knowledge.—A Student No. 2. Stimulants certainly not good; the other matters depend on the health, strength, and constitution of the student.—B. Your vernier reads more closely by being so divided; adding the extra divisions is equivalent to bisecting the divisions on the limb.—G. G. D. Measured from true noon, the change is equal on either side, but mean noon slightly differs from true noon. Read any text-book account of equation of time. Fully answered in ordinary explanations of the calendar. F. H. R. Edinburgh is so often pronounced Edinburg, that custom may be regarded as at least justifying the practice. In Scotland I have seldom heard it, but always Edinbro', among the less educated often Embro'.—W. Wilson. Cannot see how a scientific theory can depend on the merely verbal questions you raise. I would rather, for my own part, say, "I see the house," than, as you suggest, "I see the vibrating ether; or, I have received on my consciousness, through the retina, the optic nerves, and the brain, an impression by vibrating ether," indicating the existence of a house, &c.—Gerald Massey.—The motion of the apsides does not affect the precessional period. It shortens the interval between the epochs when spring, or any other fixed seasonal point, coincides with perihelion or aphelion, but the spring equinox makes the circuit of the ecliptic in the period of about 25,870 years mentioned in books on astronomy.—J. A. Dobson mentions that Drew's Geometrical Conics is sufficient for a first class in fourth stage at Kensington.—M. H. P. The dream theory of your lecturer has no scientific basis.

Letters Received.

T. V. H., A. Aitken, Acacy, S. de M., J. B. Dimbleby, E. M., J. C. H., F. W. Beckett, J. R. H., J. J. M., Isaac Isaacs, Cosmos, Charles Gray, X., Amiens, G. A. L., J. A. Miles, Magic Squares, Student (not A Student), Vega.

Notes on Art and Science.

An Electrical Stature Alarm.—A curious application of electricity is described in *La Lumière Electrique*. It consists in a device to prevent military conscripts practising frauds as to their stature by bending their knees. When the youth stands erect against the measuring post, the hind parts of the knees press on electric contacts, causing two bells to ring; the ringing ceases when there is the least bending. The sliding bar which furnishes the measure has also a contact, which is pressed by the head, whereby a third electric bell is affected. For a correct measurement, the three bells should ring simultaneously. This system, the invention of M. Cazala, is now employed in the Spanish army.

Capacities of Lungs.—Dr. Nagorsky, having measured the capacities of lungs of 630 boys and 314 girls in the schools of the district of St. Petersburg, now publishes the results of his investigation in a Russian medical paper, the *Surgeon*. He has found that the capacity of lungs, in relation to the weight of the body, is 65 cubic centimètres for each kilogramme of weight in boys, and 57 cubic centimètres for girls. The law of Quetelet being that, with children below fifteen years of age, the weight of the body is proportionate to the square of the height, Dr. Nagorsky has

found that it is proportional to 2·15 of the same, while the capacity of lungs is proportional to 2·4 of the height for boys, and to the square of the height for girls. Dr. Nagorsky's researches will soon be published in a separate work. As to the relation between the weight of man and the capacity of lungs, it is tolerably permanent, and its variations are mostly due to differences in the amount of fat in the bodies of different men.

Explosion of Aqua Ammonia.—The *Pharmaceutical J*[illegible] records a recent case of an explosion of ordinary liquor ammoniæ followed by serious results. A Belfast woman, subject to headache, sent her daughter to the druggist to purchase a small quantity of "smelling salts," for which he gave her liquor ammoniæ, or "spirit of hartshorn," instead of the salt, carbonate of ammonia. The vial was put on a shelf and not used for a few days. Having a headache, the woman lifted the remedy to apply it, and had it in her hand for a few minutes only when the vial suddenly exploded, scattering the contents over her face. Her eye was destroyed, and her mouth and throat burned, the skin of both having been torn off. The vial had been put on the mantelpiece previous to the time it was used, and when about to apply the contents the woman was sitting near the fire.

A new work, by Mr. Richard Meade, Assistant Keeper of Mining Records, entitled "The Coal and Iron Industries of the United Kingdom," will be issued about the 15th inst., by Messrs. Crosby, Lockwood, & Co. Besides a description of the coal-fields and the principal seams of coal, Mr. Meade's book will include an account of the occurrence of iron ores in veins and seams, and a history of the rise and progress of pig-iron manufacture since the year 1740. Maps illustrating the position of coal-fields and iron-stone deposits throughout the kingdom will accompany the work. Messrs. Crosby, Lockwood, & Co., will also issue, during the month of February, a new work, by Mr. Lowis D'A. Jackson, author of "Hydraulic Manual and Statistics," entitled "Modern Metrology." This manual will treat of the metrical units and systems of the present century, and will include an appendix containing a proposed English system. The book will, we understand, be dedicated, by permission, to the Right Hon. W. E. Gladstone.

Our Mathematical Column.

MATHEMATICAL QUERIES.

[29]—An ellipse has semi-diameters DB, DF ($DB=2DF$). From D, DC is drawn, making an angle $\cot^{-1}2$ with DB (on the same side as DF), and DA making angle ADC=angle CDB. Make $DA=DB$, and draw ACE. From AD cut off AE=one-fourth AD. With centre E describe circle AGH, cutting elliptic quadrant AF in H and G (G nearer to F). Draw GI perp. to BD. Let AI cut DC in J. With centre B and radius CJ describe circle cutting CD in K, and with centre C describe circular arc KL, cutting KB in L. It is required to determine geometrically the ratio of the arc KL to the straight line CB.—Edmund Hunt.

[30]—Simultaneous Equations.—

$$x^2+y=11$$
$$y^2+x=7$$

—Thomas Fawcett.

[This equation can readily be solved, so far as finding the obvious roots is concerned; on the other hand, if a and b be written for 11 and 7, the equation cannot be reduced to a quadratic. We leave the equation in the above form, as an exercise for the young reader. Ed.]

[31]—Equation.—

$$x^4+4x^3=27.$$

—W. H. B.

[32]—How to Analyse a Curve?—A curved object has to be reproduced in different sizes. A tracing of its curve has been made on paper. How can an analysis and definition of the curve most easily be arrived at? A base line has been drawn on the convex side of the curve and offsets taken to the curve as noted below:—

Base line	0	·15	·25	·35	·6	·7	·85
Offsets	2·3	1·7	1·5	1·3	1·	·9	·8
Base line	1·0	1·2	1·45	1·75	1·95	2·1	
Offsets	·7	·6	·5	·4	·35	·35	
Base line	2·4	2·65	3·0	3·5	4·1		
Offsets	2·25	·2	·15	·1	·05		

The measurements being in inches and decimals, can the law or laws of the course of the curve be deduced from these figures? If yes, how?—Fred. W. Foster.

[33]—Bisect a triangle by a line drawn from a given point outside the triangle.—J. A. Dobson.

[34]—Eleven school-girls went for a walk every day, and were each offered a bouquet every day so long as a different six presented themselves to the giver, and provided also that no *five* girls ever found themselves in the same group twice. On how many days could they so arrange themselves?—G. M. T. Bates.

[22]—The equations are—

$$(a)\qquad \frac{x}{a}+\frac{y}{b}=\frac{a}{x}+\frac{b}{y}=a+b$$

($x=a$, $y=b$, obviously simultaneous roots.)

$$(b)\qquad 2\ldots+a^2+2\sqrt{\ldots}+b^2=a^2-x^2$$

May I suggest another astronomical problem? A lunation is 29·53059 days, and period of the sideral revolution of the moon's node is 6793·5 days; show that after 14558 days eclipses may be expected to recur in an invariable order.—Rutvierte.

[22a]—Correcting the obvious misprints, we have

$$\frac{x}{a}+\frac{y}{b}=a+b\ \ldots\ldots\ldots\ (\text{i})$$
$$\frac{a^2}{x}+\frac{b^2}{y}=a+b\ \ldots\ldots\ldots\ (\text{ii})$$

One solution is obviously $x=a$, $y=b$. But proceeding *secundum artem*, *i.e.*, finding value of x from (ii), substituting in (i), and reducing, we get

$$(a+b)y^4-2b^2y^3-3ab^2y^2+2b^3(a+b)y-b^5=0$$

As $y=b$ is a known root, it follows that $y-b$ is a factor of this equation. Dividing, then, by $y-b$,

$$(a+b)\,y^3+(ab-b^2)\,y^2-(2ab^2+b^3)\,y+b^4=0.$$

On trial, $y-b$ is found to be a factor of the new equation, and we get $(a+b)\,y^2+2aby-b^3=0$, a quadratic.

whence $$y=\frac{b}{a+b}(-a\pm\sqrt{a^2+ab+b^2}).$$

besides the previously obtained values b, and b.

Then, by symmetry, the values of x are

$$a,\text{ and }a,\text{ and }\frac{a}{a+b}(-b\mp\sqrt{a^2+ab+b^2})$$

F. J. Butt.

I should be glad of a better solution of the following than I have been able to devise.

$$x^2=\frac{39}{y}-\frac{14}{x}$$
$$y^2=\frac{42}{x}-\frac{13}{y}$$

Science and Art Department, May, 1881.

In my hands it becomes, by substitution, from (i) in (ii) $x^9-81x^6+1623x^3-21952=0$, which, treated as a cubic, and solved by Cardan's rule, gives $x=4$, and $y=2$, besides imaginary values. But I think there must be a shorter cut.—F. J. Butt.

[$x^2+3y^2=\frac{112}{x}$, $3x^2+y^2=\frac{104}{y}$; whence $(x+y)^3=216$, $x+y=6$. Then $3(6-y)^2+y^2=\frac{104}{y}$; $3(6-y)^2y+y^3=104$, giving $(y-3)^3=-1$; $y=2$; $x=4$, with imaginary roots.—Ed.]

[28]

$$\frac{a+x}{\sqrt a+\sqrt{a+x}}+\frac{a-x}{\sqrt a-\sqrt{a-x}}=\sqrt a$$

rationalise denom., and we get

$$(a+x)^{\frac32}+(a-x)^{\frac32}=3x\sqrt a$$

square $\qquad (a+x)^3+(a-x)^3+2(a^2-x^2)^{\frac32}=9ax^2$

i.e. $\qquad 2a^3-3ax^2=-2(a^2-x^2)^{\frac32}$

square $\qquad 4a^6+9a^2x^4-12a^4x^2=4a^6-12a^4x^2+12a^2x^4-4x^6$

i.e. $\qquad 4x^6=3a^2x^4$, from which we get

$$x=0,\text{ or }\pm\sqrt{\frac{3}{2}}\,a$$

W. N. W.

[Similarly solved by Yarletonian and others.]

Our Whist Column.

THE GAME IN No. XIII.

Your correspondent, "Five of Clubs," in his observations on Z's play (p. 284), approves of Z's holding up the turn-up card, and playing a higher one to the adversary's lead of trumps because he thereby gives information to his partner. Now I submit that on an adverse lead of trumps, the right rule is to give the ad-

versaries as little information as possible as to the number one holds, and for that reason one ought, as a rule, to play the turn-up card, when a small one, as soon as possible. Giving information in the case of an adverse lead of trumps is so, it seems to me, sacrificing the real object in play, viz., trick getting to the principle of giving information, which is only a means to an end. But this is a result to which the teaching of those who make this principle their hobby, and assert it to be the basis of modern scientific Whist, necessarily leads. The principle of giving information of strength to a partner is as old as the game itself, and is sound, because, as a rule, it is information by which a partner can gain more advantage than the adversaries, but I am sure that to tell the table of one's weakness is giving information of which the adversaries can avail themselves more to one's detriment, than a partner can to one's advantage. If anyone doubts it, let him play dummy. If dummy's partner, he will find how much less use it is to him to know dummy's cards when dummy is weak, than when he is strong. If dummy's adversary, he will, on the contrary, find that he can take much more advantage of knowing dummy's weakness than he can of his strength. So that, judging by experience and by reasoning I believe that the old rule of trying to conceal weakness, even at the risk of deceiving your partner as well as your adversary, is sound. I dwell upon this point, because it is one on which I consider "Five of Clubs" essentially wrong. He, at page 42, says in effect that there is *no limit* to the principle "that it is more important to inform your partner than to deceive your adversary." If this be true, *A* and *B* playing against *Y* and *Z* would gain an advantage by being allowed to expose their hands on the table, whilst *Y* and *Z* held up theirs. But does any reasonable man believe this? I suppose not, but if not, there is some limit. Let us apply a further test. Suppose *A* is permitted to expose his entire hand whenever he chooses, but has to play it himself, does "Five of Clubs" really contend that *A* would be wise to do so when he has a weak hand? If he does not, he admits a still further limit to what he calls the great principle of the modern scientific game, and really comes back to the principles taught by Hoyle and Mathews. I am aware that "Five of Clubs" says that these two are out of date, but I am not alone in the opinion that Mathews' treatise is the one of all others most likely to develope the powers of a player, to make him use the rules as his servants, instead of being their servant, and to enable him to cross the line which divides the mere book player from the first-class player.

One word more, and I have done. Will "Five of Clubs" refer to his observation 9, p. 284, and explain why *Z*, after two rounds of clubs, will, because *Y* plays the ten, be able to place the Knave of Clubs in *Y*'s hand? *Y*'s play of the Club ten strikes me as decidedly bad; he was, of course, bound to keep up the small Club, but as it could not matter which of the three higher ones he played if *Z* held the Queen of Clubs, as well as the Ace and King, he was bound to play on the assumption that *Z* held only the Ace and King; now, by playing the ten, *Y* would tell *Z* that the nine was in the hands of the adversaries, and *Z* would properly conclude that if either adversary held the Queen or Knave with the nine, he, *Z*, could not get the command of the suit by leading the Ace, and would, therefore, feel bound to lead a small one upon the chance of *Y* holding the Queen, the very thing which *Y*, having four clubs, ought to try to prevent *Z* doing.—MOGUL.

[I am obliged to MOGUL for pointing out what he considers to be faults in my views respecting Whist strategy, because in this, as in all other matters depending on experience and reasoning combined, the truth can only be got at by what may be described as a system of trial and error. With regard to "Mogul's" general view on the question, whether is is better or not to give as much information to partner as can be given consistently with the rules of the game, I may for the present content myself with the remark that modern Whist adopts the principle which he considers unsound, and that, this being so, one can hardly depart from it without actually deceiving partner (which is not quite the same thing as failing to inform him). Clay has clearly shown in his chapter on False Cards, that the result of attempting to keep the adversaries in ignorance of the constitution of one's hand may in this way be most mischievous. It may be the case that in a particular hand, giving information of weakness may do more harm than can be compensated by any advantage; but, in the long run, uniform play in each hand (up to the point when the strategy of the hand has been fully developed) is best, the play of false cards in one hand (though, perhaps, saving from immediate disaster) leading to doubt and mistrust in many others.

In passing, I may note that playing dummy (single dummy, presumably, is meant) is not a trustworthy test of the system of modern Whist; for, in a number of cases, what may be information to one's partner in the usual game may be no information at all to the adversaries.

If anything I said at page 42 means in effect that *there is no limit* to the principle "that it is more important to inform your partner than to deceive your adversary," then I certainly said what was entirely and egregiously wrong. It is often absolutely essential to success in the closing rounds of a hand to deceive the adversary, whether partner be at the same time deceived or not.

Turning now to the game at p. 284, I note first that I entirely differ from "Mogul" as to *Z*'s policy in playing five of trumps and holding up four (the turn-up card). I had not expressed approval, simply noted why *Z* had done this. But the reason which "Mogul" urges against *Z*'s play does not exist. The adverse lead of trumps is not such as to indicate more strength than *Z* possesses himself. *A* has led trumps when his own suit is exhausted from one of his adversaries' hands as well as from his partner's. *Z* can be certain that *A* has not more than four trumps one honour, or three trumps two honours, for if he had had more he would have led trumps earlier, with such strength as he had (and has already shown) in spades and diamonds. Now *Z* has himself four trumps, headed by Ace, ten. He has a long suit headed by Ace, King, Queen. His only chance is in playing as with strength in trumps; and his policy is therefore to show his partner all he can of his trump suit. The game is gone anyway if *Z*'s strength in trumps shall prove insufficient to bring in the long suit, and he simply plays as if he knew for certain that he had sufficient strength. If the score were "love all," the case would be different; *Z*'s policy would then have been to play a backward game, so as to lose as few by tricks as possible. But playing a backward game as the score stood would have been the same thing as throwing up the cards.

Similar remarks apply to *Y*'s play at trick 9, which was rather warmly canvassed after the evening's play. Consider what *Y* knows, what *Z* knows, and (which is quite another question, and very often overlooked) what *Y* knows that *Z* knows. *Y* knows that the Diamond King cannot be with *Z*, or *Z* would have played it as soon as trumps were out. He knows that *Z* cannot credit *Y* wrongly with the Diamond King, for at trick 2 *Y* would have put it on *A*'s Queen, if he had had it. *Z* knows his partner has no more spades (this is clear from *A*'s play in leading trumps at trick 5, which he would not have done if there were two Spades outside his own hand—knowing his partner with none). *Z*, then, can put three Spades in *A*'s hands, three Diamonds at least (from penultimate lead, and four if he has noticed that the lead was really from the ante-penultimate) in *B*'s hand, headed by Diamond King. Also from *B*'s discard of Club five, when only one trick was wanting to win, *Y* knows that if *Z* has not the Queen himself he will certainly not place it in *B*'s hand. Thus *Y* knows that if *Z* has not Ace, King, and three Clubs, he would throw up the cards, for in that case one trick *must* go to *AB*. He knows that *Z* is certain to draw the Queen if it lies with *A*. And he sees that among the various cards, including Queen, with which *Z* *might* credit *Y*, there is only one which would justify the lead of a small card; viz., if *Y* held ten, Knave, Queen, and no other Club. But as *Z* ought to know *A* with only two Clubs, and *B* with only one (it is not *Y*'s fault if *Z* has not noticed *B*'s ante-penultimate lead—tricks 2 and 3), *Y* must have four Clubs. By playing as he does, then *Y* does, not run the risk Mogul suggests, while if *Z* has Ace, King, and draws the Queen from *A* (who, so far as *Y* knows, may hold it), *Z* will know from *Y*'s play in the second round of Clubs that Knave must lie with him, his attention being in the most marked manner called to this by the unusual play of 10 before 9. As I said in the notes, this was not essential to the success of *ZY*, for if *Z* played according to the fall of the cards, he could not fail to draw *Y*'s Knave with a small card (if not holding Queen), but it was well to call his attention to the point. Note that if *Z* had been inattentive to the earlier play, and supposed *Y* to have held originally either Queen, Knave, ten, and a small one, or Queen, ten, and a small one (we are considering the matter from *Y* point of view, who does not know that *Z* holds the Queen), it would be unnecessary to lead a small one, for *Y*'s small one could be used, after Queen and Knave were played in one case, or Queen in the other, to put the lead again in *Z*'s hands. If *Z* thought that *Y* held Queen and ten only, originally, playing a small one would be fatal, as, after winning with Queen, *Y* would have no Club left. With only one hand out of four possible ones, would lead of small one be right—viz., if *Y* had Queen, Knave, ten, in which case if *Z* played the Ace before the small one, *Y* winning third trick in Clubs, would have no Club to return with. Only one chance in four suggesting lead of small one, *Z* would, of course, play the King. As a matter of fact, he ought to know that *Y* could not hold that particular hand. I am disposed to think *Y* displayed unnecessary ingenuity which, as I chanced to be his partner, was perhaps, natural, for partners seldom criticise without prejudice.

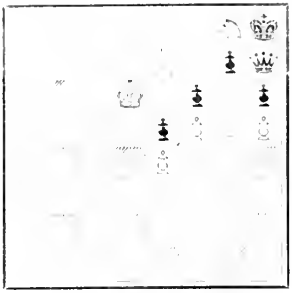
FIVE OF CLUBS.

Our Chess Column.

No. 17

Problem [illegible] by the veteran [illegible], Herr B. Horwitz

BLACK.



WHITE.

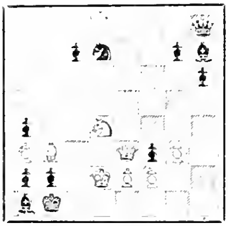
White to play and win.

Mr. Grimshaw has amended his problem in a very ingenious manner, adapting the position by the addition of simply a Pawn to both the solutions that his original problem admitted. He has thereby made two problems out of one, which, although looking almost exactly alike, nevertheless embody different ideas.

No. 18.

By W. Grimshaw.

BLACK.



WHITE.

White to play and mate in three moves.

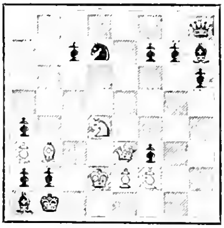
Solution.

Kt. to B.5.	B. takes Kt.
Q. to K.6.	and mates next move.

No. 19.

By W. Grimshaw.

BLACK.



WHITE.

White to play and mate in three moves.

Solution.

Q. to Kt.5.	P. takes Q.
Kt. to Kt.5.	and mates next move.

It will be seen that the addition of a White Pawn in the first problem on Kt.3 renders Q. to Kt. 5 useless, as White now does not threaten mate by Q. from Kt.5 to Kt.sq. Then again, in the second problem, the addition of a Black Pawn on B.2 prevents the first solution, as, after Kt. to B.5, B. takes Kt., the Q., of course, cannot now go to K.6. We are glad to see that our remarks on the original problem, published in the *Illustrated London News*, have had such a satisfactory result. The twin-problem, as it may be called, is a perfect Chess curiosity.

CORRECTION.

Solution of Problem in No. 8, page 171.

WHITE.	BLACK.
1. K. to K.3.	1. K. takes Kt.
2. R. to Q.Kt.7.	2. K. takes R.
3. R. to K.7, mate.	
or,	
	2. Any other move.
3. R. discovers check accordingly, and mates,	
or,	
	1. B. takes P. (ch.)
2. R. to Q.4 (dis. ch.)	2. K. to K.3
3. R. to B.6 mate.	
or,	
	1. B. to B.5 (ch.)
2. Q.R. takes B. (dis. ch.)	2. K. to K.3.
3. Q.R. to B.6 mate.	

ANSWERS TO CORRESPONDENTS.

*** *Please address Chess-Editor.*

W. Golden.—Solution of Problem No. 14 correct. You are right; it is a beautiful composition.

A. J. Martin.—If you assert anything most emphatically, and especially if you are right, as you were in this case, you need never fear nor tremble! See correction. Self-mate is where White compels Black to mate him.

J. P.—You were right; see correction. No. 14 right.

B. Pierce.—In the Evans Gambit, against correct defence the first player loses less than a Pawn, as he has some attack for the abandoned Pawn. You can safely decline the Gambit by playing 4. B. to Q.Kt.3. We, however, prefer to accept it.

G. Woodcock.—No. 9, incorrect—P. to B.6 is the move; 10, correct; 11, incorrect; 12, correct. In note (a), read P. to K.R.4.; in note (b), R. to B.sq.

Mahnie.—No. 14, incorrect. R. to R.4 is the move; 15, correct.

A. C. Skinner.—Solution of No. 14 correct and neat.

Vicar.—Solution of No. 15, correct; 14, B. to R.4.

W. Thurman.—Solutions correct. Thanks for problem.

Salford.—Solution of No. 14 incorrect.

S. L. P.—Solution of No. 14 correct, only you have reversed the board.

Geo. O'Donnell.—Solution of No. 14 incorrect—try B. to R.4. In No. 15, White compels Black to mate him in two moves.

G. M.—Solution of No. 14 incorrect—try B. to R.4.

Arthur Black.—Received with thanks.

F. H. L.—Solutions correct.

Contents of KNOWLEDGE *No.* 14.

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LONDON: FRIDAY, FEBRUARY 17, 1882.

CONTENTS OF No. 16.

BIRTH OF THE MOON

BY TIDAL EVOLUTION.

BY DR. BALL, ASTRONOMER-ROYAL FOR IRELAND.

PART I.

THE daily rise and fall of the sea, which we call the tide, has long been known to be connected with the moon. The discovery of the law of gravitation enabled Newton to explain how the tides were caused. Newton showed that the tides were partly due to the attraction of the sun, but chiefly to the attraction of the moon.

In the present paper we shall principally consider the tides which are produced by the moon, and the reader will please understand that this is the tide to which we refer, except otherwise stated. The tides are of the utmost importance in our seaports. They are not, perhaps, very often employed to do useful work, in the sense of driving machinery, but on work of one kind or another the tides are unceasingly busy. No one who has watched the rise and fall of the tide on the beach, or the currents of the tide in a river, can doubt that the tides do work. We need not here attempt to enumerate all the varieties of tidal work. Let it be sufficient to mention one kind, as an illustration. The waters of a strong tidal river like the Avon, at Bristol, are heavily charged with mud in suspension. The tides are carrying that mud, and in doing so, they accomplish work, at an expenditure of energy which could be expressed by an equivalent amount of horse-power.

The steam-engine will only yield an appropriate horse-power when the boilers are heated by a proportionate quantity of fuel. So also the tides can only accomplish their gigantic work all over the world because they are bountifully fed with energy. Whence do the tides obtain their energy? They draw it from a certain store which is being steadily squandered and never replaced. The supply in the store may be great, but it is not inexhaustible. It is easy to discover the store when we consider the circumstances of the case. Fixing our attention solely upon the earth and the moon, we can enumerate the different forms of energy which may conceivably be available. The case can be very simply stated; there is a store of energy in the earth due to the fact that the earth is rotating on its axis. There is a similar store of energy due to the rotation of the moon on its axis. The latter is, however, very small, and may be left out of sight for the present. A third source of energy is due to the fact that the moon is separated from the earth, and that, as it would require energy to force the earth and the moon asunder, so, if the earth and the moon were allowed to draw together, energy would be given out. To this must be added the energy due to the motion of the moon in its path around the earth. To put the matter briefly, we may say that the available sources of energy for the tidal work must be sought either in the rotation of the earth on its axis, or in the distance of the moon, including in the latter case the energy due to the velocity of the moon in its path, which is intimately connected with the distance of the moon from the earth. As the tides are incessantly drawing on this store, it is imperatively necessary that one or both of these sources of energy be decreasing: we are therefore forced to admit that the velocity of the earth's rotation on its axis must be diminishing, or that the distance of the moon is decreasing, or that both velocity and distance are decreasing. There can be no doubt as to which is the true explanation, for the question is determined by a well-known dynamical principle. This principle assures us that the supply of energy required by the tides must be drawn from the rotation of the earth. Indeed, we may go further than this. It is most curious to observe that a second draft is made upon the reserve energy stored up in the earth's rotation; this second draft is actually expended in pushing the moon away from the earth.

We have, then, two very remarkable astronomical consequences of the tides. These consequences are founded on dynamic principles, but in a manner not very easy to explain without going into technical matters. The first consequence is that the velocity with which the earth rotates must be abating—in other words, that the tides on the earth are increasing the length of the day. The other consequence is not a little remarkable. It states that the moon must be describing an orbit around the earth, which, in the course of ages, is gradually becoming larger and larger. It must be remembered that these two consequences of the tides are not mere speculations. They are as true as the laws of dynamics, which have been confirmed by universal experience. The propositions just stated will not be questioned for a moment by anyone who is acquainted with mechanical principles. Let us take first the important fact that the length of the day is gradually increasing. It must be admitted that the change in the length of the day is excessively slow. Even in centuries, the change is but a fraction of a second; but the change is always in one direction, and, consequently, ever since the earth and the moon commenced to have a separate existence, the length of the day has been getting steadily greater and greater, until it has at present attained the well-known 24 hours. We are now to look back into the history of the earth and the moon in very remote antiquity. Our ordinary chronologies of thousands of years are here quite inadequate. The unit of time adapted for the earth-moon history is one million of years. A million years ago the length of the day was appreciably shorter than it is at present. There was a time when the day, instead of being 24 hours, was only 23 hours; earlier still, we find the day still less and less, but we shall not halt at any intermediate stage; let us at once project our view back to the earliest and the most interesting epoch in the history of the earth-moon system. At the very remote epoch to

which I refer, the day was very much shorter than our present day. It was, indeed, only a small fraction of 24 hours. We cannot be sure of the precise number of hours in the day at that time; it seems to have been more than two hours and less than five hours. For simplicity, we may speak of the length of the ancient day as about three hours, but it must be carefully remembered that this estimate is to be regarded merely as provisional, though perhaps approximately correct.

It had long been known that the tides were increasing the length of the day, but the very remarkable researches now to be described have only been made quite recently. They are the work of Mr. G. H. Darwin, Fellow of Trinity College, Cambridge. Mr. Darwin's labours are contained in a series of memoirs of a very abstruse nature, and we here propose to give a general sketch of the principal results to which he has been conducted, so far as the earth-moon system is concerned. We shall endeavour as clearly as possible to indicate what portions of the theory are to be regarded as absolutely established and what portions are still more or less speculative. We have halted in our retrospect at a day of 3 hours. Why is it that we cannot look much earlier—to a day of one hour, for instance? There is a very good reason why we cannot do so. In those exceedingly early times, our earth was not the hard rigid body with which we are familiar. It was in those days so heated as to be quite soft, even if not actually molten. A body like our earth in a molten state will not remain in a spherical form when it is rotating on its axis. It will bulge out at the equator: it will become flattened at the poles. The greater the velocity, the greater will be the protuberance at the equator. If, indeed, a certain critical velocity be attained, it will be impossible for the body to hold together; the centrifugal force would be too great, and a rupture of the body must ensue. It is not practicable to calculate what that critical velocity may be. The critical velocity depends upon circumstances not within our present knowledge, but it can be shown that the velocity does not differ, perhaps, very much from a rotation once every 3 hours. We thus see that a rotation of this amount is about the greatest that our earth could ever have had in the present order of things. What occurred prior to this is not to be discussed at present.

We now turn our attention to the moon, which, in consequence of the tides, must be describing an orbit of gradually-increasing dimensions. It may be well to remind the reader that the orbit of the moon is at any moment a nearly circular ellipse, and that this ellipse is subject to many disturbing influences well known to astronomers. But these disturbances are all periodic. They increase and they decrease. They may, in the course of ages, be entirely overlooked in comparison with the tidal changes, which constantly act in the same direction. In very ancient days, the moon must, therefore, have been nearer to the earth than it is at present. The further we look back, the nearer must the moon be. There is no reason why we should not look back to an excessively remote time, when the moon was as near as possible to the earth. The most extreme case would arise when the moon was so near the earth that the two bodies were almost touching, and we are bound to believe that, at some inconceivably remote epoch, this did actually happen. It is easy to calculate what must then have been the length of the month, or the time which the moon occupied in completing one revolution around the earth. Kepler's law shows that when the moon completed one revolution around the earth in three hours, the two bodies must have been very close together. There was thus a certain very critical epoch in the earth-moon history. At that time the earth and the moon were close together; the earth was spinning round on its axis in three hours, and the moon was revolving around the earth in the same time. The three hours is, as already remarked, open to some uncertainty; but there can be no doubt that at this critical epoch the earth was turning round in the same time as the moon, whether that time be three hours or some other amount of about the same magnitude. At this interesting epoch the earth kept the same face directed towards the moon, and the moon kept the same face towards the earth. In fact, the two bodies revolved just as if they were bound to each other by invisible bands.

MICROSCOPIC VISION AND MINUTE LIFE.

By Henry J. Slack, F.G.S., F.R.M.S.

In the early days of the microscope, wonderful reports were made of its revelations, and in 1745 Baker complained that "some people made false pretences and ridiculous boasts of seeing by their glasses the atoms of Epicurus, the subtle matter of Des Cartes, the effluvium of bodies, the emanations of stars, and such-like impossibilities."

One doctor declared that he had seen the effluvium of magnets as a mist. Probably he mistook a misty view for a view of a mist. As the instrument improved, and more knowledge was gained, the sham wonders ceased to appear, and although little was known of the molecular construction of matter, it was no longer imagined that its minutest or ultimate particles would be seen with the powers employed. We are not yet in a position to say exactly where the limits of vision must necessarily end. Dr. Pigott reduces the image of a spider's web to less than one-millionth of an inch in diameter, and then shows it by remagnification. His process is like diminishing an object by looking at it through an inverted telescope, and magnifying the small image thus obtained. After proving that "spider lines, miniatured down to the fourteenth part of the hundred-thousandth of an inch, could be made visible to ordinarily good eyesight, under proper* microscopical manipulation," he sought for actual objects comparable in minuteness with these optical images, and succeeded in showing them. One plan he adopted was to smash, with a watch-spring, very small globules of mercury in a minute drop of petroleum, inserted under a thin cover on a slide. Many of these mercurial particles thus obtained were exceedingly minute, some round, and others irregular. Upon some of the irregulars he found minute black points, visible with a power of 1,000 diameter, and comparing them with the thinnest spider line, he found one, in particular, less than one millionth of an inch in diameter.

The visibility of extremely minute objects depends much upon their optical properties, and how they are situated in reference to neighbouring bodies. In his remarkable investigation into the life-history of certain small objects, called monads, Mr. Dallinger employed a magnification of five thousand linear, and could not, with this great power, see the minutest germs capable of development into active organisms. In the course of the spontaneous germination controversy, the extreme minuteness of these germs was not dreamt of by advocates of that theory, and Pouchet thought the "panspermists," as he called those who adhered to the doctrine of *omne vivum ex ovo* in the sense of regard-

* "Proc. Roy. Soc.," p. 208 1880.

ing every living thing as the offspring of a previous living thing, quite overthrown by considerations of the density air must have if it were as full of germs as they supposed. Pasteur, Dallinger, and Tyndall have completely destroyed this argument, and shown that germs in vast numbers can exist floating in the air without any appreciable addition to its weight, or obvious diminution of its transparency, though, as we shall see, that is easily affected. Germs of various kinds are most numerous in the air of towns and inhabited rural districts. They become fewer as mountainous heights are ascended, and in well-selected situations disappear entirely. All ordinary air will cause life to appear in appropriate fluids that have been previously rendered sterile; but if a bottle containing such a fluid is opened with due precautions on a mountain peak, and then hermetically sealed, no life is developed. In such experiments Pasteur used glass vessels with their necks drawn out, and sealed up by melting them in a spirit lamp, or with a blow-pipe. To ensure against accidentally introducing any germs he might have carried with him up the mountain, he broke the tips of the vessels' necks with pliers made hot in a spirit lamp, and, after air had been admitted, instantly closed them by remelting. He thus found pure air to be free from any life-producing particles. Tyndal's experiments were made by imitating the well-known motes in a sunbeam. He found that a beam of electric light gave evidence of amazingly minute particles floating in the air, and that when this effect entirely ceased, the air contained no germs. We can seldom form an accurate idea of the real size of the minutest objects we can just discern with the microscope. They often look a good deal bigger than they are, through the optical defects of the instrument and the eye, though Dr. Pigott found that when all the conditions can be rendered favourable, the error is very small. The smallest floating particles lit up by Tyndall's artificial sunbeam are too minute for individual recognition by the microscope. It is only when they are numerous enough to form a delicate cloud that their presence can be made manifest. Objects while floating in the air could not possibly be seen with high powers. It is necessary to collect them, and keep them either still, or only moving in some fluid with moderate velocity. If they are very nearly of the same refractive power and colour as the fluid in which they are immersed, they can only with great difficulty be distinguished at all. Naturalists and physiologists can seldom arrange all the circumstances in the way most favourable for attaining to the extreme limits of vision, and their researches are usually made within much narrower limits. If, however, the utmost possible power of the microscope could always be employed, it would not bring us near the probable limits of organic life. The minute organisms capable of inducing changes analogous to the fermentation caused by yeast have received great attention of late years, and several important diseases are distinctly traced to them. Béchamp estimated that eight thousand millions of germs of one micro-ferment only occupied one cubic 25th of an inch. Not one of these minute bodies could develope except by carrying on complicated processes of a chemical nature, involving very active movements of its atoms and molecules.

The mathematicians have made calculations founded upon the pressure exerted by gases, and other considerations, which show that a particle of the sort of matter, such as albumen and protoplasm, chiefly concerned in life processes, contains in a space of one cubic thousandth of an inch more molecules than any one could possibly form any conception of. Sorby, taking a probable mean of such calculations, supposes one cubic thousandth of an inch of water to contain 3,700,000,000,000,000 molecules. A sheet of ordinary note paper is about one hundredth of an inch thick. One tenth of this would, of course, be one-thousandth of an inch, and a little square box of that size each way would hold the amazing number of water molecules mentioned. Perhaps a few thousands of such molecules may suffice for some manifestation of life, but even if many millions should be requisite for the structure of the humblest and simplest germ, we could never expect to see the actual beginnings of life.

When one million is spoken of, few persons form any definite conception of the quantity meant, and billions, trillions, quadrillions, &c., convey no graduated conceptions to anybody except in the roughest way. Mr. Samuel Butler, in his work on "Unconscious Memory," states that "a man counting as hard as he can repeat numbers one after another, and never counting more than one hundred, so that he shall have no long words to repeat, may, perhaps, count ten thousand, or a hundred a hundred times over, in an hour. At this rate, counting night and day, and allowing no time for rest or refreshment, he would count one million in four days and four hours, or say in four days only. To count a million a million times over he would require four millions of days, and roughly ten thousand years. For five hundred millions of millions he must have the utterly unimaginable period of five million years." And yet in how small a space the matter around us contains molecules to this inconceivable extent! The things unseen far surpass in number, as in minuteness, the things seen.

THE LAST TRANSIT OF VENUS.*

By the Editor.

MY friend, our F.R.A.S., used to say the transit of Venus was, with me, like King Charles's head with Mr. Dick. The resemblance was certainly striking; Mr. Dick was always trying to keep King Charles's head out of the Memorial, and constantly failed; I spared no efforts to bring the transit of Venus before the public, and always succeeded. Be this as it may, it is certain that, except in the case of an event like the transit, which was bound to come off at a particular time, matters of scientific discussion are generally none the worse for waiting. Certainly, now that the transit is over, and no good can arise from any discussion of the best ways of observing it, I should have thought myself very unlikely to go again over the well-worn ground, or to recall the circumstances of a long past controversy: is not the story told in the Encyclopaedia Britannica, in the American Cyclopaedia, and in the Monthly Notices of the Astronomical Society?

But I must confess the introduction of the treatise before us—a treatise giving ample evidence of the zeal and energy with which Sir George Airy could do any work to which he gave his mind—has somewhat changed my views as to the desirability of silence. There is not a word which is absolutely untrue in these pages; but there is a quiet *suggestio falsi*, a calm and complete *suppressio veri*, which I cannot but consider *par trop fort*. Let me briefly run through the facts of the case.

In 1857, Sir George Airy made a communication to the Astronomical Society, in which a comparison was made between the transits of 1874 and 1882, with regard to the suitability of the two chief methods for observing these

* Account of "Observations of the Transit of Venus," 1874, December 8. Printed for the Government Stationery Office, under the authority of the Lords Commissioners of the Treasury, 1881.

phenomena. In 1861, May 3, he addressed another communication to the same body, advocating *for the observation of the transit of* 1882 *only* a *reconnaissance* of Antarctic stations. In 1868 he made yet another communication, advocating the same views, and an active discussion followed, in which the Hydrographer of the Admiralty, Captain (now Sir G.) Richards took part, and in which it was generally agreed by the naval officials present that Antarctic stations could, and should, be occupied, as suggested, for observing the later transit.

It chanced to be my duty at that time to write the reports of astronomical progress for the "Quarterly Journal of Science," and for the "Popular Science Review:" and having the belief that such reports should not be limited to mere quotations from those who are assumed to be authorities, but should involve a little independent inquiry, I began the investigation of the problem which I then supposed that Sir G. Airy had most fully and satisfactorily dealt with. But I found his investigation to be incorrect. A certain assumption at the beginning, which had every appearance at first sight of being right, turned out on closer inquiry to be altogether wrong. To give an idea how wrong it was, I need only point out that the method deemed most suitable for the earlier transit turned out to be the only method available (of the two in question) for the second, while the method regarded as only available for the second turned out to be far and away the best for the first transit.

After calling Sir G. Airy's attention to this matter in a courteous letter (supposing, of course, that when once he had noticed the error he would set it right himself), and receiving from him (as on several other occasions) a reply more curt than courteous, I set to work to complete my investigation for publication, and I eventually communicated it to the Astronomical Society. Its accuracy was never questioned. Sir George Airy admitted in a letter (by no means intended to say pleasant things) that it was the most complete and accurate discussion of the transits published up to that time. It was only open to one exception; it was not official: and because it was in no sense my duty to make this investigation (in other words, because I was not paid for doing the work), some (chiefly minor officials) fondly imagined that I had no right to make it.*

Now, in this complete investigation of the matter, I was able to demonstrate two points of great importance—first, the utter inadequacy of the arrangements suggested for the observation of the transit of 1874; and secondly, the utter uselessness of the proposed Antarctic expeditions for observing the transit of 1882. Thirdly—but scarcely of less importance—I noted a region in British India, including several of the best stations for observing the transit, and heretofore overlooked (because of the singularly unsatisfactory method of mapping the observing districts which Sir G. Airy had unluckily adopted). To these important matters I called attention more publicly than by papers read before the Astronomical Society, viz., in an article which appeared in the *Spectator* of February, 1873. My views were stoutly and skilfully supported by Sir Edmund Beckett, in a paper which appeared in the *Times* of Feb. 13, 1873.

Note that in these papers it was shown (1) that a number of northern stations where the whole transit of 1874 could be observed should be occupied, for which kind of observation *no provision at all had so far been made;* (2) that no stations need be occupied for observing the whole transit of 1882, and *especially that the dangerous Antarctic stations could not possibly be occupied with advantage then* (but it was noticed that if they can be occupied at all, they should be occupied in 1874, to supplement stations already provided for in the south, where, *as it happened*, though whole-transit observations had not been intended, such observations could be made); and (3) that the North-Indian region mentioned above should be occupied.

Government, of course, followed the customary official course,—inviting the officials whose judgment was oppugned to say whether they were mistaken. Equally of course, those officials said they had made no mistake, implying even, by their tone, that officials never do, or can, make mistakes.

Sir G. Airy tried the same line with the Astronomical Society. He pooh-poohed the notion that Siberian and north Chinese stations could possibly be occupied—and a fortnight later news came that American, Russian, and German astronomers were to occupy these very regions. He ridiculed the North Indian region, which he had overlooked—and very soon after he had to provide for extra stations in that very region. But he specially ridiculed the suggested Antarctic expeditions (one of the islands—St. Paul's—which I had recommended, was eventually occupied by the French, and good work done there), as if I had ever had any reason but his own advocacy of such stations (ridiculous advocacy, he now asserted) for believing that they could be occupied. And of course, the very officials who, when he had wanted the stations for 1882, had urged no objections, now swallowed all they had before said, and—greatly daring—said the very opposite.

Just here, *where I had gone wrong in following him and believing in official utterances, was the one point where what I had advocated was not carried out in every detail; and just this point is all that Sir G. Airy chooses to notice in the introduction to the volume before us.* He describes my paper in the *Spectator*, and Sir E. Beckett's, in the *Times* (three columns), urging most important changes, which had eventually to be adopted, as papers "strongly urging the adoption of Enderby Land (which, after careful consideration, I had rejected) for a southern station." As a matter of fact, Sir G. Airy never had rejected Enderby Land for 1874: he had never thought of it; he had urged Antarctic stations for 1882, and had only given them up after I had shown that such stations, useful enough astronomically in 1874, would be of no adequate value in 1882. He gave up Antarctic stations simply because, if they had been occupied at all, they must (after what I had shown) have been occupied for a purpose which he had himself overlooked. *I have not a shadow of a doubt, after carefully studying what was said by Airy, Richards, Ommanney, Davis, and Stone, on Dec.* 11, 1868, *that but for my demonstration of the astronomical uselessness of Antarctic stations in* 1882, *we should have had, before now*

* Strangely erroneous ideas are very common about official positions. A man is appointed to an important office in order that he may do certain work, for which he is to be more or less handsomely remunerated, out of money provided by the tax-payers. If this officer is at the head of a department, he has, besides his salary, authority over all other officials in that department. But many seem to imagine that this authority extends to persons outside official circles—an idea preposterous on the face of it; for such persons are in reality among the employers of such officials, paying them to do certain work, and having a right (if they chance to have the necessary knowledge) to inquire how the work is being done. The trouble is, that while so many have the right, so few have the knowledge. Those who have it, if they possess also the time to do the work, have something more than the right—it is their duty to make the inquiry. Who else is going to do it? If—which is altogether unlikely—our present able and active Astronomer Royal were grossly to neglect all the duties of his office, who is there above him in office who could indicate his shortcomings? and who is there below him in office who would venture to. Somebody outside of office *must* do the work.

to pay for expeditions to Possession Island and other places in the dangerous Antarctic seas, and to provide for stations to be occupied there during the transit of next December. It was this pet plan of his which Sir George Airy was really giving up, when he explained to the Government that Antarctic stations were "geographically unsuitable," and so forth.

It is a favourite argument of the junior officials who tried to earn approval from their chief by attacks on extra-officials like Sir E. Beckett and myself, that Sir G. Airy never did yield as to the stations for observing the whole transit. It is not quite true. He yielded in the case of the Indian stations, which astronomers of other countries were not likely to occupy. I think too highly of his real zeal for science to suppose he would not have yielded in the case of Siberian and North Chinese stations, if America, Russia, Germany, and France, by occupying the stations he had overlooked and later stigmatised as useless, had not saved him from the concession.

Like all that Sir George Airy has ever done in this way, the record of the observations made by the various parties is exceedingly well arranged. His labours here would have served to very much more than retrieve what, after all, was in its inception but an unlucky mistake (which might easily have been corrected, and the world none the wiser, if he had not been so needlessly impatient of extra-official suggestions). Sir George Airy shows himself in the body of this work what he has shown himself during his whole tenure of office, the most energetic and laborious of our Astronomers Royal. If he had left out all reference to the discussion of 1869-1873, or if, referring to it, he had told the whole story, this would have been all I need have said; but I have not thought it just, either to myself or those who by their aid ensured the adoption of proper measures for observing the transit of 1874, to allow an imperfect and entirely misleading account of the matter to remain uncorrected—though I know very well that for one who might be misled by Sir G. Airy's inexact account, thousands (including himself) know how the matter really stands. It seems almost incredible that the writer of this account should be the same man who so frankly and nobly acknowledged his error in the Adams-Leverrier controversy.

THE CRYSTAL PALACE ELECTRICAL EXHIBITION.

Second Notice.

THOSE who can look beneath the surface will see that during the past week some considerable progress has been made towards completing the exhibits. To a cursory examiner, however, but little improvement can be apparent. A few Jablochkoff lamps (*Compagnie Générale d'Electricité*) are lighted, but present a poor appearance when compared with the other systems. The four Cromptons which light the space in front of the stage are also far from satisfactory. We may hope to see something better in the telephone section during the next week or so.

We will now give a description of

The Brush System

of electric lighting. In the machine-room, as mentioned last week, are a number of generating machines, which, when all in work, will be driven by five steam-engines, two of them of 20-horse-power, two of 30-horse-power, and one of 25-horse-power. The latter is the only one at present fitted up. It is working up to about 15-horse-power, and drives three of the generating machines, two of which maintain between them thirty-two arc lights, and the other about fifty incandescent lamps. The third machine is, however, capable of supplying the current for as many as 150, or perhaps 200 lamps.

The lamps are very simple in construction, requiring no adjustment after being once put in order. Fig. 1 is one

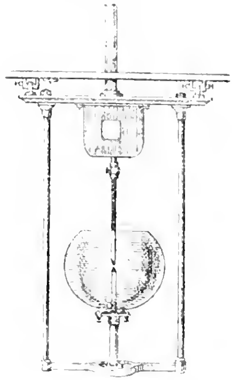


Fig. 1.

of the ordinary forms which will burn uninterruptedly for eight hours. The carbons are made in 12-in. lengths, and when the lamps are required for more than eight hours together, they are furnished with a double or even treble supply of carbons, the current passing from one pair to another automatically. Fig. 2 is a somewhat rough

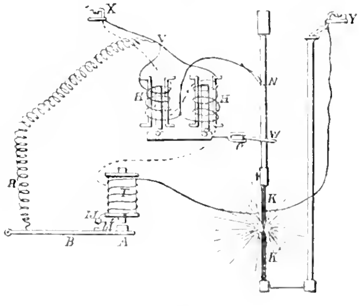


Fig. 2.

diagram illustrating the principles and action of the lamp. The current enters at *X*, and at *V* splits into two sections, each of which passes through a small coil of thick wire *H H'*, and on leaving the coils the currents combine again and pass to the upper carbon rod; thence through the arc to the lower rod, and out at *Y*. From *X* there also passes a thin wire which is coiled many times round *H H'* outside the thick wire. These thin coils are wound *in series*, and on both bobbins they are *in opposition* to the thicker coils. The thin wire then passes several times round another bobbin *T*, carrying a small coil of thick wire wound in *the same* direction. The circuit is completed by a wire to *Y*.

The lever, *A B*, is in metallic connection with the positive pole, and carries a small button, *M'*, over which, but not

normally touching it, is another button, *M*, in connection with the thick wire attached to the wire leading to *Y*. *W* is a washer or clutch, consisting of a small circular disc of brass fitting loosely on to the carbon holder, so that, if slightly tilted, it takes a grip on the holder on both sides, and prevents the carbon rod falling. When the current enters, the rods being in contact, a circuit is formed. The current divides between the thick and thin wires on *HH'*, only 1 per cent., however, going through the thin wire, its resistance being 150 ohms, while that of the thick wire is only 1·5 ohms. The current going through the thick wire draws up the soft iron plungers, *SS'*, and with them the washer, *W*, which pulls the carbon holder up a short distance, and so creates an arc. When the arc gets too long, the resistance of the thick wire circuit is considerably increased, while that of the thin wire circuit is relatively decreased. Consequently, the upward tendency imparted by the thick wire is counteracted by the greater amount of current passing through the thin coils, and the holder falls until it is again restrained by the clutch. Should it so happen that the carbon becomes broken, or that by any means an arc cannot be formed, the whole of the current passes through the thin circuit, and in passing through *T* is sufficiently powerful to attract *A*, thereby putting *M* and *M'* in contact. The current then flows from *X* along lever *BA* through *M'* to *M*, through the small, thick wire, and away to *Y*, thus cutting the carbon-rod completely out of the circuit. This "cut out" allows the lamps to be joined up in one circuit, without the risk of all of them going out in the event of one being faulty, a feature belonging only to this and one other form of lamp.

Preparations for further lighting are being made on a magnificent scale; a 30 horse power engine is to drive one of the largest-sized machines, which, it is said, will maintain one light of 150,000 candle-power, and from another machine a light of 50,000 candle-power is to be directed towards the Houses of Parliament.

A magnificent display is being rapidly prepared in the Alhambra Court, which promises to outdo everything else in the Exhibition. We must, however, defer any further remarks on this company's exhibit until a future occasion.

POPULAR ASTRONOMY

BY THE CHIEF OF A GREAT NATIONAL OBSERVATORY.*

SEVERAL correspondents, in the course of their inquiries as to books on astronomy, have asked us whether we can recommend the treatise written by Professor Simon Newcomb. Although the book has been more than two years before the public in this country, as well as in America, we deem it well, in response to these queries, to give an account of it, as manifestly many in England are unaware of its merits.

We do not, as a rule, much admire the way in which *soi-disants* professional astronomers treat the wonders of the heavens and the grand problems presented by the movements of the celestial orbs. Too often they discuss these as a mere land surveyor might discuss the teachings of the earth's crust. Methods and instruments of observation are of much more interest in their eyes than the lessons to be learned from observations after these have been carefully made. They seem, at times, even disposed to be angry, as Flamsteed was with Newton, when the raw materials which they have gathered together are worked into the manufactured article—Knowledge.

It is a pleasant disappointment, then, to find that Professor Newcomb, the chief official astronomer of the United States, does not take so limited a view of popular astronomy. In his "Popular Astronomy," he deals more or less fully with every part of the science which could possibly be of interest to the general reader. He gives a full, though condensed, view of the history, principles, methods, and results of astronomical research. The historic and philosophic sides of his subject are treated with more fulness than is usual in works of this kind. The purely technical side has been somewhat condensed, but is dealt with as fully as is necessary.

Having briefly treated in the first chapter of the apparent motions of the heavenly bodies, and described the Ptolemaic system, Professor Newcomb proceeds in the second and third to describe the work of Copernicus, Kepler, and Newton, in establishing, step by step, the true system of the universe. These three chapters form the first part of the work. The second part deals with practical astronomy. The picture of Bianchini's monstrous tube, as mounted in the grounds of the Barberini Palace at Rome in the seventeenth century, gives a good idea of the difficulties with which the earlier observers had to contend. A telescope that a child could handle would, in our day, give better views of the heavenly bodies than this unwieldy instrument, the moving of which required the efforts of several men. After a description of the modern achromatic telescopes and reflectors, there follows a section on the magnifying powers of telescopes, which we would recommend to the especial study, not only of observers, but of those who have occasion to discuss observations. In particular, this section will serve to correct the common error that large telescopes increase the apparent brightness of objects which present a visible surface, as distinguished from objects like the stars, which even under the highest telescopic powers appear as mere points. We could have wished, however, that this error had not been corrected merely by an *ex cathedrâ* statement, but that the reasoning establishing the true relations between magnifying and illuminating power had been given in full. The second part includes also an account of the application of the telescope to celestial measurements, a most interesting and valuable chapter on measuring the distances of the heavenly bodies, and chapters on the motion of light and on the spectroscope.

The third and fourth parts of the work are devoted to descriptive astronomy. In the former, after a chapter on the general structure of the solar system, we have a long and very interesting chapter on the sun, and chapters on the inner group of planets (including the earth and moon), on the outer group or family of giant planets, and on comets and meteors. The fourth part deals with the stars—first, as they are seen with the naked eye and with the telescope; secondly, as they are probably arranged in space; and, thirdly, as each has probably been formed. The third chapter discusses questions of extreme interest, such as the sources of our sun's heat, the secular cooling of the earth, the plurality of worlds, and the nebular hypothesis. Lastly, we have lists of the principal telescopes of the world, and catalogues of double stars, nebulæ, star clusters, and comets; the usual tables of elements (but greatly improved in many respects), a useful glossary, and a set of star maps.

One general characteristic of these various chapters may be broadly indicated. There is scarcely one statement, properly so called, in the whole work which is not strictly accurate, or rather, which does not present with strict

* "Popular Astronomy." By Simon Newcomb, LL.D., United States Naval Observatory. (London: Macmillan & Co.)

accuracy the present views of astronomers in relation to the subject matter. This remark is not intended to include expressions of opinion on the one hand, or, on the other, such statements as are subsidiary to explanations or illustrations, but only definite statements respecting astronomical matters. With regard to all, or very nearly all of these, the reader may trust our author with perfect confidence. In this sense the work deserves the high praise accorded to it, of being thoroughly trustworthy.

The arrangement of the work throughout is admirable, and the treatment of individual subjects is at once lucid and attractive. Here and there the style is rather commonplace, but it is never confused. Every sentence has one meaning and one only. Moreover, the meaning of every sentence lies on the surface. In these respects, too, many who write on scientific subjects have failed to consider the requirements of the general reader. To the mental effort which the study of such subjects requires, they add the effort required to determine the meaning of ambiguous phrases. Moreover, Professor Newcomb commonly avoids the mistake of using technical terms where ordinary terms would serve equally well.

The treatment of the subject is simple in another sense, perhaps not quite so satisfactory. Professor Newcomb leaves many matters unexplained which are commonly explained with more or less fulness in treatises on general astronomy. For instance, in nearly all such treatises we find some explanation of those disturbances of Uranus by Neptune which led to the discovery of the latter planet. The explanation is usually incorrect, but that is a detail. It always requires more or less mental effort either to follow the explanation, or to discover that the explanation is, as usual, all wrong. No such effort is required in reading what Professor Newcomb says respecting the discovery of Neptune, simply because he contents himself with a mere statement of facts. Possibly this was the wisest course in the case of a treatise intended for general reading. It may be doubted whether the true explanation given by Sir J. Herschel, for instance, has been followed by a tenth of his readers, or whether one-tenth of the readers of Lardner's, Chambers's, and other such works, detect the error underlying incorrect explanations of this subject. It is necessary, however, to remind the reader of the work before us that the ease with with which he follows the author here and elsewhere is due to the circumstance that difficulties are avoided—they are not overcome. This is even the case with Professor Newcomb's treatment of the subject of the sun's distance, which few have mastered more thoroughly than he has. His statements respecting the various methods available for determining the distance of the sun are thoroughly trustworthy, and his opinion respecting the result to which observations point may be accepted as the one which accords best with the evidence. But the matter is not explained. We are able, indeed, without making too long an extract, to quote all that he says in the way of explanation, which is simply this :—

"In consequence of the parallax of Venus, two observers at distant points of the earth's surface, watching her course over the solar disc, will see her describe a slightly different path, as shown in Fig. 50. It is by the distance between these paths that the parallax has hitherto been determined."

The general nature of parallax has been already explained, and the distinction between Halley's and Delisle's methods for determining this distance is briefly indicated further on. But the principle underlying both those methods, as well as the photographic and heliometric methods, is explained no further than in the sentence quoted above. It is not too much to say that it is not explained at all.

An important and, in our opinion, a valuable feature of this work is the discussion of ideas more or less speculative with regard to the heavenly bodies. There are some astronomers who object strongly to the introduction of ideas of this sort into treatises on astronomy. But the objection seems unwise, not to say peevish. The chief charm of the study of astronomy lies in reality in the mystery which enshrouds the orbs of space. What we know respecting these bodies is little; the unknown is infinite. Now, unquestionably, mere idle speculations, not even suggested by observation, are profitless. But speculations based on the results of observation and physical research are not only interesting, but fruitful. In such speculations have originated nearly all the hypotheses from among which the established theories of the science have been evolved. It is noteworthy that the greatest astronomers have indulged freely in speculations respecting the unknown. Copernicus, Galileo, Kepler, Newton, the Herschels, and a host of those to whom astronomy owes the noblest of its triumphs, have discussed speculations and hypotheses, of which some have been forgotten, others are remembered only because of the theories which they suggested, while some few have become themselves the theories of the science. It may, indeed, be truly said that no astronomer who has been unwilling to allow his thoughts to pass beyond the boundaries of the known has ever made great discoveries. We are glad to see that Professor Newcomb, while he has shown himself an exact and careful observer, and while, in certain departments of mathematical research, he has held his own with the greatest mathematicians of our time, is ready to consider subjects which belong as yet to the region of speculation. He presents, for example, the various theories respecting the sun's condition which have been advanced by Secchi, Faye, Langley, and Young, though three of these are certainly, and all four possibly, erroneous. He adopts, with apparent approval, several opinions respecting the condition of the larger planets which have been advocated in recent years in this country. He accepts the conclusions of the Bonds, Peirce, and Clerk Maxwell respecting the condition of Saturn's rings. And speaking generally, he presents astronomy, not as a subject respecting which certain facts and statements have to be committed to memory, but as a living science, full of promise, though also full of mystery, presenting a grand array of achievements in the past, but offering also a vast number of noble problems for attack, and possibly for solution, in the future.

The present work, it will be inferred, is a very valuable contribution to astronomical literature. It is especially characterised by originality of tone and treatment throughout. It is remarkable also for the care with which details have been attended to, quotations verified, tables corrected, and so forth—matters respecting which scientific writers of eminence are not always so careful as they might be.

(To be continued.)

Intelligence in Animals.—J. H. sends the following :—"An amusing account of a pet baboon, in a letter from a friend at Zanzibar is communicated to *Nature* this week by Miss Julia Wedgwood. An interesting statement (in relation to the contention that laughter is one of the distinguishing attributes of man) is, that 'Judy,' the baboon in question, used, when she romped with her mistress, 'to open her mouth, show all her white teeth, and regularly laugh like a child, especially when she was tickled.' She never laughed at a joke, and nothing made her so savage as being laughed at."
[Darwin gives similar instances in his work on the "Expression of the Emotions."—Ed.]

Reviews.

SIR EDMUND BECKETT ON THE LAWS OF NATURE.*

SIR EDMUND BECKETT'S writings are always delightful, whatever his subject, and whether one agrees with him or not. His subject in the book before us (which has been some time before the public, but the new edition has not), is recondite, and Sir Edmund's views are strongly opposed to those which are generally held by men of science in these days. But from the first page to the last, the book is attractive, if only through the clearness of the reasoning and the strength of the style. There is not a writer living who has a style more markedly his own than Sir Edmund Beckett, and though Carlyle, among writers who have passed away, had a style more marked than Sir Edmund's, the peculiarities of the author of "Sartor Resartus" were assumed, whereas those of Sir E. Beckett are the result of true mental idiosyncrasies.

In the present work, Sir E. Beckett has very plainly—and, on the whole, very fairly—presented the issue between the believers in special creative, and as it were legislative, acts on the one hand, and in evolution pure and simple on the other. He has shown well the insufficiency of evolution as at present understood as regards explanation of the mysteries of the universe, and he has pointed out very definitely the sufficiency of the theory of an omniscient all-powerful Being to account for all the phenomena, including the existence of uniform laws existing so far as can be seen throughout all space and operating during all time. An evolutionist might with equal clearness, we conceive, point out the utterly inconceivable nature of such a Being on the one hand, and the sufficiency of laws of evolution within the range over which scientific research can extend to account for those observed relations which, referred to an absolute beginning, seem only explicable on the hypothesis of special creation. After all, what is science now doing but somewhat extending the range over which uniform law may be *seen* to extend its influence (not the range over which it actually does so)? What an overwhelming thought it would seem to an ephemeron that a giant tree which, during the brief lives of millions of his race, had seemed scarcely to change except in its leaves or blossoms, had been developed to be what it has been during the continuance of generation after generation of his kind! Nothing in the widening of the domain of law which has been seen during the last century can be compared to the tremendous nature of such a revelation to a being who had regarded the pushing of a bud or leaflet as the limit of the operation of natural laws. And yet how little such a revelation compared to the conception that a whole forest had grown, and that the very earth in which it grew contained the remains of past generations of trees. Science is widening somewhat like this our conception of the extension of law. But the man who thinks that this widening of the domain of law means the rejection of a Law-giver; or that, by carrying back the operation of dependent causes a few steps or even (were that possible) a few millions of steps—we get rid of the necessity of recognising a First Cause, must be strangely-minded indeed. For such a one, the book before us will be useful: to those who view aright the operations of nature, it cannot fail to be interesting.

Two points have occurred to us as open to exception in this book. One is the reference to Tyndall's work on the subject of spontaneous generation, as a "curious retribution" (though Sir E. Beckett frankly admits it is creditable to the honesty of the most eloquent prophet of the doctrine of evolution). If there is one feeling which Tyndall has shown more strongly than another, it is the desire to come at the truth, and surely another description might be found for one of the most interesting of Tyndall's labours in the search for truth. Tyndall has found, indeed, that in a particular direction the beginning of life cannot be found. Darwin has shown that, in his opinion, no researches men can make will take them to the beginning of life, any more than the study of second causes will lead men to the First Cause. All this is in perfect harmony with the views of evolutionists—it is no retribution, but a confirmation of their views. The other point to which we would take exception is the remark thrown in more than once, that natural selection acts "for no cause, so far as we can see." The cause has been repeatedly indicated by evolutionists—this, namely, that those who have not the qualities in question die out; surely the death of those who have not such qualities is a tolerably good reason for the selection of those who possess them. We may note again that our author somewhat too confidently assumes that certain qualities could be of no use till fully developed; that, for instance, until or unless spiders made perfect webs, they might as well make none at all. We know that imperfect cell-making by certain orders of bees is better than utterly bad cell-making or no cell-making at all. Why should it not have been so in the past with spiders? A few lines of web might have been useful—even a single line, however short—in the earlier stages of the struggles for existence through which the Arachnida have passed.

* "Origin of the Laws of Nature." By Sir E. Beckett, Bart., LL.D., F.R.A.S., second edition. (London: Society for Promoting Christian Knowledge, 1880.)

THE FOOD WE EAT.*

THIS is a useful book, though Dr. Fothergill's instructions are not always so definite as they might be. He tells us roast mutton has a baneful history; but there is nothing in the chapter on flesh meat to suggest that we are too carnivorous; on the contrary, we read, "beef and mutton are the meats allowed by the trainer and prescribed by the physician; and the choice is a wise one, if not made too absolute." If it is suggested in one place that Cain killed Abel in a fit of irascibility brought on by gout—the result of flesh food—it is carefully explained in an editorial note that for this theory there is no authority in the Hebrew record. The rules for the taking of alcohol are sensible. They are these:—It is well to do without alcohol during the day: alcohol may be taken at bed-time, with advantage, by those whose sleep is broken by worry" (yet what poor rest whisky-bred sleep gives): "when a little wine or its alcoholic equivalent enables a person to take a little food when exhausted and digest it, which otherwise could not be taken, it is permissible." Taking "something" early in the day to set one up, is, as our author well says, the best way to destroy working energy, and alcohol is the worst possible resource against trouble. The narcotic dose recommended as at times a useful sleeping draught, is too much for any but confirmed topers—it is "a dose at least twice the amount usually taken in a tumbler." In the chapter on fruit, our author says that the raspberry is scarcely second to the strawberry; *de gustibus non est disputandum*, but many prefer raspberries. It was not "an irreverent American," by the way, but the Rev. Cotton Mather, a devout

* "The Food We Eat." By J. Milner Fothergill, M.D. (Griffith & Farran, London.) Price, 1s.

Calvinist, who said that "doubtless God could have made a better fruit than the strawberry, but doubtless he never had."

LANDAUER'S BLOWPIPE ANALYSIS.[*]

THE second edition of this very excellent little book, which has just (last November) appeared at Berlin, should have the effect of causing Freiberg to look to its laurels, or that time-honoured head-quarters of Plattner, Richter, and the blowpipe, will have to give place to Brunswick, where this work is written.

I believe Herr Landauer was the first, and is now one of the few German writers on this subject, to point out that blowpipe analysis properly studied, is a strictly chemical, qualitative process "in the dry way," by which the constituents of minerals, as of any other inorganic substances, may be, if possible, separated; and, if separation is impossible, at all events, exhibited so as to be easily and separately recognised in presence of each other. It is most creditable to Landauer's scientific conscientiousness and literary intrepidity, that he should venture to affirm and reiterate this great truth, in opposition to the teaching of such eminent chemists as the venerable Von Kobell, of Munich; of the "great shade" of Berzelius; and even of the departed Plattner himself. All these eminent writers, and some scarcely less respected than they in America, as Professor Brush, have laid down and taught what may be termed a "mineral course of analysis" as contra-distinguished from a chemical course, in which the reactions of minerals *per se*, and not *in partibus*—as a lawyer would call it—are given. Thus it happens that even our best mineralogies are defaced by such distressing descriptions of the blowpipe analysis of minerals, as "fusible on edge"; "melts to a blebby glass, &c., &c.

In England, I have been trying for the last ten years (a paper of mine on the subject was read before the Royal Society in 1872) to preach the same "dry," analytical doctrine; but, as might be expected, have been simply "pooh-poohed" for my pains here. It really seems, however, from the rapidly-succeeding edition of this book, and its translation into English and Italian, as though Landauer would succeed where I have failed, and teach English as well as German "blowpipers" better (analytical) manners in the future.

Unfortunately, it cannot be said that the Manchester translators of this work have been at all successful in rendering what the author has stated in German, into precisely equivalent English, or indeed, in some places, into anything like what he has said. For instance, at page 10, Landauer says of my aluminium plate reactions: "The best substitute for charcoal is the aluminium plate introduced by Ross";[†] which plain sentence and note, the Manchester transmutationists have replaced by the following ingenious rendering. "As another kind of substitute for charcoal, aluminium plate may be advantageously employed." Many other instances might be pointed out, if space permitted.

It must be admitted that Landauer, with considerable reference to the American writers Elderhorst and Egleston, to his celebrated countryman Bunsen, and to myself, has contrived a "strictly chemical" system of blowpipe analysis, so far as a persistence in the use of the stupid reagents borax and microcosmic (or, as it is now called, "phosphor") salt, permitted. But such a system with these reagents is simply impossible; and Sorby, Wunder, Rose, and others have long ago shown that it is equally impossible to obtain *definite* pyrological crystallisations from the fusion of oxides, &c., in borax before the blowpipe. The plain fact is, as I pointed out ten years ago in my paper (vol. xx. "Proceedings Royal Society") that boric acid will *separate* the constituents of most minerals and inorganic chemical combinations before the blowpipe; whilst borax, or any other reagent, will not. Landauer's book is divided into four chapters and a reprint of Plattner's well-known blowpipe tables. In the first chapter (of fifteen pages) a description of apparatus and reagents is given. In the second (forty-six pages), an excellent account of the necessary operations, including my "Aluminium Plate Reactions" (ten pages). An "Appendix" to this chapter is devoted entirely (eighteen pages) to "Bunsen's flame-reactions," the connection of which, by the way, with the blowpipe, it is difficult to perceive, but it is placed in the book and "table of contents" as the third chapter, though called an "appendix." The so-called third is headed—after the arrangement in Scheerer's and Blanford's little book—"Special Examination for certain Combined Substances," whereby the reader is left to infer that all the other examinations, in many cases the best and most careful, are *not* "special." The fourth chapter describes the author's and [illegible]ton's "Systematische Gange" (very good Scotch as well as German) or [illegible]; which are both excellent in their way.

At page 3 is given a beautiful woodcut, with a remark that it is to be specially recommended, of what is called "Ross' Standlöthrohr" (Ross's stand blowpipe), which is not figured in the Manchester translation (1875), because it was not invented by me till 1880, in September of which year a description, with drawing, appeared in the *English Mechanic*. It is, in fact, a mouth-blowpipe with elastic air-reservoir and valve attached, precisely the same as that recently described by me in KNOWLEDGE, vol. i., page 137, only the uncomfortable addition of a huge metallic tube cover kind of protection to the india-rubber balloon is made, by which all portability and simplicity are ingeniously destroyed. This seems to me too great a sacrifice to make for the sake of [illegible] bags, which ought not to cost more than a farthing each. I feel sure that if M. Landauer sees this notice, he will at once repudiate all knowledge of my invention when he inserted Ross's drawing and description in his book, which I can cordially recommend to your readers as the best extant on the old system of blowpipe analysis.

[*] "Die Löthrohranalyse, Anleitung zu qualitativen chemischen Untersuchungen auf trockenem Wege, bearbeitet von J. Landauer. Zweite vermehrte Auflage." (Berlin: Verlag von Julius Springer, 1881. London agents: Trübner & Co.)

[†] Ross, "Pyrology, or Fire Chemistry." London: 1875.

SCIENCE FOR ALL.

MESSRS. CASSELL, PETTER, & GALPIN call attention to a misprint in the footnote to the review of the last volume of their "Science for All." The price of the volume is not 5s., but 9s. We note that no price is mentioned, but thought we remembered seeing the work advertised for the price named; also, the book sent us was, it appears, the fourth, not the fifth, volume. We took it for granted the latest had been sent. Our remarks apply to Volume IV., not to Volume V. The volumes are unnumbered. Mr. Denning writes (unfortunately at much too great length for insertion), noting that many of the highest authorities—Sir J. Herschel, Webb, &c.—accept the eccentricity of the ring-system as an established fact. That the rings have at times been eccentrically situated is well known; at other times there has been no recognisable eccentricity. Mr. Denning seems to think that saying the Saturnian rings "will be seen at night as a vivid semicircle of light" needed no correction, although the rings are absolutely invisible from large portions of them, not visible as a vivid semicircle at any time from any part of Saturn except the equator, and not visible from the northern hemisphere during one half of the Saturnian year, or from the southern hemisphere during the other half. We venture to say that every reader would suppose Mr. Denning's statement to mean that if you were placed anywhere on Saturn, you would at night see the rings as a vivid semicircle of light. We submit that the statement is as incorrect as the following would be:—On the earth, the sun is visible for twelve hours out of the twenty-four. We are sorry if Mr. Denning objects to being set right in a matter about which there are no two opinions among persons competent to form an opinion at all—that is, in this case, among mathematicians (for the question is purely mathematical).

NIGHT MINIMA OF ALGOL, 1882.

THE following list of night minima of Algol, by the eminent observer of Variable Stars, Mr. Jos. Baxendell, will be much valued by many readers.—ED.

GREENWICH MEAN TIME.

		H.	M.			H.	M.
Feb.	18	13	22	Sept.	4	9	35
"	21	10	10	"	21	14	29
"	24	7	0	"	24	11	17
March	10	15	4	"	27	8	6
"	13	11	53	Oct.	11	16	11
"	16	8	42	"	14	13	0
April	2	13	35	"	17	9	49
"	5	10	24	Nov.	3	14	42
"	8	7	13	"	6	11	31
"	22	15	7	"	9	8	20
"	25	12	6	"	23	16	24
"	28	8	55	"	26	13	13
July	6	10	51	"	29	10	2
"	29	12	33	Dec.	2	6	51
Aug.	9	14	15	"	13	18	7
"	12	11	4	"	16	14	55
"	29	15	56	"	19	11	44
Sept.	1	12	46	"	22	8	33

JOS. BAXENDELL.

THE MENACING COMET.

By the Editor.*

"SUPPOSING Mr Proctor's facts to be correctly stated, says the *Spectator*, "there does seem a remarkably good chance that in 1897 the sun may suddenly break out into the same kind of intensity of heat and light which caused the conflagration in the star in the Northern Crown in 1866, when for a day or two the heat and light emitted by it became suddenly many hundreds of times greater than they were before." The *Spectator* is exercised by the inquiry whether the world's belief in science is quite so genuine as it seems, seeing that "Mr. Proctor's warning has not yet caused the world to make any change in its arrangements." Without undertaking to say what change the world should make in its arrangements if its end were to come in a few years, I may remark that my warning—such as it was—appeared in an Australian Review, and was not published in this hemisphere until a very few weeks ago (the preface to the volume is dated December, 1881, and the title-page bears the date 1882), so that the wonder rather should be how my terrible prediction comes so soon to be frightening fearful folk from their customary quietude. If it were not that his Right Reverend Lordship the Bishop of Manchester had been chiefly instrumental in calling general attention to the prediction, the world might well imagine that the scare was a well-designed puff for my new volume, in which case I might be anxious to explain that, according to the terms between myself and Messrs. Chatto & Windus, I could not possibly gain, and might conceivably lose, by the rapid sale of the work at this present time.

Be the cause what it may, I find that I am generally understood to have issued a prediction that, somewhere about the year 1897, this world, with all that it inherit, shall be dissolved by fervent heat. Let us see what the article referred to by the *Spectator* really says:—

In its opening paragraph, I state that views advanced respecting the comet by others, "not by fanciful theorisers, but by mathematicians of eminence, suggest the possibility, nay, even some degree of probability, that this comet may bring danger to the solar system." And I go on to say that it is that possibility which "I have to discuss." The possibility, even some degree of probability, that a comet may bring danger—this possibility suggested by the views of others, and to be discussed by me—does not, I apprehend, amount to a definite statement on my part that there is "really a very considerable chance of a catastrophe fifteen years hence, which may put an end to our earth altogether." Let us, however, examine the article further.

Fig. 1.—The Menacing Comet.

I go on to show that the path of the comet of 1880 carried it singularly near to the sun. This, of course, is simply a scientific fact. I next explain that the observed part of the track of the comet of 1880 coincided, or nearly so, with that of the comet of 1843; but that whereas the most trustworthy calculations of the orbit of the comet of 1843 assigned a period of about 175 years, the observed period of its last circuit—if that object and the comet of 1880 are really identical—was only 37 years. This part of the inquiry is more theoretical than the former. Still, the evidence is such as to make it highly probable that the comet of 1880 really is one and the same as the comet of 1843, and that there really has been a diminution of the period of revolution from more than a hundred to less than forty years.

It is towards the close of this part of the inquiry that the anticipation of the comet's return in 1897 is referred to. As presented by the *Spectator* and the Bishop of Manchester, this might be supposed to be such a prediction

* I should esteem it a favour (though I think I might almost claim it as a right) if those newspapers who have spread the news of my supposed prediction, would be good enough to explain that I believe the world is more likely to last fifteen millions of years than to be destroyed in fifteen.—R. A. Proctor.

as, for instance, I made in 1868 of the epochs of the beginnings and endings of the transits of Venus in 1874 and 1882 for different parts of the earth's surface. As a matter of fact, the prediction is not mine, but Herr Marth's (quoted, and, as a quotation, given in smaller type than the rest); it is not advanced definitely, but in the following terms:—"I should not be at all surprised," says Herr Marth, "if it should turn out that this comet of 1880 is the same as the comet of 1843 and that of 1668, and that its revolution has been so much affected that possibly it may return in, say, seventeen years." (This was written in 1880.)

I go on to show that if this is so, the comet must before long be absorbed by the sun—still not naming 1897 or any other year, but speaking with due scientific caution "after only a few circuits—possibly one or two."

Fig. 2.—Another comet which might have been dangerous if it had gone the wrong way.

I then note the only way in which the absorption of a comet might do harm—that is, not as Newton thought, by adding fuel to the solar fires, but by the conversion of the momentum of the meteoric masses forming or following the head, into heat. I mention, in passing, my own belief that the sudden increase of splendour observed in the star T Coronæ (not τ, as has been mistakenly asserted) was due to the fall of a large comet, followed by a train of closely-aggregated meteors upon that distant sun. This I fully believe to be the most probable, if not the only available interpretation of that and similar phenomena. "Without saying," I proceed, and this seems to me the only passage in my essay which could have suggested any anxiety about the earth's future, "without saying that I consider there is absolute danger of a similar outburst in the case of our own sun, when the comet of 1843 shall be absorbed by him (a result which will, in my opinion, most certainly take place), I will go so far as to express my belief that if ever the day is to come when 'the heavens shall dissolve with fervent heat,' the cause of the catastrophe will be the downfall of some great comet on the sun." What I here consider as certain may, perhaps, have been misunderstood as the coming of such a catastrophic end; but it should be manifest that I only regard the absorption of the comet of 1843 as most certain, regarding the time as quite uncertain, and the effects as extremely problematical. I have, indeed, shown elsewhere (see "Suns in Flames," in my "Myths and Marvels of Astronomy") that there is every reason for believing that all comets of the destructive sort have long since been eliminated from the solar system. So that, as in the essay referred to by the *Spectator*, I refer back to an essay in my "Pleasant Ways in Science," in which essay I refer back to the other in "Myths and Marvels," without in either case indicating any change

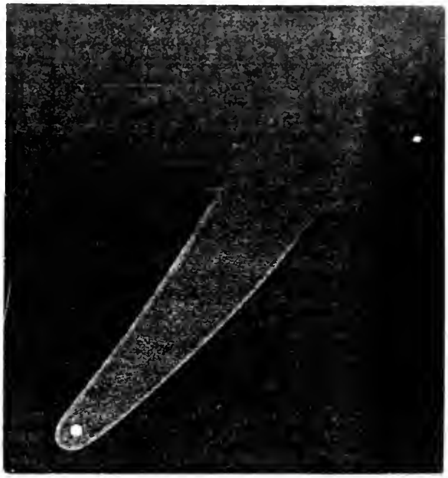


Fig. 3.—A comet which might injure a solar system, though not ours.

of view, I might fairly claim to have very definite views as to the perfect safety of the solar system, even if I had not recently pointed out, with special reference to the comet of 1843, our probable complete immunity from danger. In the *Cornhill Magazine* for December last, there is a paper, bearing my initials, on "Dangers from Comets," in which it is shown (as sundry newspaper articles have been good enough to explain in turn to myself) that if there were any real danger, save for the comet itself, we should have known it by great increase in the solar emission of heat in 1843, when the comet was checked so importantly in its career, and again in 1880, when it was subjected to another equally severe interruption of its onward course.

The article finally points out the kind of danger which in all probability would ensue if a comet of the larger sort fell into the sun. If there is anything remarkable in this part of my essay, which seems to have been regarded as the most sensational, it is its extremely cautious wording. I may go so far as to poke a little fun at myself

by saying that it is almost absurdly cautious. I point out that if there are planets circling around the sun which blazed out in Corona in May, 1866, to eight hundred times its former lustre, and if there were living creatures on them at all resembling ourselves, those creatures must most certainly have been destroyed. It takes no wizard to know this. I then go on to say that "if at any time a great comet falling directly upon the sun" (which the comet of 1843 and 1880 most certainly will *never* do) "should, by the swift rush of its meteoric components, excite the frame of the sun to a lustre far exceeding that with which he at present shines, the sudden access of lustre and of heat would prove destructive to every living creature, or, at any rate, to all the higher forms of life, upon this earth." And though I knew when I wrote this that I was making no rash prediction, I protest I never noticed until the rash predictions assigned to me by the *Spectator* and the Bishop of Manchester set me reading over my own essay, that this amounted only to an announcement of the following highly-impressive nature:—If such a comet as we have no reason to suppose actually existent (nay, every reason to consider certainly non-existent in the sun's case), should produce a degree of solar heat (which such a comet may, or may not, be capable of producing), exceeding hundreds of times the sun's present heat, and if that heat lasted but a few days, the earth's inhabitants must all perish. This very cautious announcement does not mean, I venture to point out, that fifteen years hence the comet of 1843 falling into the sun will so raise his heat that all of us will be destroyed.

I may remark that the newspaper announcement has elicited various expressions of opinion, showing the great ignorance which prevails even in these days of cheap scientific literature respecting scientific matters. Thus it has been carefully explained by some that comets are entirely vaporous, evidently in ignorance of what has been learned respecting the meteoric nature of comets; by other writers, that Lexell's comet was absorbed by Jupiter or by his satellites (which Leverrier entirely disproved); while another writer (in the *Christian World*, and followed by the *Globe*) propounds the amazing statement that the sun's heat does not travel so quickly as his light, so that even though we saw a great outburst, due to the destruction of a comet, some ninety or a hundred years would have to pass before the earth would receive the heat then generated! It would be interesting to ascertain whence this singular idea was obtained by what strange misapprehensions of some statement in a scientific work. Of course, there is not the slightest foundation for it. The sun's heat comes to us with his light, not only travelling at the same rate, but being a part of the very same undulatory disturbance, and a considerable portion being derived from the very same waves. Some of the waves, indeed, which affect us as light affect us very little as heat, and some of the waves which affect us as heat, produce no effect which the eye can appreciate as light. But the orange and red light-waves are very active as heat-waves too, and there is not the slightest reason for supposing that the so-called dark heat waves, which, with these, make up the total supply of solar heat, would lag many seconds behind them on the journey earthwards.

However, there is not the slightest reason to fear that the comet of 1843 and 1880 (assuming they are the same) will do any harm to the solar system when finally absorbed. It would be quite otherwise, I believe, if such a comet as that of the year 1811, Fig. 3, were to fall directly upon the sun. This, the most remarkable (in reality, though not in appearance) of all known comets (see Knowledge, No. 5, p. 86), was fortunately some 100 million miles from the sun at the time of its nearest approach to him, and can never bring the slightest trouble to the solar system. But if its course had chanced to be directed full upon the sun, the meteoric masses doubtless forming its head and train (not tail), falling in countless millions upon him at the rate of more than 300 miles per second, when they crossed his visible surface, and probably passing deep below that surface with ever and most rapidly increasing velocity to reach his real nucleus, would have generated an intensity of heat far exceeding that which he constantly emits. The increased emission might not have lasted a month, or even a week, but it would have sufficed.

So, again, what we now know of cometic structure leads us to believe that the comet of 1858, called Donati's, whose head is shown in Fig. 2, would have proved a very dangerous visitor had its course led it directly towards the nucleus of the sun. Fortunately, the chance of any comet visiting our system from interstellar space, travelling directly towards the sun, is so small, that it may be reckoned "almost at naked nothing." As to comets already belonging to our system, if any such have orbits passing very close to the sun, so as to be checked in their career at every perihelion passage, it is clear (from the continuance of life during many hundreds of thousands of past years on the earth) that the mischief must long since have been taken out of them—unless we suppose (which is incredible) that the last perihelion passage of such a comet preceded the beginning of life on the earth.

THE EFFECTS OF TOBACCO.

By Dr. Muir Howie.

PART II.

In the present day, we can calculate with precision the exact time, to a minute fraction of a second, which is required to transmit a message from the brain to the hand or any other portion of the body; and it has been distinctly shown that it takes much longer to send such a message after the person experimented upon has taken even a small dose of a narcotic. A message which could be sent in 0·1901 of a second, required 0·2970 of a second for its performance after two glasses of hock had been administered to the subject of experiment, thus showing how much even a slight narcotic interferes with the vital action of nervous tissue. The same effect is produced by tobacco. Tobacco prevents waste of tissue, and thus enables a man who smokes to live on less food. This is considered a very strong argument in favour of the pipe; and if good food could not be obtained, it might have very great force. But plain, wholesome diet is cheap and easily procured. Moreover, "waste of tissue" is an expression which conveys an utterly false impression. There is no such thing as waste of tissue, unless the body is wearing away more rapidly than new substance can be reproduced, as in certain fevers, consumption, &c. The tissues of the body are not a fixed quantity, like the framework of a steam-engine; they are ever changing, the old wearing away to be replaced by the new. Life is a constant series of changes; and the healthier the man the more rapid, within certain limits, will be his change of tissue. You can only preserve the tissue of a healthy man by lowering his vitality; the tissues thus preserved cannot bear the strain which can be borne by those of recent manufacture, and thus the working power is diminished. An employer of labour in Liverpool, anxious for the elevation of his workmen, suggested that they might with advantage give up the use of beer and tobacco. They informed him, however, that in such a contingency their wages would not support them, so great would be their increase of appetite. But there is another side to this question, and it is, that such men would be able to do more work, and consequently earn larger wages, by discontinuing the narcotic. Men of all classes are very slow to learn that sound bodily health is the best possible investment. The human machine is very easily kept in order, but once let it get out of repair, and it is most difficult to set right. And it can only be kept in thorough repair when every joint, muscle, and nerve is maintained in a condition of persistent activity. I do not mean that a man should always be engaged in exercising his various tissues and organs in order to preserve health; but I do maintain that every tissue should be so actively exercised that it will be compelled to employ its entire

time of so-called rest in laying up fresh stores of explosive material, and in healing up those rents which have taken place in their actual substance. In the region of nerve and muscle a man ought always to live up to his income. He can save nothing by sparing exertion, so long as he does not go beyond his income. Give your brain sufficient food and an abundant supply of oxygen, and then give it a fair amount of good hard work every day, if you wish to maintain it in a high state of healthy activity. Barristers and clergymen, who use their brains much are the longest-lived men in the country, showing plainly that regular brain work is good for the general health as well as for the efficiency of the nervous system in particular. The muscular system must be treated in a similar manner, if you do not wish it to become subject to fatty degeneration. An unused muscle shrinks, and becomes soft and flabby, presenting an appearance of marked contrast to the brawny arm of the blacksmith. Instances of the feebleness of tissues thus preserved frequently present themselves to the notice of the surgeon. A muscle is called upon to perform a vigorous contraction, but it snaps in the effort. The heart itself is sometimes torn asunder in attempting to send an extra supply of blood to some needy limb. No man can afford to lower his general vitality for the sake of mere idle gratification. He never knows when he may require all the energy which can be stored up in his tissues. A railway accident, a runaway horse, a run to catch a train, a fall on the ice, or even a fit of coughing, may bring a life of misery or an early death to one who would have passed unscathed through them all, had he allowed his nerves and muscles to wear away in vigorous activity, instead of carefully preserving them, like smoked bacon, in the fumes of tobacco. I do not attempt to deny that all narcotics possess the power to prolong life in the absence of food. I have elsewhere referred to the case of an old woman who lived for two years on opium and gin-and-water, without any food whatever, but she might as well have been in her grave. Hers was, I would not say a living death, but rather a dead life. Some may be inclined to doubt the accuracy of this story, but such will discern a possibility of its truth when I say that a narcotic seems to produce a condition of the nervous system closely resembling that of hybernating animals. The dormouse sleeps for many weeks without any food whatever. Its tissues are then in the condition of the cook's fire when covered with ashes, and if you can produce a similar condition in the human tissues, you may attain the same result of prolonged fasting. We are apt to consider the winter sleep of the dormouse as a great waste of existence; but what can we think of a reasonable man who artificially reduces himself to a similar condition during a considerable portion of the prime of life.

Tobacco soothes the exhausted and irritable nervous system after a hard day's work, and prevents the brain worrying about difficulties that may never come.

The advocates of tobacco maintain that in this manner it gives rest to the nervous system, and thus enables it to throw off work for the time, and resume it again with renewed energy. Now the mistake which our opponents make here is, that they ignore the necessity for anything but rest. What would you think of the farmer who allowed his men an hour's rest at various intervals during the day, but who, at the same time, forbade them to take food at such times, lest the muscular movements involved in carving and mastication should interfere with their complete and absolute repose? Every cell in the body is a counterpart of the whole organism. Just as the man cannot work without eating, so the cell cannot carry on its explosive action without fresh supplies of explosive material. Now, tobacco and other narcotics not only prevent the nervous matter exhibiting energy, they also prevent it absorbing its proper food; so that the rest which it obtains by means of narcotism does not enable it to resume work with renewed energy. But more, the nervous matter is thereby rendered incapable of throwing off its own ashes, which are its most deadly poison. Just as decomposing animal matter is highly deleterious to the health of the body, so the dead portions of nervous tissue become disastrous to the life and activity of their living successors. I do not attempt to deny that the relief afforded by a narcotic is most delightful and seductive. When the merchant goes home from his office, worried by a thousand trifles, and saddled with a load of cares, his nerves are agitated and restless, and the busy wheels of life seem to spin round with unceasing velocity. How delightful it is to be able, by the magic spell of tobacco, to stop those busy wheels, and to translate himself from the pains of a commercial pandemonium into the Elysian fields of perfect bliss! I confess that tobacco does all in the way of soothing that its admirers attest; it is my duty, however, to exhibit the other side of the shield, and to proclaim that the luxurious pleasure of the pipe is physiologically so expensive that the nervous system cannot afford to indulge in it. The muscles suffer along with the nerves; for without nervous influence the muscles are unable to supply themselves with the nourishment which is carried by the blood into their very substance. If you cut the nerves leading to a muscle, that muscle will cease to retain its firmness and contractive vigour, and if you paralyse the same nerves by a narcotic, its power of contraction will be similarly diminished. Any smoker will tell you that much smoking is a hindrance to severe muscular exertion. If a man has lit his pipe, you are more likely to find him dreaming in a corner than ascending a mountain. When you observe what an amount of lounging lethargy is induced by tobacco, you scarcely require an ounce of science to account for the smaller appetite of the inveterate tobaccophile. This power of the narcotic to interfere with the nutrition of the tissues produces serious consequences on the digestive organs of those who both smoke much and eat well. The smoker is often not content to suffer any diminution of the pleasures of the table as a result of his pipe. He therefore uses a variety of agents to induce in his digestive organs an artificial appetite. He is thus led to consume a much larger amount of nutritive material than can possibly be required by narcotised tissues. This nutritive material produces injury either to the stomach or liver—very frequently to both. The stomach is burdened with more work than a smoker's stomach can perform: hence the dyspepsia so frequently accompanying the pipe. The liver is doubly burdened. Its duties in connection with the food are many. It assists to prepare nutriment for nerve and muscle, and if such nutriment is not required, its further duty is to break down such rejected nourishment in order that it may be more easily expelled from the system. Hence the biliousness and other effects of liver derangement so common in the smoker.

Tobacco destroys the physical conscience.

My greatest objection, as a physician, to the use of tobacco, is, that it destroys what I have ventured to call the physical conscience. The entire body is supplied with minute nerve twigs which, in the healthy man, are maintained in a highly sensitive condition. Their function is to inform the brain when any derangement is taking place in the ultimate tissues. This network of nerves occupies a similar position in relation to physical health that the conscience does in relation to the moral condition. Whenever any muscle has difficulty in contracting, a message of the fault is at once transmitted to the brain. The same occurrence takes place when the stomach has difficulty in digesting its contents, when the liver is overburdened with excess of sugar or bile, and when the brain is being overtaxed with daily toil. These messages produce great uneasiness to the subject of their influences, just as a troubled conscience does in the mind of its possessor. Now, there are two ways of avoiding the inconvenience of the physical conscience, just as there are two ways of avoiding the pangs of a smitten moral conscience. You may either do what is right, or you may lull your conscience to sleep. Tobacco enables a man to deaden his physical conscience, and thus he may go on ruining his health without knowing it, until he is beyond the hope of recovery.

HOW SPIDERS FLY.

(BY PROFESSOR C. A. YOUNG.)

I WAS very much interested, a few days ago, in hearing a friend give an account of a manuscript she had seen, which was written by Jonathan Edwards when nine years old. It was an account of the behaviour of certain small New England spiders, the manner they fly through the air, and the way to see them best, by getting into the edge of a shadow, and looking toward the sun. It is neatly and carefully written, and illustrated by little drawings, very nicely done. The philosophical tendencies of the young writer already appear, for his conclusion as to the "final cause" of spiders and their flying is this: the little animals are scavengers, and since, in New England, the prevailing winds are west, they are carried to the sea in their flight with whatever filth they have consumed, and so the land is cleansed.

Every one knows how, in sunny weather, the little creatures, standing on their heads, project from their spinnerets fine filaments of gossamer, which are caught by the breeze, and float off into the air, though still attached to the spider. When she perceives that the thread is long enough, and the pull of the wind sufficient, she releases her hold and flies away on her gossamer like a witch on her broomstick; by watching her chance, and letting go only when the breeze is favourable; she is carried to her desired haven. Experiments have been tried by placing the animals on a chip floated in a pail of water. So long as the air was in motion about them they were able very soon to escape from their island; but when a bell glass was placed over the pail, thus preventing air currents, they could not get from the island to the surrounding shore.

But how does it happen that, on setting out for a voyage, the spider almost invariably *ascends* with her web, and continues to

rise, until, by pulling on her thread, she reduces her floating power, and so comes down. Spider web, in and of itself, is not lighter than air; how, then, is its buoyancy to be explained?

In two ways, I think. When the sun is shining, every projecting object, like a twig or stick, absorbs heat more rapidly than the air, becomes warmer than the air, and thus acts like an independent source of heat in generating an ascending current, so that when the spider lets go her hold, she and her thread are carried up partly by the action of this current.

But this is not all; unless I am much mistaken, *the action of the sun's rays on the thread itself and its surrounding envelope of air* is the main cause of its buoyancy. Air is nearly diathermanous, or transparent to heat, so that the solar rays, in traversing it, warm it only slightly. The spider's thread is not so, but in the sunshine warms up almost instantly, heating the air in immediate contact with it; and then, although the spider thread alone is heavier than air, yet the thread and the adhering envelope of warmed and expanded air taken together, are lighter than the same bulk of the cooler air around, and thus constitute a quasi-balloon, on which the spider sails away. Of course, if this is so, the poor creatures cannot sail much on cloudy days, and I think, in fact, they do not.

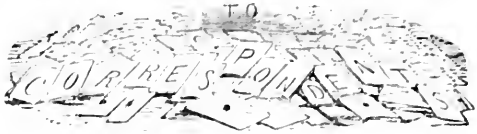
I have tried a few experiments to verify the idea, and so far as they go they all confirm it. For instance, one day in the autumn of 1880, when the air was full of floating gossamer, and there was no wind blowing, I caught some of the filaments at the end of a little stick, to see how they would behave. So long as I stood in the sunshine, they streamed straight upward, tugging with almost a breaking strain; as soon as I stepped into the shadow of a building, they lost their spirit, and drooped abjectly; the moment I put them in the light again, they resumed their buoyancy. It is, of course, possible that in the shade there were local downward air currents to account for their behaviour; but once a cloud passed across the sun, and they drooped then, just as they did behind the building.

The same theory will explain the buoyancy of any minute particles of dust or smoke. So long as the sun shines, they will absorb its rays, become warmer than the air, and surround themselves with a buoyant envelope, which will carry them up if they are not too heavy in proportion to their surface. But if the air is still, and the sun obscured, they will settle down near the earth, in the way we are all familiar with in muggy weather. Of course, if there is much wind, this will mainly control their movements, and neither their buoyancy in sunshine, nor their gravity in shadow, will be particularly noticeable.—*Boston Journal of Chemistry.*

INTELLIGENCE OF DOGS.

While at the University, taking my medical course, the facts I relate took place. Among other appurtenances to the department of physiological chemistry was a dog with a gastric fistula, which fistula was properly healed around a silver tube having an internal and external flange to keep it in place. The tube was stopped by a closely-fitted cork, except at such times as we needed a supply of gastric juice. The fistula caused the animal no disturbance whatever. He was well and hearty, was fed at and made his home at the medical department.

During the summer vacation, however, when the University was closed, he was transferred to the care of the surgeon, who took him to his house. During his frolics one day he jumped over a fence, striking it, and dislodged the cork in the tube. Ponto soon noticed that his food didn't seem to satisfy him, and that all he drank ran out of his stomach on the ground. His master having gone away for several days—fishing—he must needs take care of himself, so immediately on eating or drinking anything, he ran to his bed in the carriage house close by, turned on his back, and remained so for an hour or more, or until he felt satisfied that it would do for him to get up. Coaxing, threatening, and kicking by the domestics about the house, or by those whose attention was called to his actions, were alike unavailing to drive him from his place or from his supine position. Finally, some one who knew for what purposes the dog was used, examined his fistula and found the cork gone. This being restored, he was soon persuaded to go about as usual, and indicated by his actions that he understood that everything was all right. This incident can be vouched for by many reliable persons. Who will say that dogs—at least one dog—cannot reason? —F. L. Bardeen, M.D., in *Scientific American.*



Letters to the Editor.

[*The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with their writers. All communications should be as short as possible, consistently with full and clear statements of the writer's meaning.*]

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*** *All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.*

All Letters or Queries to the Editor which require attention in the current issue of Knowledge, *should reach the Publishing Office not later than the Saturday preceding the day of publication.*

(I.) Letters to have a chance of appearing must be concise; they must be drawn up in the form adopted for letters here, so that they may go untouched to the printers; private communications, therefore, as well as queries, or replies to queries (intended to appear as such) should be written on separate leaves.

(II.) Queries and replies should be even more concise than letters, and drawn up in the form in which they are here presented, with brackets for number in case of queries, and the proper query number (bracketed) in case of replies.

(III.) Letters, queries, and replies which (either because too long, or unsuitable, or dealing with matters which others have discussed, or for any other reason) cannot find place here, will either be briefly referred to in answers to correspondents, or acknowledged in a column reserved for the purpose.

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

"God's Orthodoxy is Truth."—*Charles Kingsley.*

Our Correspondence Columns.

OPTICAL ILLUSION.—CAT'S EYE TIMEPIECE.

[279]—In the long string of optical illusions which have appeared from week to week in your esteemed paper, I am surprised the following has not been mentioned:—Fix an ordinary fork in the wall, and on the handle balance a small cork. Having shut the right eye, walk towards the cork and endeavour to knock it off with the little finger. It is very seldom that the cork is displaced upon first trial.

Your account of the remarkable ingenuity of the Chinese in calculation reminds me of the peculiar manner in which they are able to approximately tell the time, no matter whether the day is cloudy or dull. They will run to the nearest cat, open her eyes, if they are not already open, and will at once inform you, with a certain amount of accuracy, what time it may be; all depending, of course, upon the contraction of the iris or the size of the aperture of the pupil of the eye. What I cannot understand in connection with this process is, why the clouds in interrupting the sun's light have no effect upon the cat's eye? But I suppose the Celestial land knows not what fogs and mists are, and therefore we should not be able to avail ourselves of the advantage of feline clocks here. Even if it were so, I question whether pussy would submit with such grace as the cats in the land of the pigtail seem to, to an operation which must of necessity be far from agreeable to her.—Yours, &c.,

Ernest J. Wernham.

WEATHER FORECAST.

[280]—I noticed in one of the numbers of your most valuable publication, viz., Knowledge, some remarks anent the constant inaccuracies in the weather forecasts of our Meteorological Office, and impugning the utility of them and the cost to the nation, out of all proportion to the benefit derived. As regards our own district, viz., North Wales, we are coupled with Lancashire and N.W. England, some ninety miles off, notably one of the wettest districts in England, and not one in twenty of the forecasts is correct as applied to us. I tested them for fourteen days, and not one was correct, which I forwarded, in a tabulated form, to the office, giving on one side their own forecasts and on the other the actual weather we had experienced, and stated my opinion of the uselessness of such forecasts and the injustice of tacking us on to a district so far away; in due course I received a reply from the secretary, which I am sorry I destroyed, otherwise you should have seen

the lame excuses he made for the inaccuracies complained of; but it amounted to this:—"That if I could tell them how to cast the weather for every subdivision of the Kingdom, I should be very clever, as of course the climate varied in different districts from local causes, such as mountains, &c., and it was left to each district to modify the forecasts so as to suit their different localities"; but if so, of what earthly use are they for reference? We all know the weather we are having in our own particular district, but very often we want to see what kind of weather it is where we want to go to; better, I said to the secretary, leave us out altogether, or in justice to the district let us be classed in a district to ourselves, say "Holyhead and North Wales." The secretary, I forgot to say, took the trouble to send me a tabulated statement of what the weather had been during the fortnight alluded to at Barrow-in-Furness, and which is situated some 130 miles due north of us, and which I considered had no bearing at all upon what I had complained of, and was begging the question entirely. As you say, these daily forecasts are not to be depended upon, and are apparently only a matter of guess work, and so had better be dropped, as for reference and utility they are proved to be utterly useless.

A CONSTANT READER, AND A RESIDENT IN THE PRINCIPALITY.

DREAMS.

[281]—The following incident would seem to corroborate a view which I hold—namely, that some dreams have turned out, and do turn out, to be foretellings,' &c.; and that science cannot possibly—at least, for the present—give any satisfactory explanation of them.

A gentleman friend of mine, whom I shall call A., knows a young lady B., whose house is situated in a crescent, about three miles from A.'s dwelling; he is pretty well acquainted with the neighbouring district, but is at a loss to make out where this crescent lay.

I would not for one moment doubt the veracity of his statements, and this is what he relates:—

"I dreamt that I was walking up some road, of which I have a faint recollection; it seemed about eventime. As I was going along, I came to a bye-road, where I saw B. walking on the right-hand side, and then enter into the third or fourth house. As soon as B had disappeared indoors, I seemed to run up to the house to have a look at it." . . . . And then he continues, "I suddenly awoke, and found myself on a chair sketching a house." A, on writing to the young lady the following day, told her his dream, and sent her the sketch at the same time.

Now, it turns out that what he dreamt really did happen, for on the previous evening B was just returning to her house, which, by-the-bye, is the fourth in the crescent (the bye-road would correspond to the crescent), and what is more strange, the sketch is such, that any one who has seen the house would immediately recognise it on the paper, and besides, there is a characteristic tree just before the house, which is reproduced in the sketch.

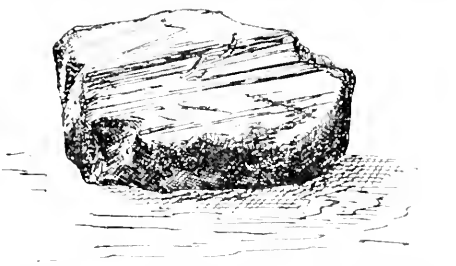
It is true that this bears the testimony of one man only; for no one but himself can tell whether he had ever seen the house before or not; he denies having seen it.

For myself, I consider the thing possible; but nevertheless wonderful and unaccountable. Perhaps one of your readers may furnish a clue to the explanation of a fact which puts me in mind of the story of Bach, the musician.—Yours, &c., REVELER.

ICE AGE IN BRITAIN.

[282]—There are many proofs of the existence of an ice age (Query 192) over northern Europe, England as far south as the Thames, Scotland, and Ireland. The undulating outlines of the smaller hills, and the lower parts of the great ones, show the smoothing action of vast sheets of ice moving slowly over the land, and grinding down minor irregularities and abrupt eminences which came in their way, out of the *débris* of which was formed the thick tenacious clay usually found a few feet below the surface in the valleys and plains. When examined, this clay is seen to contain stones varying in weight from a few ounces to several tons. Most of these stones are subangular, *i.e.*, have their sharp edges and angles worn down and blunted, while upon their surfaces, which are more or less roughly polished and worn by friction, are scratches, some so fine as to be seen only with a magnifying glass, others being deep grooves. When such stones are oblong, the scratches are mostly in the direction of greatest length.

This rubbing down and scratching has been produced by the grinding which went on between the ice and the rock-surfaces over which it moved. The rock-surfaces in many places are scratched by the stones which have been dragged over them, and these marks show the line in which the ice moved when they were made. The direction towards which the ice moved can often be ascertained by observing glaciated stones of a different formation to that on which they are found, so that if a glaciated piece of granite is seen in a limestone district, one looks for the nearest granite beds, and if the majority of scratches on the limestone rock point in that direction, it is nearly sure to be the source whence the granite fragment was



taken by the ice. These are only some of the signs of ice work. The great basins in which lie the lakes of Cumberland and Westmoreland have been, at least partially, excavated by glaciers. This drawing is from a glaciated limestone pebble, and gives a good idea of what "Arachnida" should look for. E. C. R.

AT THE NORTH POLE

[283]—What is the aspect of the heavens, when the sky is clear to an observer situated at the North Pole?

Assume the epoch to be noon at Greenwich on February 3, the date of the last number but one of KNOWLEDGE. The sun will be 16½° below the polar horizon; there will, therefore, be a dim twilight. The moon will be 10° above the horizon, and just past the full. Jupiter, Saturn, and Mars will hold their relative positions as with us, but will maintain day after day a nearly constant attitude equal to their declinations (Jupiter 16°, Saturn 11½°, Mars 27°). The constellations, together with the planets and the moon, will appear to sweep round the horizon in their daily course, the moon alone showing any appearance of setting. Her altitude will decrease about 10', or one-third of her apparent diameter, every hour, and after two or three diurnal revolutions, she will set below the polar horizon somewhere over the mountains of Greenland, if such are in view. The monotony of such a scene would be excessive. It would probably convince the most sceptical that the earth does really turn upon its axis.

Celestial observations for the verification of the position could be taken with the same, if not with greater facility, than in our latitudes, but they would have some peculiarities. For instance, there being no time of day at the pole, Greenwich time alone would be kept, the determination of which by the moon's distance from certain stars or planets, would be the first step. Or, if the moon happened to be below the horizon, the eclipses of Jupiter's satellites could be observed for the same purpose. The calculations would be more or less simplified by reason of the assumed latitude being 90°, and one element of all other positions, namely, longitude, would no longer exist.

With no meridian of the place—or, rather, with an infinite number of meridians—there could be no such thing as a transit observation. The mariner, accustomed in temperate climates to observe the altitudes of celestial bodies at their culmination, would here wait in vain for them to "dip" that he might make his ship-time noon and find his latitude. At the Pole he would never make the time noon, but he would find his latitude notwithstanding.

In the utter confusion of the north, south, east, and west points of the horizon, no meaning could attach to the term "variation of the compass." For, although the compass at the Pole should not lose altogether its directive force, since the magnetic Pole is at some distance from the true Pole, yet, in the face of the fact that every line that could be drawn from the Pole would be in a direction due south, what could we make of the variation? The direction of the magnetic Pole given by the compass could only be called magnetic north, but with no direction that could be called true north, and with an infinite number of directions true south, it would be as absurd to speak of the variation of the compass as of the longitude of the Pole. A. N. S.

SPECTRAL LINES.—OPTICAL ILLUSION.

[284]—It may interest amateurs like myself to know that the scrapings of a galvanic battery—decomposed zinc, copper, salt, &c.—when put on to red embers of a wood fire, give very pretty

lines (E3 FG, &c., nearly a dozen) in a McClean's star spectroscope. Perhaps KNOWLEDGE would help some of us beginners by giving occasionally a few examples how certain lines may be easily produced without the aid of a Bunsen burner, as we do not all live in gas-consuming districts.

If A. T. C., page 230, will prick four pin-holes, he will see four inverted pins, each pin will appear in a circle of its own, which overlaps the one next to it; and if he will hold the piece of card the other way before and look at the square hole through one of the pin-holes, he will see the square lengthened outwards, and divided into three sections, the lightest in the middle. Two pins will seem to be in the parallelogram; half of each pin will appear much darker than the other half. Is the appearance mainly due to interference? C. R. T.

Queries.

[248]—Would any of your readers kindly help me in the following difficulty:—I have a good many fossils from the Barton clay, London clay, and other similar formations. Some of these, viz., shells, are beginning to crumble, others, chiefly vegetable remains, have broken out into crystals, and some have after this disappeared into powder. I have also had impressions of leaves, &c., but these hardly lasted a week. Would some fellow geologist kindly inform me of some means of preserving these fossils from decay, as otherwise it seems a life's labour almost to form anything like a collection; and also what are the blackish metallic nodules one finds in the London clay; and are the forms it sometimes takes casts of vegetable remains, as they are very similar in appearance? And, further, are any fossils to be found in the brick earth, such as lies around West Drayton? and, if so, what kind and whereabouts in the formation is the best place to look for them?—LEPIDODENDRON.

[249]—ABSTRACT REASONING.—Can any readers of KNOWLEDGE give me (*a*) a scientific definition or absolute test for abstract reasoning? (*b*) Is objective and subjective reasoning the same in kind and differing only in degree? (*c*) Are the two possessed by one and the same individual at the same time? (*d*) Does the possession of the former constitute the individual a being of the lower thinking orders, and does the possession of the latter constitute him a being of the higher thinking orders?—W. L. ABBOTT.

[250]—SPECTRUM OF ACTINIUM.—Has anything as yet appeared in print with regard to its spectroscopic behaviour?—MABEL W. LAING.

[251]—ENCRINITES.—Will some of your geological readers kindly give their views as to the origin and formation of the "Encrinus Liliiformis," a characteristic fossil of the Muschelkalk period. In what other strata is it also found?—WILFRED.

[252]—SUGAR ANALYSIS.—(1) Is there a chemical test for distinguishing beet-root from cane sugar? (2) In a given sample of a mixed sugar, could the relative proportions of each be estimated? (3) What is the best book relating to the subject?—P. W. K.

[253]—NATURAL HISTORY, &c.—Will you kindly state the best work on natural history, conchology, entomology, and meteorology, suitable for reference, and work in all parts of the world?—H. B. H.

[254]—VINES.—If the grape in its native condition is of a purple hue, can anybody inform me how the white grape has been produced?—M. E.

[255]—Is there any known means of restoring to well-coloured old prints of flowers, the original red colour which has become black through age? Deoxidation seems the thing wanted.—X. K.

[256]—PARAFFIN.—Is there any simple method of ascertaining the flash-point of paraffin or petroleum oils?—G. F. S. CALL.

Replies to Queries.

[219]—THE ATOMIC THEORY.—For an account of the atomic theory, see the same by Ad. Wurtz; translated by E. Cleminshaw. C. Kegan, Paul, & Co., publishers. Watt's dictionary will give further information, as will the lecture "on the unit weight and mode of constitution of compounds," delivered by Professor Odling before the Chemical Society, Feb. 2, which will shorly be published in the Society's Journal, and also the *Chemical News*. There are tables for the solubility of salts, but do not know of any theory for the insolubility of certain substances in certain fluids.—TECHNICAL CHEMIST.

[220]—CHEMICAL ANALYSIS.—The following are some of the best text-books:—"Valentine's Qualitative Chemical Analysis," price 7s. 6d.; "Fresenius' Qualitative Analysis," 12s. 6d.; also "Quantitative Analysis," 15s.; "Sutton's Handbook of Volumetric Analysis," 15s., all published by Messrs. Churchill.—TECHNICAL CHEMIST.

[221]—Schwegler's "History of Philosophy" is undoubtedly the best. James Hutchison Stirling's translation is published by Edmonston & Co., Edinburgh, price 6s.—G. A. NEWCOMEN.

[223]—ANILINE DYES.—Ernest L. R. would find a short account of the aniline dyes in Watt's Dictionary, under Phenylamines. For more exhaustive article, see "Chemistry of the Arts and Manufactures," "Aniline and Aniline Dyes," page 201.—TECHNICAL CHEMIST.

[224]—ELECTRICAL.—The "rubber" for a cylindrical machine may be made by attaching a piece of leather to a piece of wood the required size, and stuffing it with horse-hair; then rub on some sodium amalgam. It is not necessary to varnish the cylinder, though it is an advantage, in so far as it helps to keep moisture from condensing on it.—AMATEUR.

[224]—ELECTRICITY.—"A Greenock Student" will find the directions he requires in Chambers's "Electricity," published at 1s. (and to be obtained from any bookseller), which would be much better than a reply through the Query column of your paper, as it gives drawings. It is not necessary that the cylinder be covered with shellac varnish. Indeed, I never heard of that being done before.—J. M. C.

[233]—BIOLOGICAL.—Physically, the difference between ape and man is much greater than that between man and man. But mentally, not. The range of variation in the capacity of the brain-case of man (healthy adult) is between 50 and 110 cubic in.; the difference between the gorilla brain-case and the lowest human is only 13 cubic in., *i.e.*, between 37 and 50 cubic in. (2.) The thyroid gland in the higher vertebrates has, by the researches of Mr. Balfour, been recognised as the rudiment of an organ called the endostyle, which occurs in the lowest group of the vertebrata, the Tunicata. (See p. 597, "Huxley's Invertebrata.") This organ takes the form of a longitudinal groove lying on the floor of the pharynx. Its function appears to be to secrete a kind of mucus which assists the process of swallowing food, though this latter point is open to question. In the floor of the mouth of vertebrate embryos, there appears a similar groove, which ultimately developes into the thyroid gland. (3.) Darwin in the "Descent of Man," quotes several instances of human beings with projecting caudal vertebræ, and I may say further that I know an individual now living who has such a tail. There have been many accounts published of races of men so adorned existing in regions sufficiently remote to prevent speedy refutation; but, so far as I know, they all lack corroboration. Herodotus mentions a tribe of Central Africans with tails, but not having access to his work just now, I cannot give details. I am indisposed to accept any of these statements, as it would be directly contrary to the theory of evolution to suppose that a useless structure should reappear and persist in a race after it had once been lost, and man's nearest allies among the apes have no tails. (4.) The Neanderthal skull. (*a*.) The brain capacity cannot be definitely ascertained, as only the roof of the cranium is preserved; but it is very small, probably below 40 cubic inches. (*b*.) The fragment of skull is remarkable for immense bony ridges projecting over the eye-orbits, of apparently nearly half an inch, and suggesting at once a comparison with those of our "poor relations," the gorillas. The facial angle cannot be ascertained. The back part of the human head where the muscles of the neck are attached, is marked by a slight ridge, called the "Lambdoidal crest." Now, the back of the head will be found to project considerably behind this point in the skull of an European, while in the lowest modern type (the Australian) it ascends perpendicularly to some height, but in the Neanderthal skull it slopes directly upwards and forwards, thus greatly diminishing its capacity. In addition to this, the brain-case is very shallow, as the following measurements will show:—A line drawn from the "Lambdoidal crest" to the front of the brow ridge gives a length of 7¾ in., against an extreme length of 7¾ in. in an average European skull. The height of the Neanderthal skull above the line indicated is 3¾ in., while that of the European is 4½ in. Extreme width of Neanderthal skull 5¾ in., European 5¾ in. The length of the Neanderthal skull is apt to mislead, as the great brow ridges are not excavated to receive the brain. Professor Huxley remarks that this is the lowest type of human skull that has been discovered. The skull was found in what is called a Cave Breccia, and belongs to the so-called "Palæolithic period," which, however, is so ill-defined as to give a very indifferent idea of its exact age, though we must certainly regard it as being tens of thousands of years old. OLD FOSSIL.

[239]—Besser, "Primitiæ Floræ Galiciæ," Paris, 1800; Brotero, "Flora Lusitanica," Madrid, 1804; Picot de la Peyrouse, "Flore des Pyrénées," Lyons, 1793-1802. All in the B. M. Library.—H. C. F., B.Sc.

[250] "C. E. H.," and [198] "J. H. B."—TRICHINÆ.—The eating

of raw, trichiniferous pork is the chief cause of the propagation of the entozoon to man; but the parasite is not easily killed, even by cooking or salting. A temperature of 144° to 155 Fahr. kills the free trichinæ, but those encapsuled demand a greater heat. (Fiedler.)—During cooking, a temperature which will coagulate albumen (150° to 155° Fahr.), renders the trichinæ incapable of propagation or destroys them. As a practical rule, it may be said that if the interior of a piece of boiled or roast pork retains much of the blood-red colour of uncooked meat, the temperature has not been higher than 131° Fahr., and there is still danger. Hot smoking, when thoroughly done, does destroy them (Leuckart); but the common kinds of smoking, when the heat is often low, do not touch the trichinæ (Kuchenmeister).—CHAS. BOYLE, M.B.

[275]—ASBESTOS PAINT AND THE SAFETY LAMP.—The principle of the Davy lamp is that the heat of the ignited gas within the lamp is absorbed by the wire gauze before the incandescent particles can pass through it. If you were to coat it, as you say, with a non-conductor, it would at once lose this power of conducting heat away from the flame. The particles of gas would pass through the meshes before they were cooled below flashing point, and an explosion would ensue. No. What you want is, if possible, to improve the conducting power of the wire gauze, and also to increase its capability of radiating heat.—C. H. WINGFIELD.

Answers to Correspondents.

**** All communications for the Editor requiring early attention should reach the Office on or before the Saturday preceding the current issue of* KNOWLEDGE, *the increasing circulation of which compels us to go to press early in the week.*

HINTS TO CORRESPONDENTS.—1. *No questions asking for scientific information can be answered through the post.* 2. *Letters sent to the Editor for correspondents cannot be forwarded; nor can the names or addresses of correspondents be given in answer to private inquiries.* 3. *No queries or replies savouring of the nature of advertisements can be inserted.* 4. *Letters, queries, and replies are inserted, unless contrary to Rule 3, free of charge.* 5. *Correspondents should write on one side only of the paper, and put drawings on a separate leaf.* 6. *Each letter, query, or reply should have a title, and in replying to letters or queries, reference should be made to the number of letter or query, the page on which it appears, and its title.*

U. G. There is a misprint, no doubt. Hydrogen, not oxygen, must be meant, when it is said that 100 cubic inches weigh 2.14 gr.; or else for 2·14 read 32·14, though this is not quite right, it would be about 34·42.—W. H. ROSSER explains that the top-gallant forecastle is that part of the forecastle where they work the anchors, and where there is generally a windlass; also, that when the royal mast is a fitted one, there are top-gallant cross-trees. Thanks. We had not known.—E. F. B. HARSTON says sailors mean simply "very high" by top-gallant, as top-gallant head-gear—a woman's bonnet.—HERBERT WEIGHTMAN. In line 8 from bottom of 1st col., p. 307, and in line preceding the final result, the v in denominator of second fraction within brackets should be deleted. The mistake arose in making clean copy for printers.—WONDERFUL PHENOMENON. The calculating boy himself in that case (G. P. Bidder) explained how he worked, but, of course, not why he could work so quickly.—H. L. says, readers who want their papers cut will want next to have them read aloud. He compares them to the sturdy beggar, who, having been given a penny to get bread, said: "It will be very dry; you might give us a drop of beer to wash it down."—C. J. C. Not Lord Rosse, but earlier astronomers proved moon uninhabitable.—A. R. BROOKS. I should say, invest about £1 in a good achromatic object-glass, £1 in suitable eye-glasses, and fit them into tubes, which you could make yourself after the manner shown at p. 275.—PHŒNIX. We must not trench on the department of our medical contemporaries. We should be flooded by replies, among which some might be untrustworthy, and a few unintentionally mischievous.—JAS. A. GEE. Read Darwin on the "Formation of Vegetable Mould through the Action of Worms."—LEWIS J. COLES. Thanks.—E. MALAN. Question would involve four or five pages of replies.—STUART MURRAY. Could you not say that shorter?—F. B. HOLT. Kindly make a neat query.—T. S. Unfortunately for your position, phrenological theories have been disproved, not neglected, by science.—C. GRIMSHAW. In American houses, none of the suggested effects are noticed. As for the open fire, with its draughty ventilation, it is a disgrace to our civilisation. After enjoying the comfort of well-warmed houses in America, with a winter temperature often falling to 20 or 30 degrees below zero, I feel ashamed when I think that in England a winter in which the thermometer falls 4 or 5 degrees below zero leaves us shivering hopelessly (half roasted on one side), even in houses intended for the rich (and at thrice the cost for fuel).—VACNOL. Fear Mr. Williams cannot tell us why fleas or their flatter allies prefer some folk to others, or why some feel their bites more than others.—W. G. WOOLCOMBE. Should say that with your mathematical knowledge, Herschel's "Outlines" would be far more interesting than Guillemin's "Heavens." I have revised the latter book; but, written as it originally was by a non-mathematician, revising can be but patchwork. After reading his carefully illustrated explanation of the way in which one meteor-stream could explain both the November and August showers, a mathematician puts down the work, knowing no original opinion by a writer who could make such a mistake can be of any value. An ordinary mistake proves nothing; but such a mistake as this carefully wrought out in details can only be interpreted in one way. Yet the descriptive portions of the work are very good.—MAJOR JAS. CUMMINGS would like to know where he could purchase such a blowpipe as Lieut.-Col. Ross describes, and wishes for further information and illustrations respecting the apparatus described in Lesson 2.—SCIENTIA CUM LEGIBUS. Believe it has been shown that scents may be emitted for very long periods without appreciable loss of substance by the scent-emitting substance.—E. TAYLOR wants name of a work on wild flowers and plants, with descriptions enabling beginner to distinguish them, to cost about 2s. 6d.—NOTES UNSIGNED received respecting moisture in air, effects of tobacco, natural philosophy, and the atomic theory.—GORGON. Whether nose or ears can be changed in form by reiterated daily compressing is hardly a question suitable to these pages. Try the *Lancet* (the paper, not the instrument), or the *Medical Press and Times*. Should say the story about fall of manna in 1846, at Yenishehir, must be a canard.—W. A. C. It is unfortunate, but matters seem so arranged that unless some animals die, many human beings must perish.—TEREDO. Could the sides of an ancient river valley have the required flatness? The natural interpretation is, that the terraces are parts of sea beaches which have been displaced later from horizontality.—J. RAF. F. R. A. S. has explained his meaning.—CHAS. BOYLE, M.B., explains that opium only assists the action of purgatives (in cases of lead-poisoning) by relaxing the contraction of the intestines, but does not itself act as a purgative.—COMET. Yes: comets obey gravity. The size and mass of a celestial body in no way affect its motion; a pin's head (or a pin) sent off with the right velocity would travel in an orbit a million times larger than the earth's, with as perfect steadiness as the most massive planet.—T. R. A. Fear cannot at present find space for articles on mesmerism, though am very certain you would treat the subject from a scientific standpoint.—F. F. Question too vague. Besides, it is not fair to ask me what books I recommend on subjects upon which I have written myself.—S. S. wants Mr. Mattieu Williams to tell him how to warm a room 13 × 13 ft., which has no fireplace, and in the walls of which no holes can be made for stove-pipes.—STUDENT. Story about inherited kleptomania in a dog rather too long for us.—T. R. ALLINSON. Thanks; but questions already answered. Your replies not numbered.—CHARLES DAWE. The question whether snakes swallow their young in time of danger is rather well worn. The usual opinion of naturalists is that there is no foundation for the idea. The story you mention about the young who had eaten their way out, after being swallowed, seems very hard to take in. Let us hope it is not true.—F. H. S. No more room for magic squares. —ECLECTICUS thinks W. S. C.'s reply to his letter, p. 202, too vague to be of use. We have not space to repeat the question.—SIMPLEX. Your account of Bell's system of shorthand too abstract for our readers, and too long; we wanted only a few simple illustrations of its characteristics, as compared with those of Pitman's.—J. F. S. Thanks. Wish we could do more that way. The correspondence stops the way. Fear can find no space for letter already printed.—A NEW READER. Thanks; but new subjects of correspondence leave no room for the old ones you discuss.—W. H. K. SOAMES. You go a little beyond what I had thought possible in the line you take. When, first, scientific discoveries seemed difficult to reconcile with certain passages in the book you mention, men of your way of thinking said simply the earth is not a globe, it does not move round the sun, and so forth, because this book says differently; we want no other evidence: so they rebuked men of science for teaching such things, and told them to be still. When, however, men of science had demonstrated the soundness of their views in those matters, your friends took another line. "These facts," they said, "are right enough, and the account in the book, rightly understood, agrees perfectly well with them." So they rebuked men of science for saying that the facts did not agree with the book account, and told them again to be still, as not knowing how to interpret the book. Men of science had *not* said what they were rebuked for saying; but that was a detail. It is, however, a new thing—so far as I know—to take your line; and to tell men of science that they are *bound* to show that the account in the book is incorrect before they indicate

results which you consider inconsistent with that account. Everything you say about the origin of man might have been said with equal force about the Copernican Theory three centuries ago. Some of it *was* said. The theory was described as obviously contrary to the plain teachings of the book. But no one, so far as I know, went so far as you do, and asked that the follower of Copernicus should begin by showing the book "to be untrue." For my own part, I should think it was much easier to reconcile (as it is absurdly called) the modern scientific belief of man's origin with the account given in the book you mention than the Copernican Theory or the results of geological research; as men of science are far better able to inquire into the truth of the scientific theory than into the question of that book's authority, I can see no reason for urging them across the line which separates science from religion. Consider a little that very clever (but, I suspect, generally misunderstood) saying of Disraeli's, "Religion begins where science ends," —this, with a very slight change, may be taken as "our mortar what we sticks to" in this matter. Where dogmatic religion begins KNOWLEDGE ends. I say all this with no more objection to religious attacks on science, than to scientific attacks on religion; I object equally to both. But just think where the science of our day would be, if, before starting any theory, men of science had had to show that the interpretation put by theologians on certain passages in religious works was incorrect.—?. R. You ask when the Greater Light was called the Sun, and refer to Genesis, ch. xv. v. 12; but that account says only when it was called by another name. The present name, or form of the name, is not quite so old. As a matter of curiosity I should like to know why you wish for an answer to this question. It must be of interest to you, as you have been asking it for years: but why? What *can* it matter?—W. J. ABBOTT. Many thanks; if KNOWLEDGE had many such friends as you we could afford to give what hitherto only much more expensive papers could present to their readers. Geological matters will soon have their turn. Do you not think the quicksilver story—instructive though it doubtless is—better suited for a more medical journal than ours? W. MIDDLETON suggests that the sea-blue bird of March may be the fieldfare (see Goldsmith's "Animated Nature," vol. ii., p. 140). J. P. SANDLANDS. Nay, the article on Found Links simply presents a certain part of the evidence for what it is worth; the evidence for the development of one form of creature from another form is one thing, the indication of such links as the development theory requires is another. Dr Wilson's argument is complete, so far as it goes; but one and the same argument cannot answer all objections. I can only infer from your remarks about luck in its relation to religion, that I should not discuss the mathematical theory of probabilities, because it hurts your religious feelings to be told that, so far as observation extends, the laws of probabilities and of averages are never departed from. That would be as unreasonable as to urge that the scientific discussion of the laws of chance does not take into account the possible influence of earnest prayer on the observed result in the case of any particular person. A and B draw tickets in a lottery, prize £1,000, number of tickets 1,000; and science says the chance of each is worth just £1. But J. P. Sandlands interferes. "Do not trespass on forbidden ground," he says; "whether you mean it or not, the inference from that statement is against religious truth. If you make a statement, and say this is all we know, and speak as an authority, does it not follow that there is nothing more? If you examine the statement you will find it ignores the interaction of scientific and religious truth." A, you imply, may be favoured for reasons which religion suggests and can explain, and science, in making no mention of this, to all intents and purposes suggests that there is no such influence from without. It may seem so to you. I cannot take your view. As I have already said, the mathematical discussion of chance seems to me to have no more to do with religion than religion has to do with experiments in chemistry or physics. Q. Many thanks. The mistake was discovered soon after the book was published. Many errors in the treatise were such as I could not possibly have avoided—accents not showing which had shown in proof, and the like. The work was very laboriously corrected, but was unfortunately in the hands of printers of very little mathematical experience. F. W. F. Problem later. F. S. The distance you require—the sagitta (say s)—is given in terms of the chord ($2c$), and the radius (r) by the following formula:— $s=r-\sqrt{r^2-c^2}$. When r is very large compared with c, we have approximately $2rs=c^2$, or $s=c^2\div 2r$.—C. H. WINGFIELD. Thanks: but the experiments to be found in elementary treatises on pneumatics and chemistry. We must not occupy space telling readers what so many know.—F. G. BARKAS. As to my supposed prediction—not quite. I would be glad to see the article you mention. There would be nothing to prevent its appearance here, if the reasoning seemed valid. I think it very unlikely I could lecture at Newcastle, or anywhere, next season. Thanks, equally.

Letters Received.

H. Mc W., R. Haveley, Gl. Trevelyan, W. A. C., A Fellow of the Chemical Society, N., T. W., A. E. S., Argo, J. Acrington, Borlase, J. Twaite, L. Parmenter, A. K. Ld., Bronte, J. Harmens, L. Tuthuil, M. Amb. K., Ecce Signum, J. Harvey, M. N. S., F. K. G., L. Partners (2), F. R. G. S. (No. 1), Hakluyt, A. L., T. P. R., F. R. G. S. (No. 2), (Birmingham.)

Notes on Art and Science.

WE are informed that Messrs. Barraud & Lunds, the patentees of the System of Time Signalling and Clock Synchronizing so largely adopted in London, have applied to the Postal Telegraph Department for an extension of their Time Exchange to Telephone purposes. For some time they have been experimenting with the object of utilising their existing system for telephoning, which has resulted in a perfect success, and they now propose to formally thus duplicate and extend their system, by which every telephone subscriber would be able to receive an hourly Greenwich time current by simply hanging his receiver on a hook in his telephone, marked "Time Signal," without any interference with the speaking power of the instruments. The proposals are under consideration at St. Martin's-le-Grand, where so beneficial an arrangement can hardly fail to receive due attention.

MANUFACTURE OF GAS FROM WOOD.—It may not be known to all the readers of KNOWLEDGE that carbonic oxide (CO), the manufacture of which "F.C.S." refers to in the number for Jan. 20, is extremely injurious to life, if inhaled in any quantity. Carbonic dioxide (CO_2) is deleterious because it takes the place that should be occupied by oxygen; whereas carbonic oxide is a distinct poison, causing death in a very short time; therefore, should any attempt its manufacture, they should be very careful to allow none to escape into the room.—F. D. H.

GLYCERINE LEATHER POLISH.—Mix intimately together 3 or 4 lb. lampblack and ½ lb. of burned bones with 5 lb. glycerine and 5 lb. syrup. Then gently warm 2¼ oz. of gutta-percha in an iron or copper kettle until it flows easily, then add 10 oz. of olive oil, and, when completely dissolved, 1 oz. stearine. This solution, while still warm, is poured into the former and well mixed. Then add 5 oz. gum senegal, dissolved in 1¼ lb. water, and ½ oz. lavender or other oil to flavour it. For use, it is diluted with 3 or 4 parts of water. It is said to give a fine polish, is free from acid, and the glycerine keeps the leather soft and pliable.—*Polytech. Notizblatt.*

AN influential company, having Messrs. Marshall Jewell, Charles E. Mitchell, Morris F. Tyler, and Robt. Wheeler Wilson on their board, and with all their capital privately subscribed, has just been formed in America, to be called "The Standard Time Co." Having carefully examined into the various systems of synchronising clocks in use both in America and Europe, the one in use in London, and known as Barraud & Lunds', has been adopted by them; a gentleman was specially sent over to England to the patentees, negotiations were at once opened, communications cabled, and the whole concession concluded in a few days. The arrangements include the whole of the American Continent, as well as the United States, and an effort will be made to bring about a concerted system of time signalling over the whole of the States; local affiliated companies are to be formed, and we shall probably soon hear of our transatlantic cousins having once more excelled us in an extensive exchange and utilisation of time signals, as they have in their telephone exchanges.

Our Mathematical Column.

T. R. Many thanks for your algebraical proof of the relation involved in our geometrical solution of the Messenger problem. "No Analyst," however, in his personal communication, said he could follow a proof referring to hyperbolic area, which would, he supposed, be involved, and referred to a problem which it appears is given in Frost's Newton. Will give next week a geometrical proof that area of hyperbolic section is related that way to natural logarithms. Unfortunately, our mathematical space is at present somewhat limited, and we are obliged to indulge as little as possible in analytical investigations. Hope soon to begin some simple papers on the differential and integral calculus, which will somewhat widen our sphere of operations. We must notify our mathematical readers, that recently we have allowed our consideration for those who have sent problems for solution to interfere greatly

with our original plan; and that, as that plan involved a promise, we must adopt in future a different line. We cannot further find space for such problems as occur in ordinary mathematical reading, except when illustrating general principles. Our mathematical column must not degenerate into a puzzledom corner.—ED.

A correspondent (N. X L.) asks for a demonstration of a property of the conic sections to which we referred a short time back, viz., that if a sphere enclosed in a cone (like a ball in a conical cup) touches the plane of section, the point of contact between the sphere and the plane is a focus of the conic section. We have prepared three diagrams corresponding to the case of (1) ellipse, (2) hyperbola, (3) parabola, and will give these next week, with a demonstration which seems to us of interest, as probably the simplest proof connecting the fundamental property of the ellipse, parabola, and hyperbola (relation between distances from focus and directrix), and the fact that the curves possessing that property are sections of the cone.—ED.

MATHEMATICAL QUERIES.

[35]—VALUE OF LEASE.—Given, 14½ years repairing lease; rent, £15; ground-rent, £4; present rental value, £32. Required, present worth of lease to make 5 per cent. interest.—JAMES GREGG.

[36]—Can you, or any of your readers, tell me how to obtain the general term in the expansion of $(a_1 + a_2 + a_3 + \&c.)^n$, n being whole or fractional, positive or negative?—CARTESIAN.

[We should deal with the problem somewhat on this wise:—Let any expression of the form $a_m + a_{m+1} + a_{m+2} + \&c.. = a_m$: also in the expansion of $(a_m + a_{m+1})^p$ take the $(r_m + 1)$ th term for general term, and put $p - r_m = r'_m$. Then

$$(a_1 + a_2 + a_3 + \&c.)^n = (a_1 + a_2)^n = \Sigma \left[\frac{n(n-1)\ldots(n-r_1+1)}{1.2.3\ldots r_1} a_1^{r_1} a_2^{r'_1}\right]$$

$$(a_2)^{r'_1} = (a_2 + a_3)^{r'_1} = \Sigma \left[\frac{|\underline{r'_1}}{|\underline{r_2}\,|\underline{r'_2}} .a_2^{r_2} a_3^{r'_2}\right]$$

$$(a_3)^{r'_2} = (a_3 + a_4)^{r'_2} = \Sigma \left[\frac{|\underline{r'_2}}{|\underline{r_3}\,|\underline{r'_3}} .a_3^{r_3} a_4^{r'_3}\right]$$

$$(a_4)^{r'_3} = (a_4 + a_5)^{r'_3} = \Sigma \left[\frac{|\underline{r'_3}}{|\underline{r_4}\,|\underline{r'_4}} .a_4^{r_4} a_5^{r'_4}\right]$$

&c., &c., &c.

$\therefore$ finally $(a_1 + a_2 + a_3 + \&c.)^n$

$$= \Sigma \left[\frac{n(n-1)\ldots(n-r_1+1)}{|\underline{r_2}\,|\underline{r_3}\,|\underline{r_4}\ldots|\underline{r'_x}\ldots} a_1^{r_1} a_2^{r_2} a_3^{r_3} a_4^{r_4}\ldots\right]$$

Where r_2, r_3, r_4, &c., are positive whole numbers, and

$$n = r_1 + r_2 + r_3 + r_4 + \&c.,$$

If n is a positive whole number, we may conveniently interchange r'_1 and r_1 in the first part of the process (the distinction being only introduced because if n is not a positive integer, neither is r_1). We thus obtain the convenient formula

$$(a_1 + a_2 + a_3 + \ldots)^n = \Sigma \frac{n(n-1)\ldots(n-r_1+1)}{|\underline{r_2}\,|\underline{r_3}\,|\underline{r_4}\ldots} a_1^{r_1} a_2^{r_2} a_3^{r_3}\ldots$$

$$= \Sigma \frac{|\underline{n}}{|\underline{r_1}\,|\underline{r_2}\,|\underline{r_3}\ldots} a_1^{r_1} a_2^{r_2} a_3^{r_3}\ldots \qquad \text{ED.}$$

Our Whist Column.

WHIST PROBLEM, No. 1.

In this problem B holds the following hand:—

Spades.—Ten, nine, six, five. (Trumps.)
Hearts.—Ace, Queen, four, two.
Diamonds.—Queen, six.
Clubs.—Ace, ten, eight.

and the four first tricks are as follows, the underlined card winning trick, and card below leading next:—

	A	Y	*B*	*Z*
1.	C 6	C Kn	$\underline{\text{C A}}$	C 3
2.	H 9	H 5	H 2	$\underline{\text{H 10}}$
3.	D 8	D Kn	$\underline{\text{D Q}}$	D 4
4.	S Kn	$\underline{\text{S A}}$	S 5	S 7

After these four tricks have been played *B* is able to place every card, supposing that all the players have followed the usual rules for play.

No one has solved this problem correctly. Fifteen solutions sent. We note that what we have hitherto said about whist leads does not quite suffice for the solution of this problem, though it help towards it. It is necessary to supplement the rules for lead, however, with only two general rules, one for second, the other for third player, to give the solution. These are first that second player, if he has a sequence of two high cards and one small one, plays the lowest of the sequence second hand on a small card led; secondly, that third in hand plays highest if he has any card higher than (and not in sequence with) his partner's lead, and no sound finesse open to him, but otherwise plays his lowest.

First Trick.—*A* has led the lowest from four at least (it should have been noticed that the inventor of this hand did not accept the rule for penultimate lead). Since two is not in *A*'s hand, nor in *Z*'s, for *Z*'s lead third hand shows he was not signalling for trumps, and *B* has it not himself, it must lie with *Y*. But no other small card can be in *Y*'s hand, who would only play Knave, having the two, if he held Queen, Knave, two, and no more. Hence four and five lie with *Z*, and no more, for *A* must have four Clubs. Thus the Clubs were originally distributed as follows:—With *Y*, Queen, Knave, two; with *Z*, five, four, three; with *B*, Ace, ten, eight; and the rest, viz., King, nine, seven, and six with *A*.

Second Trick.—*A* has no Hearts above ten, and his play of nine shows he has none lower. Hence, *A* only holds Hearts nine. As *Y* plays the five, he does not hold the three (he had not begun a signal in first round, as *B* knows, holding Clubs ten in his own hand). Hence, Hearts three must be held by *Z*, and as he played ten, having the three, he must have the Knave, but no others. Hence, the Hearts lay originally as follows:—

With *A*, the nine; with *Z*, Knave, ten, three; with *B*, Ace, Queen, four, two: and the rest, viz., King, eight, seven, six, and five with *Y*.

Third trick. Diamonds four is the lowest of four at least. *A* has no card below the eight, hence the two and three must be with *Y*, as *A* is certainly not signalling. We know also that *A* has not five trumps, or he would have begun with one; hence, as he had originally four Clubs, one Heart, and fewer than five trumps, he must have more than three Diamonds. Since eight is his lowest and *Z* has led from four at least, *B* having Queen, six, and *Y* Knave, three, two, it follows that *Z* must have held seven, five, four, and either Ace or King, showing that *A* must have had eight, nine, ten, and either Ace or King. But *A*'s first lead shows that *A* must have the Ace and not the King, for he would not have led Clubs from six, seven, nine, King, if he had had eight, nine, ten, King of Diamonds; though, following Clay's rule, he would have led a Club if holding eight, nine, ten, Ace of Diamonds, reserving the Ace-headed long suit to get in with later. Thus the Diamonds lay originally as follow:—

With *Y*, Knave, three, two; with *A*, eight, nine, ten, Ace; with *B*, Queen, six; and the rest, viz., King, seven, five, four, with *Z*.

Fourth Trick.—*B* knows already that *A* holds four Spades; *Y*, two Spades; and *Z*, three. As *Z* plays the seven, the only cards left which can make up his remaining two are the eight, the Queen, and the King. He cannot have both Queen and King, or he would have played the Queen. He must have, then, either eight Queen or eight King. But if he had the Queen, King would lie with *A*, and *A* would not have finessed the Knave holding King, Knave, and two others. Therefore *Z* held King, eight, seven. *Y*'s other card must be a small one, and Spades were originally distributed as follows:—

Z,—King, eight, seven; *B*,—ten, nine, six, five; *Y*,—Ace, two (or three, or four); and the rest, viz., Queen, Knave, four, three (or four, two, or three, two) with *A*.

The doubt as to the actual value of the small spade in *Y*'s hand can hardly be said to affect the statement that *Z* knows the position of every card in the pack, for the two, three, and four, are in this case of practically equal value.

We would now leave our whist readers to explain why *B* led trumps fourth round, when, with his knowledge of the position of cards he might, one would say, lead his only remaining diamond, through *Z*'s King, enabling *A* to make the trick with the nine.

G. THOMPSON. *B*'s lead second trick is correct. It is unfortunate having to lead from a tenace suit; but it is better than deceiving partner. Returning partner's suit at once means, "I have no strong suit."—H. P. YARMOUTH. Your method of dealing with the problem discussed by the Editor at p. 301 (letter 259) is incorrect. Do you not see that in five cuttings, according to your method, *A* would possess fifteen chances out of thirteen, which is absurd?—GRADATION. The lead of King followed by Queen from Ace, King, Queen, &c., should certainly have been added (it is indicated at p. 259); but not "Ace followed by ten from Ace, Queen,

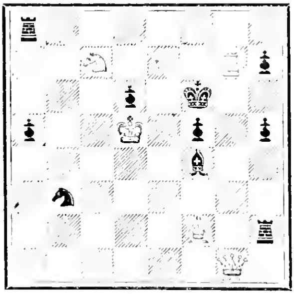
Knave, ten"; because that is not correct. "Ace, followed by nine (to lead at lowest or next to lowest) from Ace, Queen, ten, nine, and others, or from Ace, Knave, ten, nine, and others" is correct in general, but hitherto, though we have touched on the play second round, we have not intended what is said about that round to be exhaustive. Special considerations come in for second round, which render special treatment necessary in its case. One would have to mention exceptions in the last two cases; for if King and Knave fell first round in the former, or King and Queen in the latter, the highest of the sequence would be the proper card to lead second round. Our leads are complete, and the learner would make the few necessary exceptions for second round if he possessed average intelligence, just as he would not, merely because of the general rule "third in hand play your highest," put his King on his partner's Queen. As to the trump leads, we should have said that from Ace, King, not more than four others, and from King, Queen, not more than four, smallest is led. We believe we wrote four in both cases, but, as you note, it is printed "five." Thanks. The chance problems later. Some of your solutions look too simple, but they may be right. May perhaps ask Editor to give these as mathematical problems. FIVE OF CLUBS.

Our Chess Column.

PROBLEM No. 20.

By J. A. MILES.

BLACK.



WHITE.

White to play and mate in four moves.

SOLUTIONS.

No. 9.—End Game, by A. J. Maas, p. 239.

1. P. to B.6. / K. to K.sq. or A.
2. K. takes P. / P. to Kt.5.
3. P. to B.7.(ch.) / K. to B.sq.
4. K. to B.6. / P. to Kt. 6.
5. P. to K.5. / P. to Kt.7.
6. P. to K.6. / P. Queens.
7. P. to K.7. / mate.

A. 1. —— / P. to Kt.5.
2. K. to B.4. / P. to Kt.6.
3. K. takes P. / P. to K.4.
4. K. to Kt.4 / K. to K.3.
5. K. to Kt.5. / K. to B.2.
6. K. to B.5.

and wins.

If White plays 1. P. takes P.(ch.), Black obtains a draw by correct play.

End Game, No. 10., p. 240.

1. Q. to B.6.(ch.) / K. to B.sq. or A.
2. Q. takes P.(ch.) / P takes Q.

and White is stalemated.

A.—If Queen interposes perpetual ch. on R 8. and B.6.

No. 11.—Problem by Herr Gunsberg, p. 240.

1. Q. to K.R. 2. / R. takes Q. best.
2. Kt. to K.8.(ch.) / Kt. takes Kt.
3. Kt. to K.6. / mate.

Most of our correspondents gave 1. Q. to Kt.3.; but if Black replies with 1. R. to R.4.(ch.), there is no mate in two moves.

Problem No. 11, p. 259.

1. Kt. to B.5 (ch.) / K. to Q.5 or A
2. B. to K.3 (ch.) / if K. takes B.
3. Q. to B 2. / mate.

if 2. —— / K. to B 6
3. Q. to Q.Kt 3. / mate.

if 2 —— / K. to K.4.
3. Q. to K.6. / mate.

A. 1. —— / K. to K 4
2. Q. to Q 7 / K. to B 3.
3. Q. to K 6. / mate.

Problem No. 12, p. 260.

As pointed out by us, this has two solutions, viz.:—

1. Q. takes R. / B. takes Q. (best)
2. R. to K.Kt.5. / Kt. takes R.
3. Kt. takes R.P. / P. to Kt.3.
4. B. to Q.3. / mate.

or

1. Q. takes P. / Q. to K.sq. or A.
2. Kt. takes R.P. / Q. to K.4.
3. Q. takes B.(ch.) / Q. takes Q.
4. Kt. to B.3. / mate.

A. 1. —— / P. to Q.4.
2. Kt. takes R.P. / P. to Kt.4.
3. P. to Q.4.

and mates accordingly.

ANSWERS TO CORRESPONDENTS.

⁎ *Please address Chess-Editor.*

Edward Sargant (Problem No. 11, p. 240).—If Kt. to K.6.ch., then Kt. takes Kt. with a check, and there is no mate.

J. P.—In Problem No. 18 or No. 6, if 1. Kt. to B.5. / B. takes Kt. 2. Q. to K.6. / "B. to Q.6." then 3. B. takes P. / mate.

Squire.—Solution of No. 14 correct. We agree with you as to its merits.

J. H. Wootton.—There is no modern treatise on odds; we hope soon to publish some articles on these openings. If you give a Pawn, you must give your K.B.P.

E. C. H.—1. You can have as many Queens for as many Pawns as you can advance to the eighth row; 2. In Castling on the Queen's side, the King is put on B.sq. and the Rook on Q.sq.; 3. "Stale" mate is a draw.

F. W. B.—Solution of No. 14 correct. It is convenient for writing down a game that is being played that the first player should play with White; but it is not compulsory.

G. Woodcock.—Your joke, directed against us at "full cock," is good; but you are an adept in the art of firing. Remove Pawn on black Q.R.2.

M. J. Harding.—We willingly grant your request, free of charge.

Henry Planck.—Solutions correct.

F. Edmonds.—Thanks for games, which shall appear. "Mephisto" and Chess Editor of KNOWLEDGE are "one" in the flesh! but "two" in the spirit.

Notice.—A gentleman would be glad to hear of another willing to play a game by correspondence.—Address, CHESS EDITOR KNOWLEDGE.

Contents of KNOWLEDGE *No.* 15.

KNOWLEDGE
AN ILLUSTRATED
MAGAZINE OF SCIENCE
PLAINLY WORDED-EXACTLY DESCRIBED

LONDON: FRIDAY, FEBRUARY 24, 1882.

CONTENTS OF No. 17.

OUR ANCESTORS.

I.—THE STONE AGE MEN.

By GRANT ALLEN.

THERE are few questions more immediately interesting to Englishmen than the question—who are our ancestors? From what elements and in what proportions are we compounded? May we consider ourselves as all pure Teutons? or are we partly Celts as well? Furthermore, may we even reckon among our immediate ancestry some still earlier and less historical races than either of these? Such questions are full of practical importance to ourselves, and they are also of a sort upon which modern investigations into language and the science of man have cast a strikingly new and unexpected light.

Of course, in considering the origin of Englishmen, we must look at the matter in no petty provincial spirit. We must include roughly in that general name Welshmen, Scotchmen, and Irishmen as well; and if our friends in the north prefer to speak of Britain rather than of England, I am sure I, for my part, will have no objection. There are many learned modern historians, with Mr. Freeman at their head, who will tell us that Englishmen are almost pure-blooded Teutons, of the same original stock as the Germans, the Dutch, and the Danes and Norwegians. But when we come to inquire more fully into their meaning, it turns out that they are speaking only of the native inhabitants of England proper and the Scotch Lowlands, without taking into consideration at all the people of Wales, Ireland, and the Highlands, or the numerous descendants of immigrants from those districts into the south-eastern half of Great Britain. Even in the restricted England itself, these same doughty Teutonic advocates admit that there is a nearly pure Celtic (or pre-Celtic) population in Cornwall, in Cumberland, and in Westmoreland; while the western half of the Lowlands, from Glasgow to the border, is also allowed to be inhabited by a mainly Welsh race. Furthermore, it is pretty generally granted by our stoutest Teutonic champions themselves, that the people of Dorset, Somerset, and Devon; of Lancashire, Cheshire, Shropshire, Herefordshire, and Worcestershire; are all largely mingled with Celtic blood. Thus, in the end, it appears that only the native inhabitants of the Lothians and the Eastern and Southern coast of England are claimed as pure Teutons, even by those who most loudly assert the essentially Teutonic origin of the English people. We may possibly find that this little Teutonic belt, or border itself, is not without a fair sprinkling of earlier blood.

Perhaps the best way to clear up this question will be to glance briefly at the various races which have inhabited these islands, one after another, and then to inquire how far their descendants still exist in our midst, how large a proportion of our blood they have contributed, and whereabouts their representatives are now mainly to be found. Of course, in such an inquiry we can only arrive at very approximate results, for in our present advanced stage of intermixture, it is almost impossible for any man to say exactly what are the proportions of various races, even in his own person. Each of us is descended from two parents, four grand-parents, eight grand-grand-parents, and so forth; so that, unless we could hunt up our pedigrees in every direction for ten generations, involving a knowledge of no less than 1,024 different persons at the tenth stage backward, we could not even say how far we ourselves were descended from Irish, Scotch, Welsh, or English ancestors respectively. As a matter of fact, everyone of us is now, probably, a very mixed product indeed of Teutonic, Celtic, and still earlier elements, which we cannot practically unravel; and, perhaps, all we can really do is to point out that here one kind of blood is predominant, there another, and yonder again a third.

The very earliest race of men who ever lived in England are probably not in any sense our ancestors. They were those black fellows of the palæolithic or older stone age, whose flint implements and other remains we find buried in the loose earth of the river-drift or under the concreted floors of caves, and who dwelt in Britain while it was yet a part of the mainland, with a cold climate like that of modern Siberia. These people seem to have lived before and between the recurrent cold cycles of the great glacial period; and they were probably all swept away by the last of those long chilly spells, when almost the whole of England was covered by a vast sheet of glaciers, like Greenland in our own time. Since their days, Britain has been submerged beneath several hundred feet of sea, raised again, joined to the continent, and once more finally separated from it by the English Channel and the Straits of Dover. Meanwhile, our own original ancestors—the people from whom by long modification we ourselves are at last descended—were probably living away in the warmer south, and there developing the higher physical and intellectual powers by which they were ultimately enabled to overrun the whole northern part of the old world. Accordingly, interesting as these older stone age savages undoubtedly are—low-browed, fierce-jawed, crouching creatures, inferior even to the existing Australians or Andaman Islanders—they have yet no proper place in a pedigree of the modern English people. They were the aboriginal inhabitants of Britain; but their blood is probably quite unrepresented among the Englishmen of the present day.

Long after these black fellows, however, and long after the glaciers of the ice age had cleared off the face of the country, a second race occupied Britain, some of whose descendants almost undoubtedly exist in our midst at the present day. These were the neolithic, or later stone-age men, who have been identified, with great probability, as a

branch of the same isolated Basque or Eustrarian race which now lives among the valleys of the Western Pyrenees and the Asturias mountains. They seem to have crossed over into Britain while it was still connected with the Continent by a broad isthmus, or, perhaps, even by a long stretch of land occupying the entire beds of the Channel and the German Ocean. Our knowledge of them is mainly derived from their tombs or barrows—great heaps of earth which they piled up above the bodies of their dead chieftains. From these have been taken their skeletons, their weapons, their domestic utensils, and their ornaments, all the latter objects having been buried with the corpse, for the use of the ghost in the other world. From an examination of these remains, we are able largely to reconstruct the life of the Eustrarian people—the earliest inhabitants of Britain whose blood is still largely represented in the existing population.

In stature, the neolithic men were short and thick-set, not often exceeding five feet four inches. In complexion, they were probably white, but swarthy, like the darkest Italians and Spaniards, or even the Moors. Their skulls were very long and narrow; and they form the best distinguishing mark of the race, as well as the best test of its survival at the present day. The neoliths were unacquainted with the use of metal, but they employed weapons and implements of stone, not rudely chipped, like those of the older stone age, but carefully ground and polished. They made pottery, too, and wove cloth; they domesticated pigs and cattle; and they cultivated coarse cereals in the little plots which they cleared out of the forest with their stone hatchets or tomahawks. In general culture, they were about at the same level as the more advanced Polynesian tribes, when they first came into contact with European civilisation. The barrows which they raised over their dead chieftains were long and rather narrow, not round, like those of the later Celtic conquerors. They appear to have lived for the most part in little stockaded villages, each occupying a small clearing in the river valleys, and ruled over by a single chief: and the barrows usually cap the summit of the boundary hills which overlook the little dales. Inside them are long-chambered galleries of large, rough-hewn stones; and when these primitive erections are laid bare by the decay or removal of the barrow, they form the so-called "Druidical monuments" of old fashioned antiquaries, a few of which are Celtic, but the greater part Eustrarian.

At some future period I hope to lay before the readers of Knowledge a fuller account of these neolithic people and their existing remains. At present, the points to which I wish to call attention are, firstly, the fact of their existence in early days in Britain; and, secondly, the fact that many of their descendants still remain among us to the present day. Nor do I propose in this paper to estimate the numerical strength of the Eustrarian element in the population of the British islands as it now stands. It will be best to consider that part of the question at a later point in this series, when we have seen what were the subsequent races which overcame, and, in fact, displaced, the aboriginal Eustrarian folk. For the moment, it will suffice to point out that before the arrival of the Celts and other Aryan tribes in Britain, these Eustrarians spread over the whole of our islands, and were apparently the only people then inhabiting them. At least, the monuments of this date—perhaps from 5,000 to 20,000 years old—seem to be similar in type wherever they occur in Britain, and to contain the remains of an essentially identical race. I shall also add here, by anticipation, what I hope to show more in detail hereafter, that their descendants exist almost unmixed at the present day as the so-called Black Celts in certain parts of Western Ireland and Scotland, and in a few places in South Wales; while their blood may be still traced in a more mixed condition in Yorkshire, Lincolnshire, East Anglia, the Scotch Highlands, and many other districts of England and Scotland. How they have managed to survive and to outlive the various later Celtic and Teutonic conquests, we shall have to inquire when we come to consider the origin and progress of those subsequent waves of population.

BIRTH OF THE MOON

By Tidal Evolution.

By Dr. Ball, Astronomer-Royal for Ireland.

PART II.

UP to the present point, dynamics have guided us with unfailing accuracy, but if we attempt to look back still earlier, we have not the sure light of dynamics for our guide. Yet it is impossible to resist a speculation as to how the moon and the earth came into this wondrous relation. Mr. Darwin has made the suggestion that most probably the moon was actually fractured off from the earth. This is indeed a romantic origin for the moon, but listen to the grounds by which it may be substantiated. In those early days, before we believed the moon existed, the sun raised tides on the earth as he does at present. It was, no doubt, the case that the earth had then no oceans of water on its surface. The tides were manifested by actual throbs in the soft or molten materials of the earth itself. Twice a day the earth rose and fell under the pulses of the solar tides, but as the day was then only three hours, the interval between one high tide and the next was but an hour and a half. The earth was thus in a state of vibration in consequence of the solar tides. These solar tides were, no doubt, small, as the solar tides are small at the present day. But at that very remote epoch it seems not unlikely that there was a particular circumstance which was calculated to exaggerate erroneously the influence of the solar tide. The point now referred to is not an easy one to explain; let me try to simplify it by an illustration. A heavy weight is hanging by a string—say, for example, a weight of 14 lb. is suspended by a string a yard long, with a light wooden mallet weighing an ounce or less: you give the weight a series of blows—generally speaking you will not succeed in giving to the weight any large degree of swing, but if you carefully time the blows so that they shall harmonize with the natural swing of the weight, you will find it quite easy in a short time to give to the large weight as great a swing as you may desire. Your success has depended upon the fact that the impulses were timed to harmonize with the natural vibrations of the weight. In a similar manner, the semi-molten mass of the earth had a period of vibration. Impulses small in themselves which did not harmonize with that period could produce but a trifling effect. It has, however, been shown that the natural period of the vibrations of the molten earth must probably have been about an hour and a half. This, it will be remembered, is also the period of the solar tides. Here, then, we see how the solar tides in that early epoch may have risen to transcendent importance. It is also very significant that a period of rotation equal to three hours is very close to the most rapid rotation which the earth could have possessed without actually falling in pieces. Here, then, we have all the elements necessary for a rupture. The earth is on the point of breaking by its rotation, then the solar tides come into action, each tide augments the effect of the previous tides, until at length the earth, dis-

tracted by tremendous oscillations, has broken off a mighty fragment. That fragment formed the moon.

The date of this occurrence (or, to speak more precisely, the date when we find the moon to have been placed as if this occurrence had happened) we cannot tell. It is certain that it must have been more than fifty million years ago—it is probably very much more. The subsequent history of the moon can be traced with comparative certainty. It appears that the critical condition in which the moon was, close to the earth and rapidly rotating around the earth in a period equal to the day, could not last. The case is one of unstable equilibrium; either the moon must fall back again into the earth, or else it must begin to move outwards from the earth. The fact that the moon exists shows that the latter alternative was adopted, though it does not seem quite clear why that course rather than the other should have been chosen. As the moon receded, the duration of the month increased, its duration at any distance being determined by Kepler's laws. The month has increased steadily from its primitive value of three hours, up to the present time, when the month is over 27 days.

This alteration in the length of the month has entailed a corresponding alteration in the length of the day. As the distance of the moon increased, so the length of the day increased from the primitive three hours up to the present 24 hours. The ratio between the day and the month has, however, altered in a manner which must receive careful attention, as it involves consequences of the very deepest interest. In the primitive state of things, the day and the month were equal; but when they both began to lengthen, the month increased much more rapidly than the day. Of course, it will be understood that we are here speaking of the changes in the ratio of the length of the month to the length of the day at the same epoch. The month gradually became twice the day, it became three times the day, and the ratio gradually increased until the time came when the month was twenty-nine times the day. This time has but lately passed, the ratio of the month to the day was then at its maximum, and the decline has now commenced. After the month was twenty-nine times the day, the ratio gradually sank until the length of the month was twenty-seven times that of the day. This is an epoch of the most special interest—it is the present time.

The tides have thus guided us in tracing the earth-moon history from the beginning, when the moon was first cast off, down to our own days. Nor will the tides now desert us—they will enable us to make a forecast of the distant future. The day will continue to lengthen, the moon will continue to recede, the month will get longer (measured by hours), but the day will lengthen more rapidly than the month. Instead of the month being 27 days, it will in time to come be only 26 days, only 25 days, and at some enormously distant epoch the final state of things will have been reached, and the day and the month will be again equal. The first stage of this history and the last stage are in one sense identical. In each case, the day is equal to the month. In the first case, the day and the month are each three hours; in the last case, the day and the month will each have lengthened to the enormous extent of 1,400 hours. The 1,400 hours is no doubt more or less doubtful, but we are assured by the laws of dynamics that there is some magnitude of that kind to which both day and month are tending, and to which they will both ultimately become equal. In another way, also, the first stage of the earth-moon history and the final stage may be compared. The earth turned the same face constantly towards the moon at the beginning. The earth will turn the same face constantly towards the moon at the end.

FALLACIES ABOUT LUCK.

By the Editor.

As to fallacies about luck, the supposition that after a great number of heads in fifty tossings, the next fifty would probably show a smaller number, involves precisely the same error (diluted by being spread over a larger space, but not diminished in amount) that I dealt with in my former paper. How can the number of heads in one set of fifty tossings affect the number which shall appear in the next? Science says on *à priori* grounds, "not at all": Experiment repeats as emphatically (it could not say so more emphatically) "not at all." But then, says the querist, how is it that, as science assures us, there is always in the long run an approach to equality in the number of heads and tails tossed in a great number of trials. If the balance always tends to the horizontal position, surely a movement of one scale upwards should assure us that presently the other scale will begin to rise. Equality is indeed brought about in the long run, but not in the way imagined. Absolutely not the slightest influence is produced on the results of one set of, say, a hundred tossings, by the observed results of the next preceding set: (how *could* there be?). Nor is there any tendency in a very long series of tossings, starting from some particular point, to reduce a discrepancy between heads and tails, which had attained any amount up to that point. On the contrary, if we count from and after that point, as well as if we count from and after the absolute beginning, we shall find the same tendency to equality in the results of a great number of tossings. The excess of heads over tails, or of tails over heads, may go on increasing, and yet there is the tendency to equality which science indicates. This sounds paradoxical, but it is what science teaches and what experience confirms. It is demonstrable that the greater the number of trials of coin tossing, the nearer will the *ratio* of heads to tails approach to equality, though the actual excess of one over the other may probably be greater, and possibly much greater, than in a smaller number of trials.

Take a very simple case. Suppose a coin tossed four times, and consider the chance that there will be either two more heads than tails, or two more tails than heads. There are in all 2^4, or 16 possible events. That there may be two more heads than tails, three heads must be tossed, which can happen manifestly in four different ways, for the first, second, third, or fourth toss may give the single tail. So, also, there may be two more heads than tails in four different ways. There are therefore 8 ways (out of 16) in which either heads or tails may show three times as against one of the other kind. The chance is therefore $\frac{1}{2}$, or it is an even chance, that there will be this degree of discrepancy. On the other hand, there are only 6 ways in which there can be 2 heads and 2 tails, for only 6 pairs can be made out of 4 (the first tossing may be head, as also second, third, or fourth; the second may be head, as also third or fourth; the third may be head, as also the fourth; and these arrangements of 2 heads give also all the arrangements of two tails). Thus the chance of absolute equality is only 6-16ths, or 3-8ths, that is, the odds are 5 to 8 against absolute equality, while the chance that there will be a difference of 2 exactly between the heads and the tails is $\frac{1}{2}$. (The chance that all 4 will be of the same kind is, of course, 1-8th.)

Now compare with this the results we get when, instead of 4, there are 8, tossings. Here there are 2^8, or 256 possible events, and it can readily be shown (but I leave this and the general problem to a series of papers which I shall hereafter write on probabilities) that the chances of

the different results, and the odds respecting them, are as follows:—

	Chance	Odds
All heads or all tails ...	1-128th	127 to 1 against.
All but 1, heads or tails ...	1-16th ...	15 to 1 against.
All but 2, heads or tails ...	7-32nds ...	25 to 7 against.
All but 3 heads or tails ...	7-16ths ...	9 to 7 against.
Four heads and four tails	35-128ths ...	93 to 35 against.

The most probable of all events in this case, as in the last, is that there will be 2 more heads than tails, or *vice versâ*; and whereas in the former case it was an even chance that there would be just this discrepancy, the odds in the present case are 9 to 7 against it. But the chance that there will be this discrepancy *at least* is greater with the greater number of trials. For in the former case the odds were but 5 to 3, or 175 to 105, against absolute equality, in the present case they are 93 to 35, or 279 to 105 against it. And it can be shown that it becomes less and less likely the greater the (even) number of tossings, that there will be absolute equality. Yet, on the other hand, in the cases considered, the chance that heads will exceed tails, or tails heads, not by a given amount, but in a given degree, diminishes as the number of tossings is increased. Thus with 4 tossings, the chance that heads will be to tails as 3 to 1 (or *vice versâ*) is, as we have seen, one half; with 8 tossings the chance of this relation holding (6 of one kind, two of the other) is only 7-32nds. Again, the chance that heads will be to tails, or *vice versâ*, in a ratio of not less than 3 to 1 is 5-8ths in the former case; in the latter (adding together 1-128th, 1-16th, and 7-32nds), we find it to be only 37-128ths; in one case the odds are 5 to 3 in favour of that amount of discrepancy at least, in the other they are 91 to 37 against there being a discrepancy so great.

But some correspondents ask whether, even in matters of pure chance, there may not be something more than mere accident,—whether some men may not have a certain degree of good fortune given to them,—whether, in fine, what is called luck may not in some degree depend on Providence. This takes us a little outside the domain of science; but as it does not bring us upon any of the vexed questions of dogmatic religion, I will venture to make a remark or two on this (in reality) unscientific aspect of the question. To the student of science it appears as absurd to imagine that the laws of nature would be set on one side in matters of pure chance (for even in coin tossing nothing short of a miracle can cause the law of averages to be departed from in the long run—either in favour of any one or against him) as it would be to conceive that an experimenter favoured by Providence might get a mixture of carbonic acid gas* and nitrogen to behave like a mixture of oxygen and hydrogen, or as it would be to suppose that during Darwin's researches into the work of earth-worms, these creatures, *suadente diabolo*, acted in a way not natural to their kind. If in the case of so-called lucky gamblers, a supernatural power, good, bad, or indifferent, has been at work, science has no power of dealing with the phenomena. All science can say is, that the observed and recorded phenomena agree precisely with those which can be shown to be necessary consequences of the laws of probabilities: all she can do is to go on dealing with the matter precisely as a Pasteur would go on dealing with the observed phenomena of disease germs, uninfluenced by any suggestions that diseases were produced by supernatural agencies.

* I am perfectly aware that what was called carbonic acid gas twenty years ago now goes by another name; and I am equally aware that a technical meaning is given to the word "mixture" other than its ordinary significance. But I am not addressing chemists just now.

So far, I have simply considered what science necessarily does in such cases. The student of science can do no otherwise. But I may note, in passing, that just as there seems to be something irreverent in the suggestion of Providence arranging for the "breaking of the bank" by a Garcia or any other unprincipled gambler, so the general suggestion that Providence, and not the laws which have been assigned to the universe (how or why we know not), is to be credited or discredited with all the chances or coincidences which seem surprising to us, appears to me singularly dangerous to the faith of the weaker minded. Because, while many of these coincidences have been satisfactory enough in their results, at least as many have been very much the reverse, and not a few utterly deplorable.

Take for instance the following case:—

In the winter and early spring of 1881, in America, railway accidents were very common (231 happened in the first two months of that year), and any one who had (as I had) much railway travelling to do at that time had a very fair chance of coming in for wounds and contusions, if not worse.* Now it so chances that at the end of February, a train was wrecked in Missouri, in which two persons were killed and many injured. Another train was sent, carrying several medical men, and a number of appliances for the relief of the wounded. By a most unfortunate chance, this train, thus forwarded to help many suffering persons, was itself wrecked; seven persons were killed, including several of the doctors. If we are not to consider this strange and sad coincidence as belonging to the chapter of accidents, as due to the chances which always affect events depending on natural causes (as the weakening of embankments by frost and thaw, the action of winds, rain, snow-drifts, &c.), must we regard it as due to special intervention of Providence? Science tells us, and experience confirms her teaching, that in the game man plays (or his contest, if you will) with nature, the laws of nature are as laws of the Medes and Persians, that he must not expect to have his moves back, or any help outside the laws assigned (inexplicably so far as we are concerned) to nature: if he does expect this, he will most assuredly be disappointed.

THE CRYSTAL PALACE ELECTRICAL EXHIBITION.

Third Notice.

THE most casual observer can discern some improvement during the past week, although there still remains a great deal to be done before anything like completeness can be said to be attained. Some of the exhibits exist only in the catalogue, and it seems apparent that no attempt will be made to proceed any further than a display of advertisements. It has occurred to us that some

* For my own part, I thought it exceedingly likely that before the lecture season came to an end, my lecturing might be interrupted. As week passed after week without an accident, I neither judged that the next journey was more likely or that it was less likely to be disastrous. At last, towards the end of February, my turn came. The train I was in was pitched over an embankment, not far from Richmond, Missouri, falling some twenty or thirty feet, and a stove drove its way through a stout plank within two inches of the place where my head lay. Was I lucky or unlucky? unlucky in going by that particular train; or, being in it, lucky in escaping with no injuries worse than a nearly fractured skull and a nearly broken leg. As I and the other passengers looked at the shattered interior of the car, we thought ourselves lucky to be alive; as we considered the various damages which our persons and property had sustained, we took a different view. (The accident turned out afterwards to have been singularly fortunate for me, but that is a detail.)

advantage would be gained by the directors prohibiting the fixing of any advertisements whatever, until the exhibits they refer to are duly placed in position, or something approaching thereto. The most notable advances are in electric lighting. The Brush Company, to which reference was made last week, have got all their five engines *in situ*, and three of them running.

The visit of the Lord Mayor two or three weeks since has caused considerable delay in the general preparations, some of the now unprepared firms having had (in order to contribute towards making a display suitable to the occasion), to start their engines before the concrete foundations had dried; consequently, these foundations were pulled to pieces and had to be re-laid. In the Italian Court is one of the prettiest exhibits conceivable. It consists of a brass chandelier of delicately chaste design, decked with forty-two incandescent lamps of the British Electric Light Company. They are very regular, and appear to have been carefully selected. Although the light from them thoroughly illuminates the court, still greater effect is to be produced, as the Company is at present using only one 8-horse-power engine to maintain seven Brockie (arc) lamps, in addition to the forty-two incandescent lamps. When all is in working order, there will be two more engines at work, each of 16-horse-power.

Consequent, presumably, upon the serious railway accidents which have so alarmed Londoners lately, great attention is centred on the various exhibits for improving our systems of signalling, &c. In the Eastern Gallery, far away from the general bustle, is a working model of a system invented by Mr. King, of Paixton, Derbyshire, exhibited by the Electric Railway Signal Company. The model illustrates the working of three signal posts controlling a main line and a branch line connected with it. There is a treadle between the rails opposite each signal post and in connection with it. A train in passing over the treadle connected to, say signal-post A, puts that signal, by a mechanical contrivance, to danger—that is to say, the line is automatically blocked to a following train. The signal cannot be lowered until train No. 1 passes signal-post B, in passing which, by pressing on the treadle, it puts post B to danger by mechanical means, and, simultaneously, by completing an electrical circuit, lowers the signal at post A. Train No. 2 is therefore at liberty to pass post A, but cannot pass post B until train No. 1 has passed post C., and so on throughout the line. If the train has to go on to a branch line, the lever operating the points interlocks with the signals, in this case blocking the main line and clearing the branch. These signals can be reversed (that is to say, the main-line signal cleared and the branch line signal blocked) only by altering the lever. It is evident, then, that by this system there cannot be two trains in any one section of the line at the same time, unless the driver of one of the trains ignores or fails to see his signal.

An arrangement is also shown by which a train, in passing a signal box, puts the minute-hand of a clock back to zero. The hand then travels on for fifteen minutes, or until the following train puts it again to zero. In this way, the driver, as well as the signalman, can see how long a time (up to fifteen minutes) has elapsed since the passing of the preceding train. Another arrangement (not shown in the model) is to indicate the number of trains passing during the twenty-four hours, and the time at which each passed. While, however, several high authorities express a wish for a good automatic system, some of the railway officials appear to desire an improved system of hand-signalling. The King patent is readily adaptable to this form, and it is to be hoped that before long we may see such a system as this in general use. The application of this or any other new form must be a process of time, considering the radical change that would be necessary in existing apparatus, and the large outlay involved. Until the system is tried, it is somewhat difficult to form an opinion of its practicability, although to all appearance it is as perfect for clear weather signalling as can possibly be desired. It does not attempt to overcome the difficulties accompanying foggy weather; and here is apparently its weakest point. In the North Nave, however, is a model of apparatus designed for this purpose, and exhibited by the British and Irish Telephone and Electric Works Company. The model is designed by Mr. Radcliffe, of Birmingham. He makes some use of electricity. Near the signal-post is an electro-magnet (M Fig. 1), over this is a piece of soft iron (A), which, when a current of electricity passes through the electro-magnet, is drawn downwards. When the current ceases, a spring (S) draws A up again.

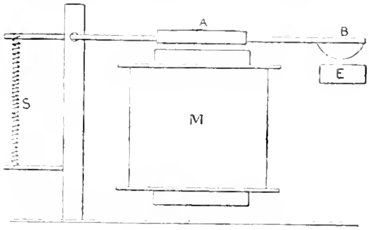


Fig. 1.

Attached to A is a short rod, carrying a block of iron, brass, or any durable substance (B). When the signalman pulls his lever over to drop the signal-arm, the electrical circuit is completed, so that A, and with it, B, are drawn down. Attached to the side of the engine is a lever, or simple rod of iron (represented in section by E), which, if drawn down, acts by means of a spiral spring upon a miniature signal-arm on the driver's platform, within a few inches of his eyes, actuating at the same time his whistle. If the signal is "down" when the engine approaches the magnet, B only touches E, which, in passing under B, simply shakes the miniature arm. If, however, the signal is to "danger," A is drawn down by the current, and B presses the rod E down, and so puts the miniature arm to danger, and by simultaneously blowing the whistle, draws the attention of the driver to the position of the signal and the attention of the signalman to the position of the train.

This, then, if found practicable, is an efficient substitute for fog-signals. It is very simple and ingenious, and it is to be hoped it will prove effective, at least in crowded districts, where the trains, although frequent, do not travel at a very high speed. It is more than possible, we fear, that there may be difficulties in the way of applying the system to fast trains.

REVOLUTION IN A HERD OF DEER. For years an old buck, the leader of the deer herd on the Boston Common, has maintained an absolute and malicious tyranny over the younger members of his own sex. His treatment rankled, and the other day, when he shed his horns, they made a combined attack upon him, which only ceased upon the death of the tyrant. The Superintendent and his assistants attempted to interfere, but were driven out of the inclosure by the infuriated animals, which became docile again when their enemy was disposed of. They still preserve, however, a sort of *sic semper tyrannis* air, and thus far, no one of their number has laid claim to the primacy.—*Scientific American.*

THE RAINBAND.*

EVERYONE who notes barometer indications and carefully compares them with the weather, knows that the barometer is but an unsatisfactory weather guide. There is promise, however, that with an aid apparently insignificant (certainly insignificant in size and expense), we may be able to predict the coming of wet weather with considerable certainty. A pocket spectroscope, directed to any part of the sky, not too near the horizon, will show the presence or absence of the rainband (which Prof. Piazzi Smyth may be said to have discovered, since he first directed attention to its importance),† and so will often tell us of the approach of rainy weather not indicated by the barometer, or that when the barometer points to rainy the weather will in reality be dry. The strength or faintness of this band in the spectrum indicates, in fact, the excess or deficiency of aqueous vapour in the air as compared with the average. With a little practice in the use of one of the rainband spectroscopes advertised in our columns, assisted by study of the little pamphlet before us (which Mr. Browning supplies with his pocket spectroscopes for rainbow study), an Englishman may become so independent of his umbrella, when it is not going to rain, that his best Continental or American friends would not recognise him.

THE GREAT PYRAMID.

By the Editor.

I HAVE prepared two views of the Pyramid regarded as a structure for observation, but as there is great pressure this week on our space, and these views will occupy nearly a full page, it has seemed well to defer that part of my subject to the week after next. I take the opportunity to discuss, as I promised several weeks ago, the curious coincidences which many have regarded as demonstrating what may be called the divine-inspiration theory of the Pyramid.

With the discovery that the base of the Pyramid is several feet shorter than had been supposed, a number of relations supposed to connect the Great Pyramid with astronomy go overboard at a single stroke. I had written a paper showing how singular these relations are, but at the same time how obviously they result from mere coincidence; and now, alas! (another strange coincidence?) the relations themselves disappear, and my remarks upon them have no longer any weight. Still, the coincidences are there. Indeed, it only requires that the Pyramid inch should be slightly altered for the relations to be all once more perfectly fulfilled. What will be done with the arguments showing the true Pyramid inch to be almost exactly the same as the British inch, and the true cubit to be twenty-five of these inches, I do not know: but past experience shows that whatever the precise value of the Pyramid inch, as deduced from these new measures, may prove to be, will be shown to be just the value which corresponds most perfectly with what may be called the Pyramid religion. So, after all, my article may come in well enough. However, I am not so particularly fond of demolishing giants of straw that, when the straw stuffing has been ruthlessly pulled out, I should persist in my attack. So I will here present now very briefly what I had before advanced at some length.

We find that while the Pyramid fulfils closely the relation which Herodotus says it was intended to fulfil, each slant face being equal in area to the square of the height, it also very nearly fulfils what Taylor tells us was the real purpose of the builder, the height being nearly equal to the radius of a circle having a circumference equal to the perimeter of the square base; and again, it almost as closely fulfils another relation, in having the slant at the edge very nearly as 9 vertically to 10 horizontally. Now, to the ignorant, it seems as though the close approximation of the building's proportions to these three relations, proves demonstrably the mathematical skill of the builders, if not their divine inspiration. As a matter of fact, however, we see from the co-existence of these three relations, any one of which might as well as another be the real one which the builders had in view (were it not certain from what Herodotus tells us, that the first only was their building rule), how easy it is to find such relations if we only look carefully for them, for two out of the three are certainly accidental. So that apart from the evidence of Herodotus, we should be free to reject all three, on the sound plea that since coincidence can so readily be detected, no reliance can be placed upon any argument from *mere* coincidence.

Then, again, according to the measurements just negatived, there were exactly as many cubits of 25 inches in each side as there are days in the year, or 36,524 inches in the circuit of the base. One would have said that if this were really proved, and if the height were determined by any one of the three geometrical rules just indicated, all the dimensions of the Great Pyramid, as a whole, were determined once for all. But even in the early days of the Pyramid religion, the Pyramidalists were not content with this. They found that the two diagonals of the square base together contained as many inches as there are years in the Great Precessional Period, and that the height contained as many inches as there are in the one thousand millionth part of the sun's distance; though, of course, if these relations really hold, they indicate coincidences, and very singular ones too, entirely outside of the Pyramid. As thus: Take one-fourth the number of days in the year, and double the square of this number; the square root of the product equals half the number of years in the Great Precessional Period. And again, take 100 times the number of days in the year, and reduce the number thus obtained in the same ratio that the radius is less than the circumference of a circle; you will then have a number equal to the number of inches which there are in one thousand millionth part of the sun's distance. These two relations exist quite independently of the Pyramid, and, so seen, even Pyramidalists must admit that they are but singular numerical coincidences. They have not a particle of real significance, any more than this one, which I make Pyramidal (by a very transparent device) merely to show how easy it is to work such things:—Take the square base of the Pyramid, and divide each side into as many parts as the Pyramid has faces. Join the corresponding divisions of opposite sides of the base so that the base is divided into sixteen squares. In each of

* "A Plea for the Rainband." By J. Rand Capron.

† Mr. Capron, in mentioning that Prof. Smyth has made himself owner of three parts of the rainband, falls into a somewhat amusing mistake about certain lines which may belong, possibly, to the nursery rhymes of the future, and, therefore, must be carefully guarded from change. He says that in university rhymes, Mr. Lockyer is said to have "made himself owner of half the corona;" whereas, in the original rhymes, written along with many others during the eclipse expedition of 1870 (at which time Mr. Lockyer supposed the now abandoned theory of the corona to be unquestionably sound) the words were, "'Of the solar corona,' says he (L.), 'I'm the owner.'" This will be of use to antiquarians of the future; just as those of our own day are enlightened by the record showing the real history of little Jack Horner, and the real nature of the plum he so deftly abstracted.

these squares, save one, place a number (after the manner of the abomination of desolation to which in our own post-Pyramidal days hath been assigned the name of the "Fifteen Puzzle")—then it may be shown that the number of arrangements which can be made of these fifteen numbers in the aforesaid sixteen squares is equal to the number of miles separating our solar system from that star which, according to the best Egyptological investigations of the date of the Great Pyramid, shone, at its meriodinal culmination, directly down the Great Gallery and its prolongation the ascending passage.

Then comes my ingenious and (outside the Pyramid) scientific friend, Mr. Baxendell, who, accepting the Pyramid dimensions assigned by Professor Smyth, finds other relations which they fulfil equally well, showing, of course, other singular coincidences existing quite independently of the Pyramid. Nay, he finds several independent coincidences for each dimension, failing, apparently, to notice that the most remarkable feature of his paper—the singular closeness of the numerical results—exists (scarcely in diminished degree) if the Pyramid be left entirely out of the question. Take, for instance, what I find many regard as singularly impressive, the six different formulæ, by which he gets out 1881·59 as the number of inches in the length of the Grand Gallery (which I need hardly say is not known to anything like this degree of exactitude). They are as follows:—

$$\frac{s\pi^2\pi\sqrt{\pi}}{25,000} = \frac{es\pi^4}{e^2\sqrt{SM}} = \frac{\sigma^4\pi\sqrt{\pi}}{400,000e^4} = \left(\frac{\sigma^2\sqrt[4]{\pi^3}}{e^2 2\sqrt{10^5}}\right)^2 = \frac{S^2\pi\sqrt{\pi}}{400,000\eta^2}$$
$$= \frac{S\sigma^2\pi\sqrt{\pi}}{400,000e^2\eta} = 1881{\cdot}59$$

How terrible these formulæ appear, in conjunction with the circumstance, that by taking dates for the Fall, the Exodus, and the birth of Christ, not quite agreeing with those approved by recognised theological authorities, the length of the descending and ascending passages correspond so closely with the intervals between the first and second and the second and third of those events (years representing inches), as to compel us to believe that the Christian dispensation cannot last more years than there are inches in the Grand Gallery. Now these formulæ, when analysed, are found to indicate a number of really curious coincidences between the numbers representing S, the sun's distance, M the moon's, s the sun's diameter, e the earth's (equatorial), σ the diameter of the sun's liquid body—quietly assumed, for we know nothing about it—η another terrestrial diameter, and π the ratio of the circumference to a diameter of a circle. If the Pyramid had no existence, these curious coincidences would remain. The fact that they exist, and are in themselves so singular, shows simply how little value there is in the argument from mere coincidence. Given ten or twenty numbers taken at random from different columns of the *Times* newspaper, or the dimensions of a house, or field, or piece of furniture, or, in fine, taken from anywhere we like, it will be found that with a little patience, any number of coincidences may be found among the numbers themselves, or connecting them with any other set of numbers, with the dimensions of the solar system, with the volumes, diameters, densities, &c., of the planets, or, in fine, with whatsoever we please. One of the best proofs ever given of this is found in the multitude of relations, independent of the Pyramid, which have turned up while Pyramidalists have been endeavouring to connect the Pyramid with the solar system. These coincidences are altogether more curious than any coincidence between the Pyramid and astronomical numbers; the former are as close and remarkable as they are real, the latter, which are only imaginary, have only been established by the process which schoolboys call "fudging,"—and now new measures have left the work to be done all over again.

BRAIN TROUBLES.

Punning.

It is not, perhaps, commonly known that a tendency to make puns is regarded by many students of mental physiology as a sign of cerebral disease, a circumstance which we would commend to the notice of those persons who are always on the watch to play upon words, without caring whether their word-play is amusing or not. Whatever opinion we form respecting puns, between the extreme views that a man who would make a pun would pick a pocket, and, as Hood extravagantly maintained (in reply to the saying that puns are the lowest form of wit), that puns are the foundation of all wit, there can be no doubt that puns of a certain sort indicate a ready, bright, and witty mind. But the wit of a punning remark depends entirely on the ideas conveyed by the word or words used in a double sense, not on the pun itself. We laugh at the lines—

> They went and told the Sexton,
> And the Sexton toll'd the bell,

because of the absurdity of the ideas suggested. We are struck by the cleverness of other puns, because of the truth of the words, in whatever sense we understand the words played on. But we find nothing amusing or clever when a second meaning, neither humorous nor sensible in itself, is given to anything that has been said in ordinary conversation; and when a confirmed punster seizes every word capable of bearing two meanings, and expects us to laugh at his word-play, we not only are not amused, but soon become unutterably bored. Yet, although it implies a wrongly-directed mind to make puns in this purposeless way, under the impression that they are amusing, it does not necessarily imply impending idiocy or insanity; for in the majority of cases of this kind, the punster does not yield to an impulse to play upon words, but works very hard to acquire the trick of verbal torturing. He may be compared to one who, having observed that the tricks of a clever clown have been received with approval, tries to excite equal merriment by grimaces which are not in the least funny, and which, if he really could not help making them, would indicate either that he was insane, or else that he had St. Vitus's dance or some other nervous disease. Precisely as such buffoonery would, in reality, signify only want of sense, not insanity or disease, so the habit of making witless puns implies only a feeble, not a diseased, mind. The case is different, however, when one who is sensible enough to see the folly of mere word-twisting finds his mind turning, as it were, against his will, to the profitless task. We cannot fail to recognise the signs of incipient brain mischief, for instance, when we see Swift taking pains to twist the name of "Alexander the Great," into "all eggs under the grate." It would have been a bad sign if he had made so wretched a joke in conversation, though an ordinary mind might have done so without suggesting the idea that the mental machinery was out of order; but that the author of "The Tale of a Tub," should be at the pains to write down such nonsense was of evil portent indeed. The matter might seem trifling enough in itself, as would it be a matter intrinsically of small moment if Mr. Gladstone or the Bishop of London chose to walk down Pall Mall in a nightcap instead of their customary head-gear; but no one who rightly understands mental phenomena could doubt

that Swift's mind was beginning to be affected when he made such feeble jokes, any more than he would doubt that a great statesman or a grave prelate who should walk along a London street in night attire was, for the time being, at any rate, insane.

Probably, few persons who have had occasion to tax their mental powers at times to the utmost, have failed to notice this tendency to play idly with words, among other symptoms of want of rest. When noticed under such circumstances, the peculiarity need not be regarded as alarming. If, however, it remains after rest has been obtained, it indicates the necessity for relaxation of a more effective kind.

And here we feel called on to object strongly to a remedy suggested in a little book on "Common Mind Troubles," for the errors in speech characteristic of impaired mental vigour, namely, "reading aloud in one language from a work written in another, for example, a French book to an English audience." It would be as reasonable to recommend persons who showed symptoms of bodily weariness to try the effect of an hour's exercise with Indian clubs or heavy dumb-bells. The proper course is to take rest as soon as possible, and above all things to avoid the mistake of seeking in distraction of the thoughts (which is only another form of "worry") for the good effects which can only be expected from relaxation. Some of the most melancholy cases of mental break-down have been caused far more by social worries sought as remedies, than by the excessive brain work to which they have been too hastily attributed.

Reviews.

CELESTIAL OBJECTS FOR COMMON TELESCOPES.*

THE first edition of this work seemed to us one of the most charming little books on astronomy ever written; the second scarce less so; the third still endeared to us by recollections of its simpler predecessors; the fourth is the first which seems overweighted by details and minutiæ. Perhaps, if we had seen the fourth first, we should have liked it as well as we did the first; yet it cannot be denied that many pages of the work before us are calculated to alarm, rather than attract, the young student of astronomy, for whom the book is specially intended.

The charm of the earlier edition lay, perhaps, a good deal in a certain *insouciance* of style, a neglect of nouns substantive, and of too strict rules of syntax, which was suggestive of enthusiasm. The subject seemed to run away with the writer. Take, for instance, the opening sentences of the chapter on Venus. "The most beautiful of heavenly bodies to the unaided eye is often a source of disappointment in the telescope." (We know somehow—we cannot tell how—that this is not a general proposition.) "For the most part it resists all questioning beyond that of Galileo, to whom its phases revealed the confirmation of the Copernican theory—an important discovery" (not the theory, nor the confirmation) "which he involved for a season in the following ingenious Latin transposition," &c., the well-known anagram about the phases of Venus. Then the work much better deserves to be called Astronomy without Mathematics—and, therefore, to be widely popular—than Sir Edmund Beckett's really profound and masterly treatise, so called. Take, for instance, the following recipe for drawing the disc of Jupiter:—"Make a rectangle 15 high, 16 wide, on any convenient scale of equal parts; find its centre by intersecting diagonals; from this describe a circle touching the top and bottom, and then *pull out*, as it were, the sides of the circle to touch the ends of the rectangle, altering the curve by eye and hand till a tolerable ellipsis is produced." Could anything be less formal or less trammelled by mathematical phraseology than this? The absence of those provoking attempts at explanation to be found in some astronomical books is another charming feature of the work before us. We have, instead, such expressions as, "Here explanation is set at defiance!" "What could it have been?" and so forth; nothing to weary the learner, or unduly tax his reflective faculties.

If Mr. Webb is unwilling to weary his readers, he has evidently not spared his own labours. The book is crowded with information, notes, references, personal experience, strange out-of-the-way facts: it is, in fact, a store-house of astronomic lore. There is nothing like it in its own line; and though the fourth edition goes far to establish old Hesiod's saying, that the half may be better than the whole, it is a work without which no astronomical library (not possessing an earlier edition) would be complete. We could have wished Green's Map of Mars had been reproduced here, instead of the one Mr. Webb has given, which is unlike anything in the heavens above, or elsewhere. But such faults are few.

* *Celestial Objects for Common Telescopes.* By Rev. T. W. Webb. Fourth Edition. (Longmans, Green, & Co., London.) Price 10s.

A Horse's Pastime.—A few years ago, while in North Staffordshire, I saw a horse amusing himself in a rather original manner. On one of the trees at the side of his field, next the road, was a branch about a yard from the ground. The horse stood on this branch with his hind legs, and, planting his fore-feet firmly on the ground, as a fulcrum, gravely see-sawed up and down by swaying the bough, getting on again when he slipped off. He appeared to derive a sort of solemn pleasure from the proceeding.—Tittesworth.

A Generous Bulldog.—My children went out for a walk—girl 12, boy 10 years—taking my dog, a cross-bred bull and terrier, also a retriever dog, belonging to a relative. This latter entered a large reservoir, on being told to do so, and paddled about for some time, amusing the children. By-and-bye he swam amongst some rushes, and they appeared too strong for him to fight his way out. He was called, but seemed exhausting himself and unable to obey. The children called louder, and were, in fact, getting frightened, when our dog, who was on the bank, jumped into the water, swam up to the retriever, seized him by one ear, and, being a strong, muscular animal, he dragged the retriever to land. On getting out, instead of fighting on account of the punishment the retriever had received, they fell to licking each other in a most affectionate manner. Was this instinct?—J. Davidson.

Cod-Sound.—The meaning of this word "sound" for the aorta, or chief blood-vessel, of the cod-fish goes deeper than *sondre*, to sever, or sunder, suggested by Mr. W. M. Williams (p. 295). The root-word is common to several of the chief groups of languages. *Son*, or *sona*, in Sanskrit, is red, blood-coloured; *sonita* is blood. In the Dravidian dialects, *sen* is red, or blood. *Sen* is blood in Egyptian, whence the derivation *sennut* denotes that foundation which in biology is blood. So, in Chinese *sen* is foundation; the heart itself, as well as heart figuratively, the inward and essential basis of being. The Assyrian *suun* also denotes fulcrum and foundation. These meanings all meet in the fish-*sound*, as that which contains the blood, the basis of life, and the name shows how much significance may be concentrated in a single word.—Gerald Massey.

Fox Story.—I can hardly distinguish the action of the fox in the following case from reason. It happened in co. Roscommon, at Kilronan Castle, where I once lived. The foxes this particular time were doing great damage to the pheasants, so the preserves were poisoned. When, in some instances, the keepers went to see if the meat had been taken, they found filth placed on the top by the fox. I have also known of a case where a fox was caught in the evening and put into a barn for the night. When, in the morning, one of the family went out to see if he was safe, he found him on the ground, as he thought, quite dead. He caught him by the tail, the fox not relaxing a muscle, but keeping quite stiff and stark. He rushed into the house to tell the news, when, coming out, he saw reynard running away as fast as he could.—Constans.

EASY LESSONS IN BLOWPIPE CHEMISTRY.

By LIEUT.-COLONEL W. A. ROSS, LATE R.A.

LESSON III.—MANGANESE—COBALT—GOLD.

(*Note Introductory.*)

I SEE your typographical demon has promoted me into the Royal Navy (one of the best pyrologists I know is a Captain R.N.), and should think the printer's other inference, that the rank of colonel has been lately created in the English navy, is also a mistaken one.—*Erratum* in Lesson I. It should be stated that the two "body tubes" of the blowpipe are "two 6-inch pieces," *not* "2 in. pieces." In Lesson II., (1) the firm in Hatton-garden is "Johnson & Matthey," *not* "Johnson & Mather." (2) Freiberg is wrongly spelled with a *u*, not by me!

I hope some of my sharper pupils, having tried the experiment with oxide of manganese, detailed in Lesson II., will find out that I have omitted to mention a very remarkable "reaction" (which is the chemical term for a phenomenon depending upon the application of any particular operation). It is this: On first heating the manganese (brown) oxide* with the phosphoric acid, great effervescence, or bubbling, takes place, and the bubbles are tinged a deep crimson colour. This extremely delicate reaction will detect 0·05 of manganese in minerals or compounds. The phenomenon described in the final paragraph of Lesson II. is evidently the result of holding the bead (on platinum wire) in two different positions as regards the blue, blowpipe-pyrocone. In any position (*a*) in front of the "tip or point of the blue," as it is called, the manganiferous bead assumes an "amethyst," or bluish-violet colour; in any position within the blue, the bead becomes colourless (*b*).

Position (*a*) is called in most blowpipe books "the oxidating flame," or briefly, OF. Position (*b*) "the reducing flame," or RF. I have altered these names in my books, because they do not correctly describe the resulting reactions. In the first place, there is no "flame" at all, but, instead, a cone of non-luminous, blue fire; secondly, many beads (an auriferous, or gold-bearing bead, for instance), so far from being oxidised just in front of the blue tip, suffer a deoxidising or "reducing" process, whilst the position (*b*) does not invariably reduce all substances dissolved in these beads—oxide of chromium, for instance—and produces many phenomena besides reduction, as that of "colouration," "precipitation," &c.† I have therefore, thought it better to describe these important situations of the subject of analysis by symbols expressing the *nature* of the fire which, in that position, attacks it, thus:—HP (instead of RF) for hydrocarbonous‡ pyrocone; OP (instead of OF) for oxyhydrogen pyrocone, and PP (no old name), for peroxidising pyrocone. This last position extends from ½ in. to 2 in., or even 3 in.—if the blast is sufficiently strong—in front of the "blue tip." Let us now revert to our phosphoric, manganiferous§ bead (these are rather long words, but necessary, and easily learned and remembered by the real student). Chemists have ascertained that the common or brown oxide of manganese contains two proportions or parts of oxygen to one of the metal manganese. Their symbol for manganese is Mn, and they, therefore, symbolise this compound thus: MnO_2. They have also found that the "red," or violet oxide contains the proportion three of metal to four of oxygen, and have thus symbolised it Mn_3O_4.

The chemical action of the different parts of the blowpipe pyrocone, therefore, is admirably illustrated by the Mn. bead; for, as we have seen, the relative proportions of metal and oxygen of the compound dissolved in it are actually and materially altered by a simple movement of the hand! The lowest—that is, the nearest metal—known oxide of manganese, is what chemists call the "monoxide,"‖ MnO (obtained by heating the common carbonate in a gun-barrel, through which hydrogen gas is passed), and is a *green* powder; what oxide, therefore, the *colourless* bead after treatment in HP, contains, is not yet, apparently, known.

One of the greatest chemists that ever lived, in days when great chemists did not despise the blowpipe, discovered this curious reaction of manganese in borax. His name was Scheele. He was at first only an apothecary at Koping, in Sweden.

I am now going to ask my student to make another "bead," still prettier than the last, by means of a substance almost as cheap as manganese, and he should go to the same place for it—viz., to the glass-works; I mean oxide of cobalt. The minerals in which cobalt was first found in Germany were so like silver, that when the miners found they did not contain silver, they said they must have been silver changed in character, or be-devilled by some demon, and *Kobold* is the German for demon.

The phosphoric acid, or, in brief, P. acid-bead, is tinged with cobalt oxide, or CoO, in the same manner as the former one was with MnO, (see Lesson II.), but see what a different result we have got! *This* bead is blue hot, but in cooling assumes a magnificent violet colour. It is not altered in appearance by holding it in the positions OP, or HP, or PP—all cause it to be blue hot and violet cold. In all blowpipe tables and books (except mine), you will see "blue" only set against cobalt; but in 1869 I discovered that P. acid gives this beautiful colour with cobalt, and thought that by adding a weighed quantity of soda to the bead before the blowpipe (or briefly, BB) until it remains blue cold, I should obtain a kind of *measure* for the soda added; and, as the bead is thus made blue by any alkali—an alkalimeter,* or alkali-measurer; and this is the fact. You can also measure the quantity of cobalt in minerals, &c., in this way, and in my little book, "An Alphabetical Manual of Blowpipe Analysis," pp. 15 to 18, is given "A Blowpipe Assay of Ores, Furnace Products, &c., for Cobalt."

Now, we must try another substance with, or in, our little chemist P. Acid, and the student need not be alarmed at my extravagance when I tell him it is to be—gold. A tiny little bit of gold-leaf (which should be quite pure, or he will get colour reactions for copper, &c.) about twice the size of a pin's head, cut off with the point of a pen-knife, is taken up at the bottom of a red-hot P. Acid bead, and *kept* there, or it will fly up and alloy the platinum wire, in which event another piece of gold is to be added, under a powerful OP, when the gold will be rapidly dissolved (no other single known acid is sufficiently powerful to do this), and its oxide, as I have before stated, precipitated in this position, making the bead "muddy." The student is now to take up a small fragment of P. Acid at bottom of the hot bead, and hold it steadily in a good PP, just over half-an-inch from the tip of the blue. When the proper amount of oxidation has been applied to the bead in this position, which occupies a time, varying with the blower's capabilities and the perfectness or otherwise of the pyrocone, the auriferous bead will be observed to be a brilliant topaz-yellow when *very* hot; then, in cooling, to become green; then greenish-blue; and lastly, when nearly quite cold, a beautiful blue-violet colour, called, when otherwise obtained, "the purple of Cassius."

Colonel Ross begs to inform Major James Cummings (Query, page 317) and other intending pyrologists, that he will be happy to reply to any private queries on the subject, briefly but concisely put, if sent to him with an enclosed stamped envelope to the following address:—Acton House, Acton, London, W.

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* *Oxygen* (acid generating).—A gas discovered by an Englishman (Priestley) in Birmingham in 1772. It is a component of almost all natural inorganic substances.

† *Precipitation* (a falling down) is the condition when a bead becomes "muddy" or opalescent, which before was transparent, or "clear." The oxide, or substance which before was dissolved in the clear bead, has, by some act, become insoluble, and is "precipitated."

‡ *Hydrogen* (water generator), a gas; with oxygen forms water, and in that form is a constituent of almost all substances, organic and inorganic. It is also with carbon, a component of oils, fats, &c., which are called "hydro-carbons," and therefore, of the ignited gases proceeding from them when burned.

§ *Manganiferous.*—Bearing (or containing) manganese.

‖ *Monoxide.*—Greek Monos one, and oxide.

* *Alkali* (Arabic, Al-kali), the reverse of acid; alkalies turn red, moistened litmus-papers blue; acids turn blue litmus-papers red. Two of the three alkaline metals, potassium and sodium, were discovered by a Cornishman (Davy) in London in 1805 by means of the "Voltaic pile." *Alkalimeter*—alkali and *meter* (Greek)—a measure.

THE BRAIN AND SKULL.

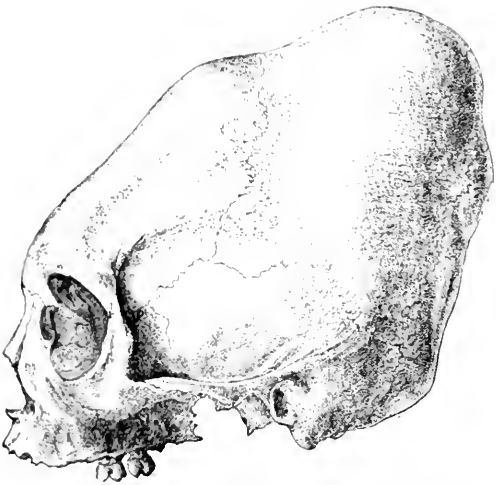
SOME correspondence has taken place in KNOWLEDGE relating to the human brain and its outer envelope, the skull. It has, therefore, seemed to me that a few notes upon facts well known to anthropologists and craniologists would probably be acceptable to readers of KNOWLEDGE.

There exists among numerous barbarous and semi-civilised peoples, scattered over the world, a very curious custom, perhaps it may rather be called *fashion*, of deforming the skull in infancy. This custom has existed from the most remote period, so that in some localities it is very difficult to obtain a skull, the measurements of which can be relied upon as distinctive of race, from the ancient graves. The ancient Peruvians were particularly addicted to the deformation of the skull, and the practice still exists among the American Indians: but the most curious of all these artificially-deformed skulls are those brought from the island of Mallicolo, in the new Hebrides group, a tracing of one of which I

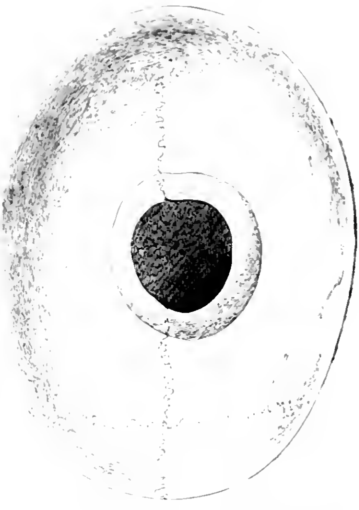
here give from a paper in the Journal of the Anthropological Institute for November, 1881, in which this skull and others exceedingly interesting and curious from the same island are described by Professor Flower, the originals being in the Museum of the Royal College of Surgeons. Here it will be seen that the skull has been worked up into a most singular form, scarcely resembling that of a human being; and this has been evidently effected in infancy by means of a tight bandage bound round and round the head, and only allowing of expansion in one direction. A different mode is adopted by some of the American tribes, as the Flat Heads, who add a board at angles to that upon which the infant is carried, which, being strapped down tightly over the forehead, causes it to become depressed while the skull bulges at the side. A drawing of this curious method of producing a fashionably shaped head may be found in Catlin's work on the American Indians, and these two modes, the tight bandage round the head and the flat boards, seem to be those chiefly adopted for producing the desired form. The first of these, the compression by tight bandages wound round and round the infant's head, may have originated in the supposed necessity for assisting nature in uniting the bones of the skull, open, as we know, at birth, and long after; in fact bandages for this purpose seem to have been used in our own country and France up to quite a recent period, and, of course, a little extra tightness would soon produce a deformity. This method, therefore, can hardly be looked upon as characteristic of race, although, doubtless, the production of a certain form soon became a fashion among many races, as among the ancient Peruvians, whose skulls strongly resemble the one here figured, and Professor Flower also mentions some from Tiflis and Hungary of a similar form; but the other method, whereby the infant's head is compressed between two boards, would seem at present to be confined to certain tribes in North America, and, as it would not appear to subserve any apparently useful purpose, it is probably distinctive of race. In all cases these deformations are generally marks of distinction, like the deformed feet of Chinese women, and often a caste privilege, reserved for chiefs and their families, under which aspect the fact becomes doubly interesting; for we know that among savages chiefs are chosen for their superior mental and bodily powers. Hence it would seem that this compression of the skull, and the consequent forcing of the brain into an abnormal form, has no deleterious effect upon the intellect; in fact, Cook and the two Forsters, who first noticed the peculiar conformation of the heads of the people of Mallicolo on their visit to the island in 1774, although uncertain as to how it was produced, speak of them as "the most intelligent people we had ever met with in the South Seas." This must be a puzzling problem for phrenologists, for it goes to prove that when the volume of the brain remains intact, the form is immaterial, even though that form may be artificially produced with great pain. It would appear that the deformation among the Mallicolese is not confined to the chiefs, but is practised by all alike, without distinction of class or sex, and is produced by a tight bandage applied to the head shortly after birth, extending from the eyebrows to the hair, and only taken off occasionally, until the child is six months or a year old.

Certainly, inferior races in modern times think little of inflicting pain, in which, probably, they resemble the ancients; for singular instances of a surgical operation performed upon the skull of young children in the later Stone Age are found in tombs in France and other parts of Europe. The late Dr. Broca, the eminent French anthropologist, was the first to notice this singular fact. Many skulls had been found in tombs belonging, without doubt, to the Stone Age, in which holes of considerable size had been made, evidently during life, as the wounded bone had become healed, and in most cases it was evident that the head had increased in size after the operation, proving that the trepanning had taken place at a very early age. I have here copied from Dr. Broca's book one of these curious skulls, with the large hole just on the top, and it seems wonderful that a child could survive such a serious operation, and live to maturity, or even old age, with the brain thus exposed; for it must be remembered that this operation, which is now performed, when necessary (as in cases of fracture), by a very perfect instrument, which cuts through the bone very rapidly, holding and lifting the piece to be removed at the same time, was in those remote times effected by slowly grating away the substance of the skull with a flint scraper, at the cost of intense pain to the suffering infant. Yet these sufferers frequently grew up, as is proved by their remains, and apparently were greatly venerated, perhaps on account of the operation they had successfully endured; for Dr. Broca has also proved that, after death, pieces were cut from the skull thus mutilated, and worn as amulets, probably to ward off epilepsy, which was the disease supposed to be cured by this barbarous operation; and as epilepsy has in all ages been looked upon as brought about by evil spirits, it is regarded as proved that the early people who thus endeavoured to cure this terrible malady had a belief in spirits, and made this hole in the head of an afflicted infant in order that the imprisoned spirit might find a door of escape, and thus, by an easy transition, the amulet taken from the mutilated skull became a charm against evil spirits.

From the instances cited above, it will be evident that the brain-case has been very unceremoniously treated by savages and semi-civilised races both in ancient and modern times, and that the



Artificially deformed skull, from Mallicolo, New Hebrides. Copied from Journal of the Anthropological Institute, vol. xi., p. 78.



Trepanned, or perforated skull; from tomb of Stone Age in France. Copied from Dr. Broca's book, "Sur la Trépannation du Crâne."

results of this treatment do not appear to have been detrimental either to health or intellect; and although the weakly, in all probability, died early, those who survived, inured to pain and endurance from the cradle, grew up hardy and able to bear suffering, which would soon kill our more tenderly-nurtured and abnormally-sensitive children. These curious facts seem to me worthy of more attention than they have hitherto received from medical men and psychologists, and I trust some of the readers of KNOWLEDGE may be induced by this imperfect and too short description to investigate this very curious subject. A. W. BUCKLAND.

INTELLIGENCE IN ANIMALS.

At one time our family rejoiced in the possession of five cats. One, a magnificent black animal, assumed the air and dignity of chief amongst them, and was deferred to on all occasions by the other members of the feline community. One day I detected him in the commission of an outrageous attack on a juvenile member of the fraternity, and at once expressed my disapprobation in a most vigorous manner, chasing the culprit about the room, under chairs and tables, till he suddenly disappeared.

I listened a moment to catch any sound that might betray his whereabouts, and suddenly heard the latch of the kitchen-door fall. I rushed into the kitchen just in time to see Tom slide his forepaw between the door and the jamb, forcing the door open and leaping out into the garden, thence on to the top of a high wall, from which "bad eminence" he regarded me with a placid and unctuous look of injured innocence.

He had opened the door by jumping on to a small shelf near, from whence, by standing up on his hind legs, he could reach the latch and push it up with his forepaw, thus releasing the door, which then swung partially open. The rest, to a cat of "Sweep's" intelligence, was easy. I often afterwards watched him do it. He never succeeded (though he often tried) in opening the door from the outside, because there was nothing sufficiently near the latch on which he could stand while he pressed the thumb-piece of the latch downwards, a proceeding the necessity for which he evidently thoroughly understood, as evidenced by the way in which his attempts to open the door from the outside were made. He would leap up and catch hold of the latch-guard with one paw, while with the other he frantically struck (downwards) at the thumb-piece, continuing his efforts till his strength for the moment failed him, and he dropped to the ground.

He never asked anyone to open the door for him. If he wanted to go out, he opened it and went out; if he wanted to come in, he *tried* to open it, and continued trying (the idea of ultimate failure never, apparently, entering his head) till the noise of his successive failures attracted notice and brought help. JOHN HUMPHREY.

WE had several times been annoyed by joints of meat having been gnawed, and often found on the floor of the cellar; of course, the cat, about three-quarters grown, was rightly blamed as being the delinquent. The maid repeatedly denied having left the cellar door open, but was for some time disbelieved, and I am sorry to say blamed, until one night, going into the kitchen after the family had retired, I found pussy, naught abashed, busily pawing away at the thumb-piece of the latch. I left her for a short time, and on returning found the cellar door open, and pussy busy with the meat. On examination I found the door would immediately swing open on the lever of the latch being pressed. Next day I had a spring put to the latch, and, needless to say, pussy has not troubled since, though it is not for want of trying. She still lets herself into the kitchen from the garden—the outer-door having a similar latch, climbing up the verandah until level with the latch, and pawing away industriously until the door swings open.

W. M.

ANIMAL INSTINCTS.—A lady, daughter of a neighbour of mine, married to a Russian, and who travelled with him and resided some time in Eastern Siberia, told me an anecdote of some swallows, which she said were building their nests under the verandah of their domicile there. One of the nests, when about completed, was found on the return of the builders to be occupied by a sparrow, whom they in vain tried to eject. On finding their efforts fruitless, they started off to the neighbouring river, from whose banks they acquired their plastic material, and in numbers proceeded at once to fill up the hole into the nest. In the evening, Madame S——a's husband, by mounting a ladder, found they had completely filled it up, and he at once, with his fingers, re-opened the hole so as to allow breathing space to the little occupant. Alas! in the morning, when they came to breakfast, he found the hole refilled, and the bird inside quite dead from suffocation.—T. H. MORGAN.

Letters to the Editor.

[*The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with their writers. All communications should be as short as possible, consistently with full and clear statements of the writer's meaning.*]

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*** *All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.*

All Letters or Queries to the Editor which require attention in the current issue of KNOWLEDGE, *should reach the Publishing Office not later than the Saturday preceding the day of publication.*

(I.) Letters to have a chance of appearing must be concise; they must be drawn up in the form adopted for letters here, so that they may go untouched to the printers; private communications, therefore, as well as queries, or replies to queries (intended to appear as such) should be written on separate leaves.

(II.) Queries and replies should be even more concise than letters; and drawn up in the form in which they are here presented, with brackets for number in case of queries, and the proper query number (bracketed) in case of replies.

(III.) Letters, queries, and replies which (either because too long, or unsuitable, or dealing with matters which others have discussed, or for any other reason) cannot find place here, will either be briefly referred to in answers to correspondents, or acknowledged in a column reserved for the purpose.

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

"God's Orthodoxy is Truth."—*Charles Kingsley.*

Our Correspondence Columns.

ERRATUM.—WEATHER FORECASTS.—MESMERISM.—ICE.—SHORTENING OF THE DAY.—FOSSILS IN METEORITES.—THE ATOMIC THEORY.—HISTORY OF NATURAL PHILOSOPHY.—BAROMETRIC OSCILLATIONS.—VEGETARIANISM.—THE POLAR SUN.—LECTURES.

[285]—I must begin by correcting a remarkable compositor's error in the ninth line of the second paragraph of my letter (255) on p. 296, as I there find "2,111,956 miles," where I most certainly wrote 211,956; on the whole, a very decidedly shorter distance.

Either the author of letter 256 (p. 296) must contribute infinitesimally to the taxes, or, like the Scotchman in the parable, he must be "thankfu' for sma' maircies," if he is satisfied with the return which the British nation receives for the annual sum of £15,000 expended on so-called "Meteorology." Were payment to Victoria-street and the Royal Society Committee made by results, I have an abiding conviction that a very considerably less sum would appear in the estimates next April.

I should strongly recommend "A Startled One" (letter 260, p. 301) to obtain a little book by the late Mr. Braid, of Manchester, entitled "Magic, Witchcraft, Animal Magnetism, Hypnotism, and Electro-Biology." It was published by Churchills in 1852, and is now out of print; but I should think that a copy might be obtained through a second-hand bookseller. Your correspondent may also read Carpenter's "Mental Physiology" (H. S. King & Co.) with profit.

Will "An Engineer" (letter 262, p. 301) forgive me for saying that I made no "slip" in the sentence which he quotes. All I meant to imply was that ice did not vary *in the same way* as other solids do with change of temperature—not that it did not so vary at all. I should say just the same thing of bismuth, antimony, and cast-iron.

"A Geologist" (letter 263, p. 301) appears to labour under the impression that the rate of the earth's rotation is dependent in some fashion upon her internal temperature. The most probable efficient cause of the lengthening of the day is, however, the friction of the tidal wave upon the earth's surface, as this must really retard her diurnal rotation on her axis, and produce the effect of a brake. As the Editor points out in his own note, unless we admit that the day is lengthening (at the rate of about ten seconds in 100 years) at least half of the apparent lunar acceleration is unaccounted for.

I have never seen the "Poetry of Astronomy," and so am ignorant of the line of argument pursued therein by its author with reference

to Meteorites (of which Mr. Vignoles speaks in letter 267, p. 302). Some recent pretended discoveries of fossil sponges, corals, and other forms of Zoophytic life in Meteorites have, though, been shown conclusively to be baseless, examinations of the meteorites under the microscope having demonstrated the purely crystalline character of the alleged organic markings. It is, on the whole, just as well to have one's facts right before beginning to theorize.

Probably Wurtz's "Atomic Theory" (Vol. XXX. of the "International Scientific Series") would be the best book for "Ernest L. F." (query 219, p. 303) to obtain; it is the most recent one on the subject.

Mr. Summerson (query 221, p. 303) seems to be unaware that there is an exceedingly great technical difference between "Philosophy" and "Natural philosophy." The latter is only another word for what is now known as "Physics." The former has reference wholly to mental philosophy or metaphysics. With this preliminary warning, I may say that Whewell's "History of the Inductive Sciences" is the most exhaustive work that I am acquainted with on the subject. I believe that Miss Arabella Buckley has comparatively recently written a more compendious book on the History of Physical Science; but I have at this instant forgotten its exact title. Anything she does is sure to be good. There is a History of Science, too, by Mr. Routledge, which I have seen, and which, as far as I could determine from dipping into it, seemed very well done.

I presume that "G. R. W." (in query 227, p. 303), on the subject of Barometric Oscillations, refers to a phenomenon with which I—and I presume many others—have long been familiar. The effect to me of the oscillation of the mercury during a storm is irresistibly suggestive of *breathing*. I have watched it on many occasions, and the rhythmical rise and fall of its surface puts one at once in mind of the measured movement of the chest of a sleeping person. The Editor's explanation of this is obviously the correct one.

Doctress Kingsford (letter 276 p. 322), and some of her *confrères* appear to misinterpret the position which I have assumed towards vegetarianism. I have never denied or disputed, for example, that a severe course of City-feeding might with great advantage be followed by a purely vegetable regimen, until the effects of over-eating had passed away. Nor am I concerned to contest that individuals may be so constituted as to thrive fairly well without eating meat at all. What however I protest, and shall continue to protest against, is the tone assumed by the "whole-hog" vegetarians. "I wish," said an eminent statesman of the late Lord Macaulay, "I wish that I were as cock-sure of anything, as Tom Macaulay is of everything." It is this "cock-sure" demeanour of the phytophagists, this blatant assertion that they *must* be right, and all the rest of the universe wrong, which is as irritating as it is unconvincing. As a class, they are in reality as weak numerically as they are intellectually; but to read their published utterances one would think that in mental capacity as in numbers they infinitely surpassed the remainder of their fellow men (and women). How (letter 277) a total exclusion of flesh, with the substitution of suitable vegetable products would give me "yet better health" than, I am thankful to say, I invariably enjoy, I wholly fail to perceive. Moreover, when I regard the potato-fed Irishman, and see what his diet has brought him to, or study the rice-eating Hindoo, and note his slavishness and utter pusillanimity, I do not derive much practical encouragement to eschew fish and meat henceforth and for ever. One question I should, in conclusion, like to have answered. I perpetually see the names of Dr. B. W. Richardson and Sir Henry Thompson quoted as strong advocates of vegetarianism. Now, my question is this. Does either of these gentlemen restrict himself to vegetable food? *Ars probat artificem.* "The proof of the pudding is in the eating." It is useless to repeat with the clergyman of old the anecdote, "Do as I say, not as I do." If the two eminent men whom I have named do not themselves practise what they preach, the publication of their testimony must, I venture to think, have a precisely opposite tendency to that hoped for, and intended by, those who cite it.

A few elementary considerations will enable R. W. I. (query 236, p. 323), to answer his own questions. Actually at the North Pole the diurnal circle of any heavenly body—assuming such body to remain stationary in the sky—is rigidly parallel to the horizon; the horizon in turn coinciding with the celestial equator ("The Equinoctial" of the maps and globes). Very well, then, neglecting the effect of refraction, it is quite obvious that as long as the sun is south of the equator, or has south declination, he must be invisible from the pole; but that, as soon as his upper limb touches the equator, he will begin to rise. In these latitudes sunrise and sunset are phenomena referable to the axial rotation of the earth, but this quite evidently cannot be the case at the Pole, where, as I have said above, the diurnal circle of a star is parallel to the horizon. The sun, then, at the North Pole will only rise at the same rate as he increases in north declination. Let us take March 21, when he has been invisible there for six months. We find from the *Nautical Almanac*, that between the 20th and 21st the sun is moving northward at a mean rate of 59·23″ per hour. At this date his diameter is 32′ 10·1″, or 1930·1″. If, then, we divide 1930·1″ by 59·23″, we shall obtain 32·5865, the number of hours the sun will occupy in rising. During 24 out of these 32·5865 hours, the earth will have turned once on her axis, so that the rising sun will have travelled through 360° (more nearly 361°.—Ed.) of the horizon. There are, however, yet 8·5865 hours to elapse ere he will be wholly above the horizon; and during this period the arc he describes may be approximately found by the proportion 24 : 8·5865 :: 360° : the arc required (with a slight correction for refraction.—Ed.).

I should think that "X. X." (query 241) would find Mayer and Barnard's little book on "Light," and Mayer on "Sound," both in the *Nature* series, the very things for him. Tomlinson's "Pneumatics," in Weale's series, will furnish him with numerous interesting facts about the atmosphere; and Tyndall's "Lessons in Electricity," published by Longmans, will supply him with all he needs for a lecture or lectures on the subject on which it treats.

A Fellow of the Royal Astronomical Society.

FLEXURE IN PLANES.

[286]—There has been a bad epidemic of flexure among planes lately. From all directions I have heard that their sufferings have been severe; I have also suffered with them. Will you allow me to state through your columns, without encroaching unduly upon your valuable space, that in every case I have traced the flexure to ill-treatment on the part of the possessors of the planes. They have been subjected to torture; they have been firmly wedged into cells too small for them; they have had pieces of card jammed in behind them; they have had screws and clips binding them; but, above and beyond all, they have been cemented on to wooden blocks and metal plates with hard cement, the cooling or setting of which has entirely altered their figure. I wish, then, to take advantage of your kindness to inform all those who are interested in the subject, that planes will not give accurate definition under high powers if they are subject to restraint in any way. A glass plane ½-in. thick, if attached to a block by a wafer, which is allowed to get hard, will show, as a result, flexure, and give a bad definition.

John Browning.

INTERIOR HEAT OF THE EARTH.

[287]—This is a subject in which I have always taken a deep interest, and have followed attentively all that has been published in connection with it for many years. I have, therefore, been much surprised at meeting with no allusion in any of our English scientific works or periodicals to a book published in Germany so long ago as 1875 (I give the title of the book below), and which, I understand, met with considerable support from geologists in that country.*

The author (since dead) was Professor of Chemistry and Physics at Bonn, and professes to found his theories on chemical and physical principles, as ascertained by the latest researches.

He entirely rejects Laplace's "Nebular Theory of the Formation of the Earth," denies on chemical and physical grounds its interior heat, and adduces, among other proofs, the results of a great boring undertaken by the Prussian Government in 1870-71, at a place near Berlin, which was carried to a depth of 4,517 ft., mostly through a continuous stratum of rock salt. The heat at first increased at the rate of 1° for 60 ft., but after reaching the depth of 2,000 ft., this increment gradually diminished, so that instead of being 140° at the bottom of the boring, it was only 113°.

He has a new theory to account for earthquakes and volcanoes, the former, he thinks, being principally caused by the hollowing out of cavities on the superficial strata by the action of water, and the consequent collapse of these cavities.

He denies the igneous origin of what are called eruptive rocks, and, among other proofs, adduces the fact of his having found, on an analysis of a very hard piece of granite, some of the nodules of hornblende, surrounded by pure asphalt, unchanged, which, he observes, would not have remained there if the rock had been subjected to the action of great heat.

The coal measures, he maintains, were formed entirely by marine plants, of which, he says, there were (and now are) immense forests in the sea.

These and many other theories, entirely opposed to the opinions usually held by geologists in general, he maintains with con-

* Geschichte der Erde: ein Lehrbuch der Geologie auf neuer Grundlage. Von Friedrich Mohr, Professor zu Bonn. Verlag von Cohen und Sohn, Bonn, 1875.

siderable show of reason, and adduces some striking facts in support of them.

Being myself a mere sciolist in geology, or, indeed, on any scientific subject, I can offer no opinion as to the validity of his reasoning in general, but I am anxious to know whether any of your readers have met with the work, and how far it is considered worth attention by English scientific men. B.

Edinburgh, Jan., 1882.

HOG PUZZLE.

[288]—Here is a new hog puzzle, by Lieut.-Col. W. H. Oakes, of arithmetical renown. Some of your readers might like to solve it. You shall, next week, have his solution of it, and also one of the original puzzle in short and simple arithmetical form.

Four married couples went to buy hogs. Each individual bought as many hogs as he or she gave shillings for each hog. Also each husband laid out the same number of guineas more than his wife, and this number of guineas was the smallest consistent with the condition that the numbers of hogs purchased by the respective husbands in excess of the numbers purchased by their respective wives form four consecutive terms of an increasing arithmetical series. How much did each husband expend more than his wife? HERBERT REES PHILIPPS.

PLATING—ASTRONOMICAL—CHEMICAL.

[289]—I have to thank "C. T. B" for his reminder. He might try the solution described, for iron, but I am of opinion that he will find the only way for iron to be with a battery and alkaline solution. But, surely, "Watts' Dictionary" should help him through.

On page 211, Professor Young says, in effect, that the sun's pull on the earth could only be sustained, or replaced, by a bar of steel many square miles in section. Sir E. Beckett, in "Astronomy without Mathematics," (page 312) tells us:—"The tractive force on a fast railway train of 400 tons, on the level, is found to be about four tons. . . . . But if the earth were such a train, it would exert a *centrifugal* strain of less than five hundredweight on the rope which held it to the sun." These two views are opposed to one another, and therefore one must be incorrect. [Pardon me; the two views are quite consistent with each other. Sir Edmund Beckett says, if the earth were such a train, that is, if her mass were only 400 tons.—Ed.]

The account of the manufacture of gas from wood, page 246, also requires elucidation. Carbon monoxide is rather "combustible" than supporting combustion. The finished article is said to be free from "dangerous, obnoxious, and otherwise objectionable products." It would be better worth while to caution one against the exceedingly poisonous properties of this gas; besides which, coal gas is harmless. Unlike the case of carbon dioxide (which is *not* poisonous, and which *has* an odour, whatever our text-books may copy one into another), fresh air does not revive one from suffocation by carbon monoxide. This gas, as is well known, burns with a pale-blue flame (the blue flame often seen over a sluggish fire is CO) of feeble luminosity. How, then, can it confer on "an inferior coal gas" a "great candle power?" Lastly, the CO_2 takes up carbon from the heated charcoal according to the equation—

$$CO_2 + C = 2CO,$$

so that eventually the charcoal disappears, except an ashy residue. Where, then, is the danger of "too great an accumulation of charcoal?" and why withdraw from the retort the substance that is also put into it? LEWIS ARUNDEL.

[The account was not quite clearly written. It seemed obvious, however, that F.C.S. referred to wood gas itself, when burning in the usual way, as innocuous, not to carbonic oxide.—Ed.]

ELECTRO-PLATING.

[290]—Letter 119 has evidently escaped the notice of your readers. If I were W. Vaneys, I should throw down the Cu from the solution by means of the battery. Cu in an Ag solution, being thrown down before the Ag. He will understand me, without my taking up any more of your valuable space. F.C.S.

HORSERADISH—INTELLIGENCE OF A CAT—SCENT—"KNOWLEDGE."

[291]—With respect to horseradish, Mr. Heneman must know that it, in common with garlic (*Allium ursinum* and *A. sativum*), onion, leek, eschalot, &c., yields on distillation a fetid-smelling compound oil called allyle, from the genus that it characterises. The astringency of mustard and horseradish (probably also cress, radish, and such like) is due to sulphuret of allyle in combination with cyanogen. I mention the fact that there are other plants having those properties which we value in horseradish to remind Mr. Heneman of the extraordinary proclivity of all nations to use them as condiments. I cannot call to mind a single nation that rejects them; in fact, some people will not be satisfied with anything weaker than *ferula assafœtida*. I do not suppose that horseradish "acts" in any way upon the stomach, although so general a use betokens some effect beneficial, or at least pleasing, to the system. While "warming" the appetite, and, as a condiment, grateful to the taste, it probably has no undesirable effect.

There lives, near where I write, a cat that can without fail open the back-door by springing from the ground to the latch-handle, and, while holding with one paw, can raise the latch with the other, finally swinging the door forward by means of a push with the hind leg. We might almost expect, as a last step to so great reason, a certain amount of culture. But the housewife complains that, having taught itself, to gain its own end, admission, it will not consider further, and close the door again.

"Prester W." himself states about all that is known of the nature of a scent (No. 11, query 170). It is questionable, however, that he is able to recognise a *perceptible* decrease of weight in scent-giving substances. I was under the impression that there was no measurable decrease in weight, but having paid no attention to the subject I await a correction. Dr. Carpenter (in "Comparative Physiology") remarks that "a grain of musk has been kept freely exposed to the air of a room, of which the door and windows were constantly open, for a period of ten years, during all which time the air, though constantly changed, was completely impregnated with the odour of musk; and at the end of that time the particle was not found to have sensibly diminished in weight." Here is an illustration of the extreme minuteness of a molecule!

I express my regret, sir, that you (whose leadership my "set," at least, fully trust) should have been thus far so troubled with *suggestions*. KNOWLEDGE assuredly needs no such small patronising ways, and judging from its rapid spread in this district, it will be the magazine of the future of its sort. CONNERHUGEL.

THE WEATHER OF JANUARY 12-24, 1882.

[292]—Some notes on the weather in the South of Ireland during the past exceptionally mild month may be interesting, as enabling your readers to make comparisons with the weather of more northern and eastern districts. It is not common to observe in winter a high barometer and a high thermometer together, but we have here observed a remarkably high mean of both instruments during a considerable part of the period. During the fortnight Jan. 11-24th, the mean height of the barometer reduced to 32°, and mean sea-level was 30·5 in. The mean of maximum thermometer, was 51·7° of minimum, 43·6°, mean for fortnight, 47·65°. The mean daily temperature was 50° or above, on the 11th, 12th, 13th, 14th, 15th, and 16th, and fell short of it by about half a degree on the 23rd and 24th. The highest temperature in the sun was 80° (*not* by a black bulb in vacuo) on the 24th. The average daily temperature for the above-mentioned fortnight (mean of 50 years at Greenwich), is 36·3°, showing the prodigious excess of 11·35° above the mean this year.

As might be expected, the effect upon vegetation has been striking. On Ross Island, Killarney, horse-chestnut had opened, and several boughs in full leaf were gathered Jan. 22. On that date the following plants had been found in flower, the first-named three or four having been in bloom a fortnight:—

Ulex spinosa	Prunus spinosa (once)
Senecis vulgaris	Nepeta glechonea
Bellis perennis	Crocus (yellow)
Veronica hederofolia	Galanthus nivalis
Capsella bursa-pastoris	Potentilla fragariastrum
Viola tricolor	Lauristinus
Lamium intermedium	Laurel (coming in flower)
Leontodon taraxacum	Primula vulgaris (beginning of month)
Eranthus hyemalis	Poa annua
Petasites vulgaris	Veronica chamædrys
Ranunculus ficaria	Cardamine hirsuta
,, repens (once)	
Coryllus avellana	

No rain has been registered between the 15th and 25th. The mean force of wind has been only 1·7. During the height of the anti-cyclone (when for three days the barometer stood above 30° 7 in.), the sky was thickly covered with stradus cloud. Sunshine accompanied the reduction of pressure. On the 18th the barometer reached 30° 94 in. G. R. WYNNE, F.M.S.

Queries.

257.—Nebulæ. Are nebulæ external to our cluster? If so, what reasons are given for believing them to be? A Derby Student. Nebulæ are proved, I think, by the reasoning of Herbert Spencer and others, to belong to our own stellar system. I believe not a trace of aught external to our cluster has ever been seen with the telescope. But the reasoning is not readily given in a sentence. In my "Universe of Stars," it occupies two or three hundred pages, and requires a number of illustrated maps. Ed.

258. Drying Wild Flowers. I am about to visit Egypt and Palestine, and wish to bring back some specimens of wild flowers. Will you kindly inform me how to dry them, so that they may retain their natural colour and form? H. R. S.

259. Ventriloquism. Could any reader of Knowledge kindly explain to me how ventriloquism is produced? Erin.

260. Lightning. There are two hills with an altitude of about 350 yards, and whose summits are about 3,000 yards apart. On the inner slope of one hill, and about half-way down, three cottages were built, and each of them was destroyed by lightning, separately, and in a period of seven years. An opinion of what is the cause of the lightning concentrating itself and making this particular spot its point of contact in preference to points of higher elevation will greatly oblige. Secretary.

261. Commercial Tables.—As the Education Code does not require illegal or reputed "measures and weights" to be taught in public elementary schools, can you inform me if commercial tables (on cards or otherwise) of such as are only legal, or at any rate practically used in trade, are published, or what is taught in the London School Board Schools?—W. F.

[262]—Old Atlas.—Is the following work rare or valuable? "Atlas Novus sive Tabulæ Geographicæ totius orbis faciem partes Imperia Regna et Provincias exhibentes exactissima cura iuxta recentissimas observationes æri incisæ et venum expositæ à Matthæo Seutter sac: Cæs: Majest: Geogr: Augustæ Vindelicorum." It contains some sixty maps, fifty by Seutter, and about ten by Lotter, which are splendidly printed and coloured, and is of very large size. —S. P. Q. R.

[263]—Vegetable Food.—Will Mrs. Dr. Kingford kindly inform the writer where he will find guiding information to enable him with safety to enter on a course of vegetarianism? He should like for himself to test the truth of her statements.—Provost P.

[264]—Strata.—In travelling from London to Exeter (G.W.R.), what formations are passed through? I noted the following rocks on the way down, but do not know to what age, &c., they belong:—London to Reading, gravel; chalk nearly as far as Goring; then, through miles of grey clay to Didcot; then red clay, light-yellow and grey rock, to Corsham; layers of rock and red earth outside Bristol; limestone beyond Weston Junction; red soil opposite the Wellington monument; and slate at Exeter. Any information will greatly oblige—Carus.

[265]—Pigments.—Wanted a list of colours soluble in alcohol or wood naphtha; especially what blues and blacks are soluble; or name of books giving this information.—Arthur.

[266]—Sulphur Cast.—I should be obliged by information how a sulphur cast (which seems to give more perfect details than one in any other substance) can be made a sufficiently good conductor to electrotype upon. I have tried rubbing it over with plumbago, but it would not take the deposit of copper.—C. J. W.

267.—Thoracic Integrity. I have been taught to regard the chest as an air-tight cavity, any opening into which would cause speedy death. In "Science for All," vol. ii., p. 305, it is related that, through an opening in the chest, the heart has been handled. Have I been misled, or has some unauthenticated statement found its way into the publication named?—C. M.

[268] Photography.—Can any person, through the medium of these columns, give me information concerning photography? 1, where to get the cheapest articles required for photography? 2, how to go about it? and 3, what are the chemicals required?—Anon.

The Magic Wheel.—If those of your readers who possess an induction coil and a small vacuum tube will revolve their magic wheel by the light of their "tube," they will find it has the same effect as the looking-glass, if a certain speed is maintained. The revolutions, of course, should number of breaks at contact breaker + slits in disc.—G. E. V.

Replies to Queries.

195. Quicksilver. Your little note at the end of reply to query 195, p. 324, respecting a dose of small shot, reminds me of a custom here in Lincolnshire, which still prevails amongst the labouring class, of taking a few "shot corns" to cure "the rising of the lights." What this means I have never been able satisfactorily to discover. I once knew a well-to-do tradesman who frequently took a dose.—C. J. C.

210.—The Iliad.—Newman's "Iliad" is much better than dry Bohn's.—Jacteruf.

[228] Microphone. Dry pile not at all suitable. The most simple galvanic pair far better; or a strip of carbon 1 in. × 3 in., and zinc the same size, separated by a pad of blotting-paper moistened with weak sulphuric acid would be strong enough, and would work as long as moist.—G. E. F.

[229]—Hair.—I do not believe there is a single authenticated instance of "a person's hair turning white instantaneously from fear, or other causes." On the other hand, it is well known that a person's hair has become white in a short time, such as a single night. Some years ago it was often stated, and as often contradicted, that the hair of one of our most eminent statesmen was the subject of this remarkable change. This case I can set at rest, for the gentleman to whom I refer told a friend of mine in this city (Manchester) that, when on a visit in Ireland, he went to bed one night with dark hair, and rose next morning with it exceptionally white. It is generally understood that this only takes place when the person is suffering from extreme mental anxiety, intense grief, or bodily suffering; but these causes were excluded from the case of the statesman to whom I refer. The medical man whom he consulted told him there was no cause for alarm, and he thought no more of the matter. A short time ago I heard him address an audience in this city, and I am of opinion that his hair has become a shade or two darker, and less snow-like in colour. I am not aware that the sudden change of colour in human hair has been scientifically explained.—Wm. Horsfall.

[229]—Hair.—The sudden change of the hair from dark to grey which sometimes happens has never been satisfactorily explained. It appears in some instances to be due to the development of air between and among the cells composing the air.—Quain's "Anatomy," vol. ii., page 226., Eighth edition. It is a fair explanation to say that the change is probably due to an impression upon the nerves of the scalp, in common with the rest of the skin, causing a contraction of the capillary blood-vessels, and a consequent with-holding of pigment.—Robert Macpherson.

[230]—Telescope.—The paint used for the inside of the telescope was common, dry, black paint, mixed with water, a quantity of thin flour-paste being added by way of size. Diaphragms are placed in the eye-piece tube; there are none, however, in the principal tube. —A. P. M.

[231]—Chemical Problem.—Let the required equation be

$$aCu + bHNO_3 = xCu(NO_3) + y_2H_2O + zNO.$$

Then the multiples of Cu on the two sides must be equal; *i.e.*,

$$a = x\ ;\ \text{so}\ b = 2y\ ;\ b = 2x + z = 2a + z\ ;\ \text{and}\ 3b = 6x + y + 2 = 6a + \frac{b}{2} + z$$

$$= 6a + \frac{b}{2} + b - 2a = 4a + \frac{3b}{2}\ :\ \text{whence}\ b = \frac{8a}{3}\ ;\ y = \frac{4a}{3}\ ;\ z = \frac{2a}{3}.$$

—T. J. P.

[232]—Chemist.—"W. A. Fyson" should apply to the Registrar of the Pharmaceutical Society, 17, Bloomsbury-square, W.C., for a copy of "Regulations of the Board of Examiners" and "Hints to Students. Both would be sent on application with stamped envelope. The fees are:—Preliminary (as apprentice), £2. 2s.; minor (chemist and druggist), £3. 3s.; major (pharmaceutical chemist), £5. 5s. The two former are compulsory, the latter optional. Certificates of having spent three years with a duly qualified pharmacist, and of attaining the full age of twenty-one years, are demanded before the candidate can enter for the minor. There is at present no compulsory curriculum at a school of pharmacy, although such is the usual course, and costs from £15 to £150. A few, however, with "severe study," pass without such aid. The premiums for apprenticeship vary from nil, in heavy country businesses, to £200 in first-class town pharmacies; £100 being near the average in fair dispensing establishments. Before commencing pharmaceutical studies, the preliminary or classical examination must be passed. After this the spare time of two years may be well spent in studying with "Attfield's Manual of Chemistry"

(price 15s.); then, during the last year, "Bentley's Botany" (14s.). "Pereira: Materia Medica" (25s.), and "Fownes' Chemistry" (18s. 6d.). The least possible expense (exclusive of living and books) is £5s. 5s.; the least time, three years—both expense and time being generally very much exceeded. Capital required for business, from £500 to £2,000. At present the game is not worth the candle, but what the future of pharmacy in this country is, doth not yet appear.—PHARMACIST.

[233]—BIOLOGICAL.—John Hampson will find the relative brain capacity of the Neanderthal skull with man, with "illustrations," also the geological formation in which it was found, in Lyell's "Antiquity of Man." He will also see, by reference to Dr Morton, Professor Huxley, and others, what is, indeed, very easy of demonstration, that the difference between the brain powers of the higher apes and the lowest savage is very much less than the difference between the brain powers of the lowest savage and the cultivated European.—F. SELBY.

[236]—THE POLAR SUN.—Neglecting the ellipticity of the earth, it takes the same time as to decrease in polar distance by its diameter (including +) the effect of refraction, that is

$$90^\circ + \text{ref.} = 90^\circ\ 34'\ 54''$$
$$89^\circ\ 27'\ 50'' + \text{ref.} = 89^\circ\ 56'\ 33''$$

$38'\ 21''$ at $\cdot 99''$ per min. = 2,280 minutes.

and in 2,280′ the sun's movement along the horizon + his own motion (apparent, of course) = say 572°.—TRIA.

[236]—THE POLAR SUN. To an observer at the North Pole, the rational horizon is coincident with the celestial equator; the sun will, therefore, rise above the polar horizon at the same instant as he crosses the equinoctial at the vernal equinox. At this epoch the change of declination (or angular distance from the equinoctial) is at the rate of 59·27″ per hour, and the apparent diameter of the sun's disc at the same epoch is 32′ 11″; hence the time occupied by the rising of the sun's disc is found by division to be 32·57 hours, or 1,954 minutes, and the arc of the horizon moved over during this time will be at the rate of 360 degrees per 24 hours, which is 488½ degrees. A more rigorous method of arriving at the above result (which is only approximate) is to calculate the exact instant of Greenwich time when the sun's *upper* limb is on the horizon, and then to repeat the calculation for the *lower* limb on the horizon. The difference of the two times is the exact interval required. Nothing, however, is to be gained by entering more minutely into such purely technical calculations. The above result is probably correct to a few seconds of time.—A. N. SOMERSCALES.

[247]—WARMTH AT NIGHT.—Unless J. M. J. proposes to confine himself entirely to the house, and unless he keeps his sitting-room at the same temperature as his bedroom, a fire all night would certainly be injurious. The advice, however, that can be given in this column will not be of much use to him. He should consult his medical adviser, who can make himself conversant with every symptom. Advice is best, as a matter of course, when it is based upon accurate knowledge.—ROBERT MACPHERSON.

[248]—LEPIDODENDRON.—To prevent the decay of fossil shells, &c.:—Steep them in a weak solution of gum-arabic for several days. All fossils taken from an exposed sea cliff of loose soil—as, for instance, the drift shells of Blackpool—must be soaked in fresh water for two months and then treated as above. The metallic nodules you mention are concretions of iron-pyrites, the so-called "Thunderbolts" of the Isle of Wight. No doubt the apparent casts of vegetable remains are due to the crystallization of this substance. It is almost useless to carry home any fossils, however fine, which contain a trace of this substance, for, though they may be as hard as steel when first obtained, they will, sooner or later, come to grief. If the "brick earth" of West Drayton be glacial clay, the chances of finding any fossils are very remote. To decide this question, look for ice marked boulders, such as that figured and described in KNOWLEDGE, p. 345. Be particularly careful to keep all fossils in a perfectly dry place; I have seen many valuable specimens completely spoiled by the neglect of this precaution.—J. H.

GOATS TO PROTECT SHEEP.—The farmers of Hunterdon and Somerset counties, New Jersey, use goats to protect their sheep from dogs. Two goats can drive away a dozen dogs, and two are about all each farmer puts in with his sheep. As soon as a dog enters the field at night, the goats attack him, and their butting propensities are too much for the canine, who soon finds himself rolling over and over. A few repetitions of this treatment causes the dog to quit the field, limping and yelling. Formerly, when a dog entered a sheep-field at night, the sheep would run wildly around and cry piteously. Since the goats have been used to guard them, they form in line behind the goats and seem to enjoy the fun. The idea of utilising goats in this way came from the West, where they are put in sheep-pens to drive away wolves.—*N. Y. Sun.*

Answers to Correspondents.

⁂ *All communications for the Editor requiring early attention should reach the Office on or before the Saturday preceding the current issue of* KNOWLEDGE, *the increasing circulation of which compels us to go to press early in the week.*

HINTS TO CORRESPONDENTS.—1. *No questions asking for scientific information can be answered through the post.* 2. *Letters sent to the Editor for correspondents cannot be forwarded; nor can the names or addresses of correspondents be given in answer to private inquiries.* 3. *No queries or replies savouring of the nature of advertisements can be inserted.* 4. *Letters, queries, and replies are inserted, unless contrary to Rule 3, free of charge.* 5. *Correspondents should write on one side only of the paper, and put drawings on a separate leaf.* 6. *Each letter, query, or reply should have a title, and in replying to letters or queries, reference should be made to the number of letter or query, the page on which it appears, and its title.*

A. ARTHUR READE.—I should be glad to give my experience of the effects of smoking, but I smoke so little that I may practically be said to have no experience in the matter. I never smoke unless the presence of a great number of smokers renders it unpleasant not to. As for alcohol, when I work hard, which is most of the time, I find the less alcohol I take the greater my working energies. For eighteen months (some years ago) I took no alcohol, and my work never went more easily. At present, even when I work hardest, I do not go down to absolute abstinence: still, my working allowance is too small to make much difference one way or the other. W. J. COLLINS. We quoted Dr. Carpenter's address because of its intrinsic merits, not as part of a discussion. For the discussion of a medical question you should go to medical journals. J. A. MILES. Many thanks for the table; but fear there is no space. The algebraist can get any of the tabulated relations at once when he wants them, others would not look at the table. Will try to find room for problems 1 and 2, as they are general.—J. OFFORD. Many thanks. There may be delay in appearance of your interesting communications; but they will appear. The paper you write on quite suitable for printers.—H. P. COOPER. Thanks; will try to find space for new form of electrical accumulator.—G. E. SUTCLIFFE. Reference to the key, after sufficient attempts to solve problem, perhaps the best course; you should notice how the difficulties which had foiled you are mastered. The best promise of proficiency in your letter is your evident anxiety to become proficient. With a little practice you will probably succeed.—WM. DAVEY wants to know how he may soften animal hair without destroying it or injuring its colour; he would have been among the Queries if he had put his question in proper query form.—T. J. P. Thanks; but why not follow rule and put title of query? When we get such a reply as yours, we have to hunt up the query through back numbers to get the proper heading, and our work is heavy without this. It would be easy for you, with the query before you, to write its title.—J. McGRIGOR ALLAN. No space for articles or any but very concise letters about mutilation of animals. Section V. on other subject later if can find room.—S. S. S. S. wants best book of Mechanical Philosophy for C.S. examination. Both the French writers you name good popularisers of science, but without mathematical knowledge, so that they are only to be trusted when quoting the opinions of others. Lardner's "Museum" partly obsolete, but in part still trustworthy. Impossible to answer more definitely in space at our command. Magnetism cannot be interrupted, as electricity, by non-conducting bodies. We use your words, but they are inappropriate.—W. P. WANTAGE. Already answered.—W. H. SAND. A. R. MOLLISON. Answered.—NAMELESS. 49, Victoria-road. Solution correct.—R. G., J. N. P., A LEARNER, PHRENSY, EQUES, ROB. E. ALISON, B. G. MORRIS, and others. Dear sirs, how can we find time to work out such sums as you send? If we could hire a calculating boy, who could tell us in 2 sec. how long a wire $\frac{1}{50}$th of an inch in diameter could be made out of a cubic foot of brass, or in 3 sec. how often a cart-wheel 3 feet in diameter would turn if rolled a distance equal to the sun's from the earth, &c., we would tell you all about these things. *One* of you has taken an appropriate name.—CHARLES BURNS, noting that the Goat and Compasses = God encompasseth us, desires to have corresponding equations for the Pig and Whistle, the Magpie and Stump, and the Bear and Ragged Staff.—THOS. SIDDALL. Will insert queries if you will make them suitable.—B. M., F.R.C.S. Thanks, suggestions noted.—A. M. SOMERSCALES. Thanks, will wait till C. T. B. explains what was really meant. Of course, oblique is less than full illumination. In case of still water, the brightness you mention is due to reflection, not to surface illumination. But this was not C. T. B.'s difficulty.—A. J. P. Know of only one kind of aneroid, in which the box is completely exhausted of air, the elasticity of covering balancing the atmospheric pressure.—A. ORMSBY. Would give your problem (Napoleon's) for solution: but it is necessary to reserve mathematical column for other matter. You will find no difficulty in solving it if you go to work as follows:—Pay no

attention to the equilateral triangle, which lead you away from true solution. Note only that the centres of these are centres of arcs on the sides [illegible] containing [each an angle of 60 degrees. Through A [illegible] can be drawn, to meet arcs on AB, AC, line PAQ parallel to line joining centres of these arcs. PQ is the maximum [illegible] which can be so drawn. Let PB and QC produced meet in R. PQR is the maximum equilateral triangle which can be described about ABC. Hence PR and QR are parallel to lines joining centres of the other pairs of arcs (for otherwise a larger [illegible] could be drawn, and one side of this would be [illegible] than PQ, which is impossible). Hence the triangle [illegible] joining centres of these circular arcs has its sides parallel to [illegible] of triangle PQR, and is therefore equilateral. —Oswald [illegible]. Come in early with definitions, and [illegible]. I quite agree with you that at the beginning [illegible] sound definitions are very important. Yes, Professors King and Rowney were the opponents of Eozoon Canadense. 'Twas they of whom the poet (that good fellow [illegible]) wrote in 1869:

> And through the lists a cry has flown
> A daring challenge for a fight—
> "Eozoon, be it known,
> Has structure like the nummulite;"
> And on brave Dawson's gauntlet warm
> Is dimly traced in mystic line,
> "Of fibre of film asbestiform,
> And chambers all acervuline."
> But back! a loud, defiant shout
> Resounds from Connaught's distant strand:
> The King of Galway has set out,
> All mail'd in ophite, pen in hand;
> And to the field he hies him straight,
> With gentle Rowney, knight renown'd,
> Whose prowess none will underrate
> On Chemistry's broad fighting-ground, &c.

M. M. The size of the moon's image formed at focus of object-glass having 42 inches focal length, will be about 37·75 inches, as you can readily prove by directing the telescope to moon, removing eye-piece (and, if necessary, eye-tube also), and receiving the image on a piece of card or paper. But the magnifying power depends on the eye-piece, and you cannot say the image will have an apparent diameter of so many *inches*, but subtending such and such an angle. With a power of 100, for example, the moon's apparent diameter would subtend an angle of about 3,100′, or nearly 52°.—Elon requires information as to management of gold fish in glass globe, average duration of their lives, &c. Captive balloons (this refers to a different subject, let me explain) might certainly be sent up with minimum thermometers, to determine temperature of upper regions of air. Simplex. Quite so: but now if you would compare some of the symbols. Nearly every stenographer knows Pitman's; I use it myself, though not for reporting. Could you not give us ocular demonstration of the superiority of Bell's system. Sir T. W. Letter sent at once to publishers. C. Lloyd E. Pardon me, but I was thinking of another correspondent. Your evidence about the poker is balanced by an equally wide experience the other way. But why should I be prejudiced against the poker-across-fire theory? If a theory so remarkable could be established by the rough kind of evidence you adduce, it would be a delightful subject for Knowledge. As it is, it only illustrates what men *can* believe, and Herbert Spencer has already used it in that way.—R. C. Your difficulty is a natural one. Yet notice that every part of an ellipse is concave towards centre, or towards either focus, despite the increase and diminution of mere distance. If you draw the moon's path to scale, you will see that it is concave towards the sun all the time. Describe two concentric circles rather less than a quarter of an inch apart (·24 inch), and having radii of 92 and 92¼ inches respectively, and divide their circuit into twenty-five equal parts. Then the moon's path would be represented by a curve passing from the outermost to the innermost, and then to the outermost again, and so on, the successive contacts occurring at the successive divisions along the outer and inner circles alternately.—Tita. Will try to find space shortly both for letter and extract; meantime, let me note that you seem to me to be quite right. There seems to be as much scientific accuracy in the account as in the blind man's statement that red was like the sound of a trumpet, or in the association most of us have had as children (and, perhaps, still have) between the days of the week and particular colours. For instance, with me Sunday is yellow, Monday rich red, Tuesday olive green, Wednesday bright green, Thursday dark grey, Friday orange, and Saturday light grey. The months have also their colours. J. Rae. If you had chanced to read my accounts of Arctic travel, you would know I did not need to be informed of what you kindly tell me. It will be time enough when the Pole is reached to learn how long the explorers will have to stay there. You see, I give you another opportunity for contradiction, which seems your strong suit; but you should try to make out what you are contradicting.—B. We have not used an instrument having a vernier divided as you describe; but it seems obvious that it would serve to divide to one-half the arc to which the older form of vernier will divide. Thus, when you have the vernier divided into 60ths of 59′, you take the division nearest to one on the limb, and so get the reading true to a second of arc. Now with the vernier divided into 60ths of 119′, you can either take the division nearest to one on the limb, or noting that two divisions on the vernier are appreciably equidistant from the divisions (alternate) on the limb, you regard the bisection of the space between those two divisions on the vernier as coincident with the division midway between the two on the limb. You read this just as easily as you would if the bisection were marked on the vernier. Hence it is as though the vernier were divided into 120ths of 119′, enabling you to read to half seconds of arc, instead of to seconds only.—Erratum. No truth in the report, but thanks.—A Subscriber. Odds in favour, as you say. It seemed too obvious to need correcting. East and west in star maps right. You look down at earth, up at sky. Hence the difference. Evidence about pink tinge of ashes, in fire gone out in sun would be interesting. Why? oh, why will not "A Subscriber" follow rule, and give number and page of letters?—Farmer Will. Your queries will not go under any heading we can invent. If you do not think them worth heading, can you think them worth inserting? You say, in the midst of your queries, that if the doctrine of evolution is true, the doctrine of fate is also true. What is the doctrine of fate? Why not say, as you may with equal reason, that supposing trees and animals really grow, and are not changed from state to state by special acts of creation or change, the doctrine of fate must be accepted?—T. A. "I do not know how it is, but at present I like my hypothesis the best, it seems to my poor judgment the nearest the truth." Most of us feel that way. But, as you very truly say, Who can thoroughly understand these things? If we had not so much to insert about things we *can* to some degree understand, we might take up these inconceivables.—Hafed was dazed three days "by Magnetism? Astroism? Divine Effluence?" We do not know: but infer, it was on one of those days that he sent us advertisement of "The Magnetic Pilgrim" who "tarries top of Bunyan Street."—John T. Page. The practice of collecting autographs is not childish, far from it: nor is a toothache a joy for ever, very much the reverse.—Hon. Sec., Civil and M. Engineer's Society. Mr. Love's paper was marked for insertion, but crowded out by press of matter requiring more immediate attention,—and more concisely written.—J. Kirkman, M.A. Questions now answered. But would it not have been an inaccuracy to have, in first act of Harold, a reference to "Arcturus dancing so brightly, almost through the nucleus of Donati's comet in 1858?" not that Arcturus danced, or seemed to dance, at that time—to my eyes any way.—T. J. H. Matter too complex to be adequately treated in short notes, and no room for long ones.—Hygeia. Fear the ill-effects of tight-lacing and high heels are as well known to the tight-lacers and high-heelers as to the rest of the world. Articles on the subject would be thrown away on those, and are not needed by these.—Alpha. There is no reason for supposing that there has been, within the last few centuries, any perceptible change in our northern climate; but, if there had been, the displacement of the Pole Star by precession has nothing to do with it. The inclination of the earth's axis to the plane of her path does not vary in any such way as to affect climate.—Anti-Kreophagist. I do not think any flesh-eaters so ignorant as not to know of the wide range of food materials open to vegetarians. I am not myself a vegetarian, but at a time (three years ago) when I did a great deal of mental and bodily work (rowing every morning two hours at my hardest, and often in heavy rain) I used frequently, for several days in succession, to take no food but fruits and vegetables—not on principle, but from sheer carelessness: and I cannot say I ever felt the least failing of strength. I never had better health. My tastes, however, are of the carnivorous kind, or rather, they are for our customary mixed diet. An Abernethy biscuit and a bunch of grapes will serve me for a dinner, on occasion, very well; but I do not "hanker" for such food. I mention this, not as of any interest in itself, but to show that if fruit and vegetables agreed with me so well, it was not because I liked them (so that the evidence in their favour is so much the stronger).—A. H. E. Nos. 2 and 3 are not out of print. They might be picked up, perhaps, after a little inquiry; but I know of no place where they could be obtained. A few copies remain of Part I., in which are these numbers, and a limited number have been kept for our volumes. It would be useless to reprint them, as many others of the earlier numbers are nearly sold out too; so that if reprints were ordered at all, at least eight numbers would have to be re-

printed, which, of course, cannot be thought of.—C. C. C. The theory you advance was originally suggested by one Isaac Newton; but calculation has since shown that the phenomena of comets' tails are irreconcilable therewith. We shall come again to comets by and by, if correspondents will give us leave.—A. J. MARTIN and C. J. C. It would indeed be interesting to astronomers if the sun's elevation varied in the way described by "A Resident in Glengloy, Upper Lochaber, N.B." But too good a watch is kept on the sun at Greenwich, Paris, Washington, &c., for him to play such tricks without our knowing it. *If* the sun really shone over a hill this year, at a time of year when formerly he did not top the hill, the hill has changed, or the level of the ground where the house stood from which the observation was made. — R. H. Thanks. Solutions neat, but no space. G. S. E. How if he declines to be "relegated?" —HY. CRAGG—C. Sorry, but in fairness to other squarers must regard all magic squares as now done with.—MICROCKITH. There may be some slight difference in the friction, but otherwise can be none.—W. G. PARRISS. When a gas-flame is blown, the already ignited gas is driven away from that which in the ordinary course would be ignited next: thus this gas remains unignited, and the other part burns out. The wind or breath does not cause combustion to cease; it prevents combustion. Combustion in a steadily-burning flame is beginning and ceasing all the time.—W. G. WOOLCOMBE. I *cannot* reconcile observed facts with the postulated law, "a force acting in a direction at right angles to direction of motion of a body has no effect in altering the direction of motion." That law will be postulated a long while before it is established. Write "no effect in altering the velocity of motion," and you have less trouble. "That's how the error has arisen." Your geological difficulty suggests that those subjects do not greatly attract you; but with determination it will vanish—EXPERTO CREDE. Do not know of any one who sells very extensive series of rocks; but think a letter addressed to Prof. Tennant, of King's College, would bring you information on the subject.—J. W. C. Loomis's book gives all necessary information for projecting an eclipse from the data in *Nautical Almanack*. Johnson's does not. Believe Bogue publisher of latter work.—W. N. W. says second volume of Allen's work on "Commercial Organic Analysis" is now issued; price, 10s. 6d.—EDM. HUNT. We are content to wait. Do not think the construction we gave can possibly be misunderstood.—ZARES. Galileo, no; Newton did, though. You confound inertia and momentum. Every force, however small, affects the inertia, but only an equal momentum can match the momentum of a moving mass. I not only "seem to imagine," I *know* that I am dealing with a well-known and well-understood subject. You might with advantage study some good text-book of dynamics; or, for the history of the matter, look up Whewell's "History of the Inductive Sciences," Vol. II., pp. 45-52. Thanks for hints how to manage correspondence, but we see the matter from another point of view.—J. H. M. Such mesmeric experiments would do something to establish the claims of phrenology, if one were sure the subject knew nothing about the bumps. Your "young man of the name of Walker" (H.?) may have known a good deal.—C. T. W. The "equation of time" varies slightly from year to year, as the position of the earth's perihelion changes. But very slightly. The equation of your old dial would, if exact, indicate roughly the time when dial was made.—W. C. My "sub" and I have quite enough to do without the classification you suggest. Now, about your question: I did not mean to snub you, as you say I did: when you asked how the star-maps were to be used in the dark, it seemed an all-sufficient reply to say they were not meant to be so used.—W. F. DENNING. I also should have preferred publishing your letter. But it could not be, with space at command.—J. HARBORD. I cannot, at this moment, recall any earlier references to Mephistopheles in English literature than those in Marlowe's "Faustus."

Letters Received.

Edina, M. K. Fothergill, J. Harloch, G. E. V., Marplot, Pertinent (read it Pertneus), Carus, W. C., G. C. E., J. Harvey, X—Z, Weary, K. Hardy, M. Brant, X. L., Jansen, A. Martins, Halloween, Cardinal Point, J. Easterbrook, L. Murrill, J. S. T., Curious, J. Fordinham, M., N. Eastman, T. Elliott, J. Pearsall, Manchester, M. Pewtress, A Woman, Philaletkes, M. B. Q., R. B., Jas. Atherill, F. Brown, J. T. S-m, Post-Prandial (so we should imagine), C. Carteret, S. Y. Ellis, Porter, A Constant Reader, R. S. S., M. Peewitt, Jas. C. Christie (try Cockle's).

Special Notice.

EXCHANGE COLUMN. In No. 18 we shall open an Exchange Column, similar to that which has for several years formed a feature in our excellent contemporary, the *English Mechanic*. The charge for Exchange notices will be 3d. for the first twenty-four words, and 3d. for every succeeding eight words.

SIXPENNY SALE COLUMN. We shall also open a Sixpenny Sale Column, in which advertisements will be inserted at the rate of 6d. for the first sixteen words, and 6d. for every succeeding eight words.

ENLARGED NUMBERS.—We propose, from henceforth, to extend KNOWLEDGE to 32 pp. twice, at least, in each month; and we hope that our growing circulation will enable us to enlarge KNOWLEDGE permanently to 32 pp. weekly. If all our readers would help in extending our circulation, which some (whom we hereby warmly thank) have done so effectually, we should very soon adopt a weekly 32 pp. number.

Notes on Art and Science.

STOVE HEAT.—Those of your readers who have seen Mr. Williams's article on the "Air of Stove-heated Rooms," and who find stove-heating both comfortable and convenient, may be glad to know that the "Crown Jewel Base Burning" Stove does not diffuse its heat "through red-hot iron;" it cannot injure, but tends greatly to promote health. I have used one for years.—R. F.

INHALING SULPHURETTED HYDROGEN.—I was surprised to read in No. 14, page 293, the remarks of Mr. Mattieu Williams on the inhalation of sulphuretted hydrogen. I was rather careless about the inhalation of this gas until, after suffering six successive and unaccountable attacks of illness, I traced them to this cause. The attacks consisted of violent pains in the stomach, and in each case they occurred about three days after the inhalation of the gas. I may mention that, finally, in order to make quite sure in the matter, I purposely inhaled a small quantity of the gas, with the result of a slight attack of the pains after the usual interval. It appears to me that it is a gas which produces different effects on various people. In Roscoe and Schorlemmer's new work it is described as "a powerful poison, producing insensibility and asphyxia." This may be true, but it is not the effect it had on me—nor, it appears, on Mr. Williams.—H_2S.

SCIENCE TEACHING.—At the annual general meeting of the Teachers' Training and Registration Society, and of the Bishopsgate Training College, the other day, Prof. Goldwin Smith took laudable advantage of the opportunity to impress on those present what science teaching really means. "In respect of the teaching of science," he said, "he had constantly brought before him the wide gulf fixed between the two different kinds of what persons call knowledge. The one was a mere learning to repeat a verbal proposition, and the other was knowing the subject at first hand—a knowledge based upon a knowledge of the facts. That which they had constantly to contend against in the teaching of science in this country was that teachers had no conception of that distinction, for they thought it quite sufficient to be able to repeat a number of scientific propositions and to get their pupils to repeat them as accurately as they themselves did. If he might offer one suggestion to the governing body of the college, it was that so far as they taught science at all they should aim at giving real and practical scientific instruction; that it should be confined to those things about which there was no dispute; and that the teacher should be instructed that his business in teaching was to convey clear and vivid impressions of the body of facts upon which the conclusions drawn from those facts were based."

BITING.—A Serpent, if surprised suddenly, or brought to bay at close quarters, may be too terror-stricken to attempt flight; then it *bites*, following a curious general rule which seems to obtain throughout nearly the whole animal world, from a passionate child downward, no matter what the natural weapons of offence may be. Young *Felidæ* will keep their talons sheathed until they have exerted all possible force with their soft milk-teeth, and a lizard will seize the hand which restrains it with its insignificant little jaws, when its tail or claws might inflict far more injury. The *Boidæ* never use their constrictive powers in self-defence (unless they are gripped), and it seems probable that if a venomous snake's fangs lay in its tail, it would use its teeth *first* when attacked before bringing them into play. Indeed, it must be remembered that very few animals are provided with exclusively defensive weapons, and that the python's enormous strength in constriction, the viper's poison apparatus, the lion's teeth and claws, and the electric discharge of the gymnotus are given them primarily for the purpose of securing their food.—Arthur Stradling, in *Nature*.

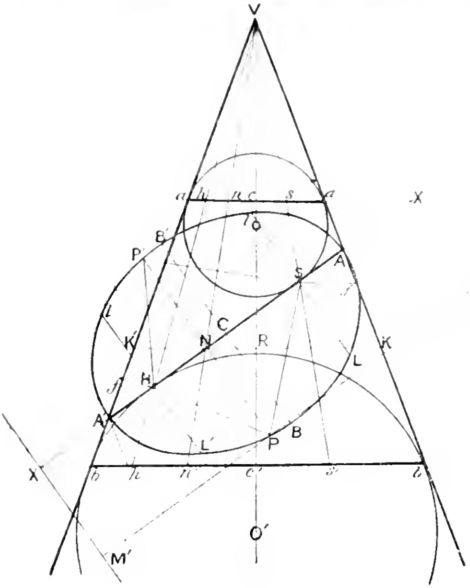

Fig. 1.

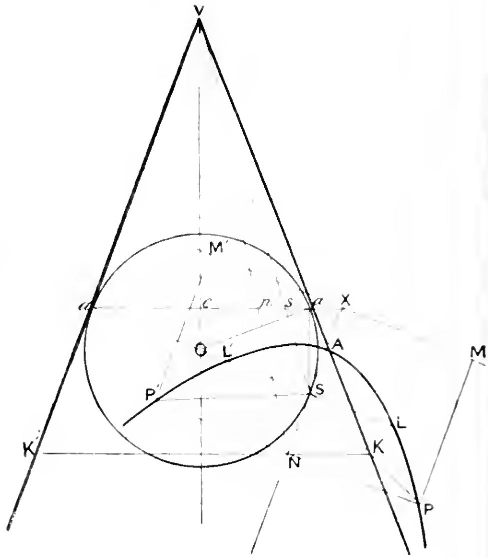

Fig. 2.

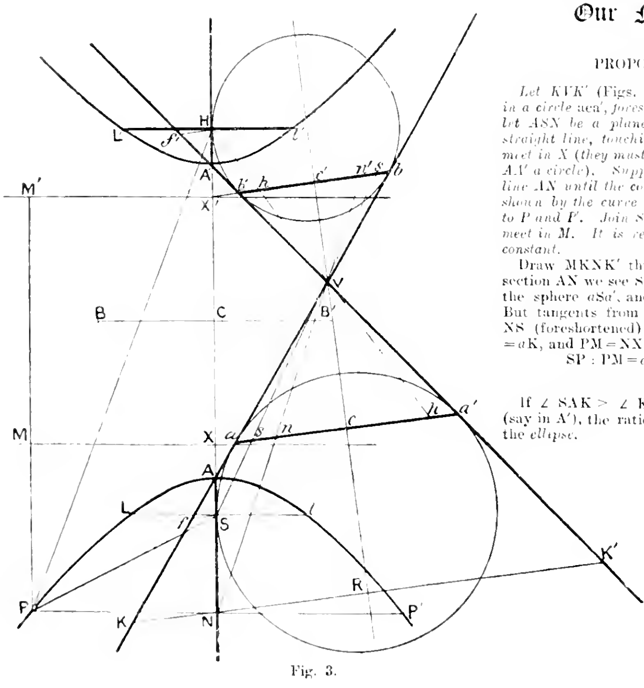

Fig. 3.

Our Mathematical Column.

PROPOSITION IN CONIC SECTIONS.

Let KVK' (Figs. 1, 2, 3) *be a cone, touched by the sphere* aSa' *in a circle* aca', *foreshortened into a straight line in the fig.; and let ASN be a plane section of the cone, also foreshortened into a straight line, touching the sphere in S. Let* a'a *and NA produced meet in X (they must meet unless AN is parallel to* aa' *or the section AA' a circle). Suppose the section AN rotated around the straight line AN until the conic section occupies the plane of the paper, as shown by the curve PAP', the points which were at N being brought to P and P'. Join SP, draw PM parallel to NX, XM perp. to NX, to meet in M. It is required to show that the ratio of SP to PM is constant.*

Draw MKNK' through N parallel to $a'a$. Now, in the conic section AN we see SP (foreshortened) as NS, a tangent from N to the sphere aSa', and Nn is another tangent to the same sphere. But tangents from the same point to a sphere are equal. Hence NS (foreshortened) or SP $= Nn$ (foreshortened), which obviously $= a$K, and PM $=$ NX. Hence

SP : PM $= a$K : NX $= a$A : AX, a constant ratio. Q.E.D.

If $\angle$ SAK $>$ $\angle$ KVK', fig. 1, so that AN produced cuts VK' (say in A'), the ratio aA : AX is less than unity, and the section is the *ellipse*.

If $\angle$ SAK $=$ $\angle$ KVK', fig. 2, so that AN is parallel to VK', the ratio aA : AX is unity, and the section is the *parabola*.

If $\angle$ SAK $<$ $\angle$ KVK', fig. 3, so that NA produced cuts K'V produced (say in A'), the ratio aA : AX is greater than unity, and the curve is the *hyperbola*.

In the cases of the ellipse and hyperbola, we can take another sphere bHb' touching the cone circularly and the plane of section in H. For the ellipse, the second sphere touches the cone on the same side as the other sphere, and the plane of section on the other

side; for the hyperbola, the second sphere touches the cone on the other side and the plane of section on the same side.

The reader will find no difficulty in extending the proof to the lines HP, PM', drawn from a point P on the curve to the other focus H and perpendicular to the other directrix X'M. The construction is given for each case.

Note also that the relations $HP + SP = AA'$ for the ellipse, and $HP - SP = AA'$ for the hyperbola follow at once.

For, rotating the curves back to the foreshortened view, we have

SP foreshortened into tang. XS = tang. $Xa = aK$

HP foreshortened into tang. XH = tang. $Xa' = bK$

Wherefore $HP + SP = ab$ in case of ellipse (Fig. 1)

and $HP - SP = ab$ in case of hyperbola (Fig. 3).

—EDITOR.

The geometrical student will find a good deal more that is worth studying in the relations here indicated. We have added several lines (latus rectum, minor axis, &c.) to the figures for this purpose.

MATHEMATICAL QUERIES.

[37]—Given any two lines meeting in a point, and some point out of the lines; required to draw from this point to the str. lines two equal str. lines which include a given angle.—AMICUS MATHEMATICÆ.

[38]—ROLLING DISC.—Given the radius, weight, velocity, and angle of inclination sideways from the vertical of a circular disc rolling freely on a level place; find the radius of its track.—F. W. F.

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[31]—

$$x^4 + 4x^3 = 27$$

$$\text{put } x = 3y, \text{ then } 3y^4 + 4y^3 = 1$$

$$\text{and } y = \frac{1}{z}, \text{ then } 3 + 4z = z^4$$

Make a perfect square on each side—

$$(a) \quad z^4 + 2pz^2 + p^2 = 2pz^2 + 4z + p^2 + 3$$

The right hand side will be a perfect square if—

$$4 = 2p(p^2 + 3)$$

$$i.e., \text{ if } p^3 + 3p = 2$$

$$\text{put } p = v - \frac{1}{v}, \text{ then } p^3 = v^3 - \frac{1}{v^3} - 3p$$

$$\therefore v^3 - \frac{1}{v^3} = 2$$

$$v^6 - 2v^3 = 1$$

$$\therefore v^3 = 1 \pm \sqrt{2}$$

$$\text{taking the upper sign } v^3 = \sqrt{2} + 1$$

$$\text{and } \frac{1}{v^3} = \sqrt{2} - 1$$

$$\text{and } p^3 = \sqrt[3]{\sqrt{2} + 1} - \sqrt[3]{\sqrt{2} - 1}$$

Let p have this value, then, from equation (a)—

$$z^2 + p = \pm\sqrt{2p}\left(z + \frac{1}{p}\right)$$

A quadratic with two roots corresponding to each sign, thus giving four values for z, and, therefore, four values for x ($x = \frac{3}{z}$, as might have been anticipated.—W. G.

[EQUATION, p. 328, No. 15].—"W. B." points out that in our solution of equations

$$x^2 = \frac{39}{y} - \frac{14}{x} \text{ and } y^2 = \frac{42}{x} - \frac{13}{y}$$

after getting $(x + y)^3 = 216$ by addition, we might have got by subtraction $(x - y)^3 = 8$. It is obviously the simpler course.—ED.

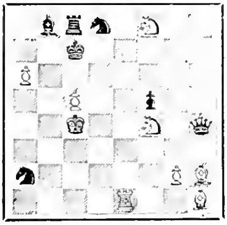
22b.—"Yarletonian," F. J. Butt, and others solve this equation; it needs only transposing, squaring, and simplifying, then squaring again and simplifying.

[Mr. McGowan's solution to 25, p. 307, to hand, correcting obvious blunders (20 for 120, and £$\frac{6}{5}$ for £$\frac{7}{6}$). It shall appear in our next.—ED.]

MESSRS. J. & A. CHURCHILL have recently published two interesting tables; one, showing the average weights of the human body and brain, and of several of the internal organs at eighteen periods of life in both sexes; the other showing the same (at decennial periods of life) in the insane, the forms of insanity being specified.

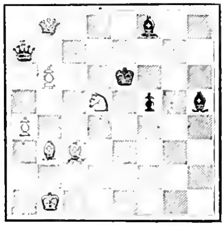
Our Chess Column.

No. 21.
By I. G.
BLACK.



WHITE.
White to play and mate in two moves.

No. 22.
By W. Thurman.
BLACK.



WHITE.
White to play and mate in two moves.

SOLUTIONS.

PROBLEM No. 14, p. 282.

1. B. to R.4.	1. K. takes Kt.
2. B. to Kt.3. mate.	

If P. to K. 5, then Q. takes P. mate. If P. to Q.3; Q.Kt. to B.7. mate. If P. to B.3.' K.Kt. to B.7. mate. Finally, if P. to B.4. then Q. to Kt.8. mate.

PROBLEM No. 15, p. 282.

1. Q. to Q.6.	1. K. to K.6.
2. R. to Kt.3.ch.	2. B. takes R. mate.

If K. to B.6. then R. to B.3.ch.

PROBLEM No. 16, p. 308.

1. Kt. takes P.	1. K. to K.5, or a, b, c.
2. Q. to K.6.ch.	2. K. to Q.6, or B.6.

Kt. mates accordingly either on Q.Kt.4. or K.R.4.

(a) If 1. K. to Kt.7., 2. Q. to K.2.ch., 2. K. to R.6., 3. Kt. to B.4. mate, or 2. K. to Kt.8., 3. Q. to B.2. mate.

(b) If 1. K. to Kt.5., 2. Q. to Kt.6.ch., 2. K. to B.6., 3. Kt. to Q.4. mate, or 2. K. to R.6., 3. Q. to Kt.3. mate.

(c) If 1. B. to Kt.7., 2. Q. to Q.B.1. anything, 3. Kt. to R.4. mate.

ANSWERS TO CORRESPONDENTS.

*** *Please address Chess-Editor.*

Edward Sargent.—Nos. 18 and 19. If 1. Q. takes B.P., then 1. Q. to Q.R.sq.

R. S. Standen.—Solution of No. 15 correct.

C. H. F.—Solution of No. 17 correct.

H. A. L. S.—Solution of No. 17 correct.

J. P.—Remove Pawn on Black Q.R.2.

F. H. Jones.—Solutions correct.

Received offers to play by correspondence from—

M. J. Harding	A. C. Skinner
H. C. Angell	J. N. Sieklebotham
E. A. Dillon	Edw. P. Westlake
F. H. Jones	D. Cudmore.

We have paired them in the order named above.

It is necessary the first players should play White. Two games may be carried on simultaneously, each player having the move. Answers should be sent next day after receipt of move, at latest. To avoid mistakes, the last move should always be repeated. For example:—12. $\frac{\text{P. to B 3.}}{\text{Q. to R.3.}}$ 13. $\frac{\text{P. to. K.5.}}{}$ In case of any misunderstanding arising, players may refer to us.

A SOCIETY to be called the North Middlesex Natural History Association has recently been established. Address, 26, Ingleby-road, Grove-road, Holloway, N. Its objects are the formation of a Natural History Museum and Library of reference and circulation; also the diffusion of natural history knowledge by means of lectures, papers, &c., and (in the summer) field excursions.

Our Whist Column.

By "Five of Clubs."

Dear "Five,"—The enclosed letter from our esteemed Whist correspondent, "Mogul," just received. After what I have told you about demands on space, you will see that it is quite impossible to find room for it (it would take more than a column). But it is not quite fair to answer a letter which has not appeared. I should be glad if you could write a short note, putting things right, without unfairness to our correspondent. Yours faithfully,

Editor.

Dear Editor,—(1.) "Mogul" is right in regarding as "proven" my approval of Z's play from strength in the first round of trumps (game No. 13). There was little chance of Y utilising the information, but it was better, I think, to give it. I hasten to correct an impression, which he thinks readers might entertain, that dealer should *always* give information in this way. (2.) "Mogul" is in doubt how far I think the principle of playing a straightforward game should be carried, saying that apparently I admit of no exception but in the "closing rounds." That was rather an illustration than an exception. I may say that I do not go farther than that great whist-master, Clay; scarce so far. Clay's abhorrence of false cards was, I think, *tant soit peu* exaggerated. Clay, Cavendish, and Pole are all pretty much at one, however. (3.) When I speak of modern scientific whist, I do not mean that the older method was unscientific, but (as the grammar of the phrase implies) I distinguish the modern from the older scientific games. Rules have come into vogue now which were not formerly adopted; and whether they are good or bad, running counter to them means more than declining to give partner information; it means deceiving him. (4.) As to the game in No. 13, I gave it as actually played, faults included. "Mogul," in his published letter, pointed out objections to Y's play, and I showed *then*, what seemed to me unnecessary before, how Y could tell that Z (i) must have five Clubs, and (ii) must know that he, Y, had four. "The length of my explanation is," Mogul says, "its condemnation." I am disposed to agree with him. All that is there explained at length should have been obvious; but "Mogul" did not seem to find it so. I agree with "Mogul" that Z should have led the Queen instead of the King; (as you know, I am not in this harder on Z than Z would think fair). 5. With all that "Mogul" says in his concluding paragraph I thoroughly agree, except, of course, in his persistent mistake that I am of a contrary opinion. If you will allow me I will quote what I take to be most excellent in this part of "Mogul's" letter. [We prefer not; "Mogul" would probably not think it fair to quote the excellent rules he gives without those passages in which he implies that "Five of Clubs" teaches the opposite; for when these passages are omitted, "Mogul" seems to teach precisely what "Five of Clubs" has been teaching.—Ed.] There is only one small addition I would make to his rule, "When strong, tell your partner; when weak, don't tell your adversaries," viz., this, even when weak, do not unnecessarily *deceive* your partner,—Yours truly,

Five of Clubs.

Play Second Hand.

There are few points which distinguish more thoroughly the good from the inferior whist player, than the play second hand. We are not, of course, referring to players so inexperienced as to know no other rule than "second hand play low." Nor are the rules for play second hand, at least in the opening rounds of a game, less definite than those for leading. But somehow it happens that many players who very seldom lead unwisely, who know well when to play highest and when to finesse third in hand, and when it is essential to success to win partner's trick fourth hand, are apt to trust, second hand, to chances which are demonstrably against them. Given, for instance, an original lead of a small card (plain suit), second player with Queen and a small one (and no special reason for risking something to get a lead), how often do we see the Queen played, though it is known that, in the greater number of cases, the card is thus thrown away. Of course, the play often steals a trick. Perhaps in five cases out of eleven it may do so, but it is bad, because in a greater number of cases it fails; and in every case it suggests for a while to partner that you hold either the Queen alone, or King, Queen, and a small one. So in other cases which might be cited.

The rules for play second hand are in reality sufficiently simple, though here, as in the case of the lead, they seem multitudinous.

We note, first, that in general a low card is to be played second hand; for, in the first place, the suit is presumably your adversaries', and it is well to keep the commanding cards of their suit; and, in the second place, your partner lies at an advantage over third player, who ordinarily must play his highest, lest the trick should fall an easy prey to your partner. By playing high second hand you waste a good card, whether third hand takes the trick or your partner; you are rather worse off, too, if, though you take the trick, partner could have won it had you left it to him; for when the suit is returned, the lead will be through your partner's strength to your hand, weakened by the loss of its best card in the suit. You only gain it at so chance that neither third hand nor your partner has a better card; and it is unwise to play for only one among several chances.

Yet, still supposing the suit your adversaries', and that you have originally not more than three cards, it may still happen that a high card should be played. Thus, if you have Ace, King, one or more small ones; King, Queen, and one or more small ones; Queen, Knave, and *one* small one; Knave, ten, and *one* small one; or ten, nine, and *one* small one; play the lowest of the sequence. In the first case, you win the trick and still have the commanding card of the suit; in the others, if you do not win the trick, you avoid the risk of its being taken with a low card by third in hand, or your partner compelled to play a very high card.

Again, if a high card is led, and you hold a higher card and one or two small ones, it is generally best to cover. If third in hand take the trick, two good cards have fallen from the enemy to make one trick.

We leave to another occasion, however, the discussion of the play second hand in detail. We shall endeavour (though the task is not so easy as in the case of the lead) to reduce the play to system, instead of presenting some forty or fifty rules, as has usually been done.

We propose next week to give a game made up from the account given in Clay's "Short Whist," (see p. 176) of a case in which evil results followed from unwise persistence in forward play.

Maxim.—The best whist-player is he who plays the game in the simplest and most intelligible way. *Clay.*

G. Thompson.—Whist problem correctly solved in your second letter. *B* played according to custom in such cases. Cavendish (p. 50 of latest edition) and others touch on the question whether it is well to lead from a long suit headed by Ace, Queen, and agree that it pays better in the long run to do so than to wait for the chance of being led up to. There is a good chance either of drawing the King, or, if second player has it, of partner winning the first trick. By waiting in such a case, you deceive your partner as to the chief constituent of your hand.

Problem I.—"Mogul" and "Vaenol" point out that this problem from the "Westminster Papers" is unsound, as *Z* may have Heart King. The objection seems valid, as certainly with King, Knave, ten, three, second hand, ten would be the right card to play. As third player holds only one Heart, the nine, there is a somewhat greater probability that *Z* is strong in the suit, than that he is weak, although it is *A*'s long suit. The writer in the "Westminster Papers" may have had some reason for considering that *Z* cannot hold the King; but we can detect none, certainly none which would occur to *B* in ordinary play. "Mogul" adds that *Y* might have been signalling for trumps holding Knave, two, for aught *B* can know. This, however, appears to us incorrect. Playing Knave second hand from Knave, two, would not be signalling for trumps, but an attempt to take the trick. Clay, in his chapter on the signal, discusses this point, and, as it seems to us, his opinion that there would be no signal is correct.—Five of Clubs.

NOTICES.

The Publishers beg to announce that in future Monthly Parts of Knowledge will be issued. The following can now be had:—

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PLAINLY WORDED-EXACTLY DESCRIBED

LONDON: FRIDAY, MARCH 3, 1882.

CONTENTS OF No. 18.

A STUDY IN MINUTE LIFE.

BY HENRY J. SLACK, F.G.S., F.R.M.S.

No. I.

THE interest to be derived from the use of the microscope soon passes away unless it is accompanied with some scientific thinking about what is seen. With the help of such thinking, there is no end to the combinations of amusement and instruction that was previously gained. Suppose, for example, one of the oldest experiments is made, that of putting a little wisp of hay into a vessel of water, and noticing what happens. In a tolerably warm place, many hours will not pass before a sort of skin forms on the surface of the water, and after this soon appear a swarm of moving creatures. M. Pouchet called this skin a proliferous pellicle, and ascribed to it a kind of maternal power in generating the animalcules that come after it. This notion he connected with a theory of creation in which at some imaginary periods of great cataclysms and catastrophes, prodigious putrefactions and decompositions occurred, and out of the seething mass arose monstrous forms.

Gorgons, hydras, and chimeras dire.

The fact, however, is, that the quantity of the decomposing material has no direct action upon the kind of life that appears, and that a great many creatures neither want putrefaction or its products, but are injured by the process. The chemical changes that occur in fermentations, like that of sugar into alcohol and carbonic acid, or in decompositions that by no means charm the nose with pleasant smells, are caused or promoted by living organisms: in the case of yeast-cells, resembling little bladders, and in other cases, minute rods or wriggling spirals. It is not, however, with these micro-ferments that we are now concerned, but with higher forms that are sure to be found injurious, but which can be well developed in water that contains their appropriate nutriment, but remains sweet.

It is best, when varieties of minute life are required, to operate on a tolerably large scale, say a good handful of hay in a gallon of water, but a pinch of it in a tumbler is sure to yield a good harvest, and germs of various organisms are so widely and commonly distributed that success may be obtained with a grain of chopped hay in a quarter of an ounce of water. In such small experiments a two drachm vial answers very well. In a shallow vessel the water dries up too quickly. The student need not at first trouble himself with the particle or with any object not big enough to be easily seen with an inch power, or at most a half-inch. An accurate stock of general ideas and broad principles should be acquired before attention is directed to the most minute structures. Before taking much trouble to identify the various objects and learn their names, it will be well to notice certain peculiarities of structure. A crowd of little restless creatures is sure to be seen in the infusion mentioned, and their movements are produced by hair-like projections, called cilia, no other external instrument being visible. Their runnings to and fro are incessant. Whenever they are looked at, by day or night, their activity is striking, and until they become weak or dying their pace is maintained. While the cilia are in quick movement, it is impossible to see exactly what they are and what they do. If, however, a good-sized specimen of any ciliated object is allowed to get nearly dry, and consequently enfeebled, the motion of the organ can be plainly discerned. For our purpose a little drop of water containing some of the creatures should be placed on a glass slide, covered with thin glass, and the object watched under the microscope until, from vigorous movements while there is plenty of water, they grow languid and slow as evaporation from the edges of the covering glass lessens the supply. A cilian is then observed to move much in the way we can imitate with an elastic stick about a yard long, and having an impulse given to it by sharp turns of the wrist. There is a wave motion from the bottom to the top of the cilian like that which can be made to agitate the flexible stick. As cilia are usually very numerous and close together in the sort of objects under our notice, it is evident that if they did not move rhythmically and in a thoroughly orderly way, they would be in frequent collision. This would be very awkward if the purpose to be served were only, as with many species, the production of water currents to bring them food, and it would be quite inconsistent with the rowing action required for locomotion. A common species is very conspicuous in the *Paramecium aurelia*, an oblong little creature ranging in size from a little more or less than 1-100th of an inch in length. It possesses longitudinal rows of cilia; Ehrenberg found some fine specimens with as many as fifty-two of these rows, each containing from sixty to seventy cilia, making 3,640 in in all. Each cilian rises from a minute knob, which, though not containing positive muscular structure, acts like a nicely-arranged combination of muscles. There is again no nerve cell, but the whole group of organs is made to work harmoniously by some undiscoverable, but no doubt most methodically-arranged, groups of molecules, which receive impressions from their surroundings, and stimulate the contractions and expansions by which the movements are produced. If we were furnished with three or four thousand limbs whose use and movements had to be divided by our intelligence and our will, we might be a long time learning how to get on without grievous mistakes. In the case of the infusoria, we cannot imagine anything like human volition or purpose, but one as high in the rank of life as a Paramecium has to use its army of external organs for these purposes, to produce currents of water so that fresh streams continually reach its surface and provide amply for its respiration, so that the minute objects that serve for its food may come within reach of its mouth and be swallowed, and that the cilia particularly engaged

in moving it about shall not be hindered by other cilia pulling in wrong directions.

The tendency of matter in motion to become rhythmical is well known to all physicists. Air readily vibrates in musical pulsations; the waterfall makes its musical chord, and the pendulums of adjacent clocks are said to conform their beats. All such considerations are very interesting, and they enable us to find resemblances of analysis between the action of our ciliated infusoria and a host of other rhythmical and orderly processes. Our explanations, however, soon come to an end, and wonder engulphs us where actual knowledge fails.

In another paper we will endeavour to obtain some more ideas of natural history and physiology from our hay infusion. Now, we will only add that cilia are found in all the vertebrate animals, and in most of the invertebrates, though not in crabs, spiders, and insects. Man has them in many places springing from epithelium cells, in such living membranes as those of the larynx, trachea, and bronchial tubes. They do much in their situations to keep back dirt particles, but their success is far from perfect. Town air, especially foggy days, makes the human being too much of a dirt bin to be consistent with health.

THE CRYSTAL PALACE ELECTRICAL EXHIBITION.

Fourth Notice.

Very great progress has been made during the past week in all departments, more especially in that of electric lighting. Siemens, Swan, Hawkes, Gerard, Jablochkoff, &c., are all at work.

The Palace has never before looked so attractive, and even the gallery, with its quota of light, is very pleasant, both to the visitor and the exhibitor. One of the collections which visitors should not fail to inspect is that in the north nave, exhibited by the War Office. The most attractive feature of the exhibits is the destructive apparatus, torpedoes, &c., technically termed "mines," a name which is perhaps more innocent or less startling to tender nerves, than that by which they are more generally known.

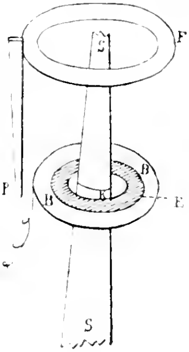


Fig. 1.

They are exhibited in various forms and sizes, but, of course, none of them are charged. One, made to contain 100 lb. of gun-cotton, is suspended from the roof, and represents the manner in which it would be moored so as to float near the surface of the water. Mines, however, are generally of a larger size, and are placed on the river-bed or sea bottom, where the depth does not exceed 60 feet. In deeper water they are suspended by a buoy (containing the circuit-closing apparatus, to be described further on) at a depth of 50 feet. Some of the specimens exhibited are constructed to hold a charge of 500 lb. of gun-cotton, which, on exploding, automatically or otherwise, at the above-mentioned depth, sends into the air a column of water 80 feet in diameter and 150 feet high.

Fig. 1 illustrates what is known as the "circuit-closing apparatus." S S′ is a steel rod rigidly fixed at the bottom, and weighted at the thin upper end, which is free. A small collar of brass (K) is attached to the rod at about the middle of its length. Round K is a ring of ebonite (E), to insulate another brass ring (B) from the rest of the apparatus. The brass ring (F) is a portion of the framework, and is metallically connected to the flat brass spring P. The wire from one end of the battery (about four Leclanche cells) on shore is connected to the brass ring B. When a vessel strikes the buoy or mine, as the case may be, the steel rod oscillates sufficiently to make contact between the ring B and the spring P. The current will then pass from B into P, and thence through the framework—the electrical circuit being completed through the water and earth. So far, however, no more damage is done than to ring a bell on shore. Fig. 2 shows how this is done. MM′ are extensions (known as pole pieces) of the soft iron core of an electro-magnet. The current which is produced by the impact of a vessel on the buoy passes through the electro-magnet, and in consequence M and M′ attract the piece of soft iron, AA′, which is pivotted at C, so that both M and M′ tend to draw AA in the same direction. D is a pin on AA′, RR′ is a lever, with the hammer, H, at one end, and pivoted at C′, so that when AA′ is drawn away, D allows H to fall, and strike the bell, L. This gives notice to the officer of the approach of a vessel, which he may then blow up. The blowing-up, however, is very easily made automatic. NN′ (fig. 2) is a piece of brass, with a slit wide enough to allow the hammer-lever in falling to enter and make contact at G. This completes the electrical circuit for firing the mine. Of course, all this is but the work of an instant.

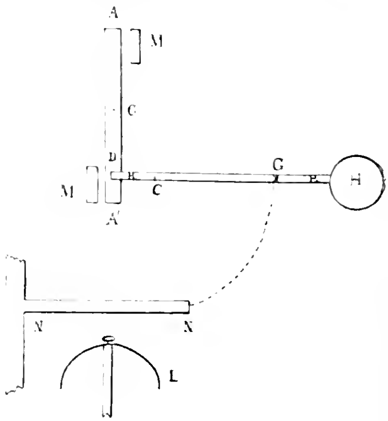


Fig. 2.

The fuze is represented in fig. 3. B is the beechwood cup, and W W are the extremities of a piece of fine

platinum wire stretched across the cup. When the firing circuit is completed, the current enters by means of a wire joined to W, and, passing this, the platinum leaves by another wire joined to W′. Fifty to sixty Leclanché cells are used, and make the platinum red-hot, thereby igniting a small quantity of gun-cotton wrapped round it, the flame from which, consuming a thin paper cover P P′, enters the

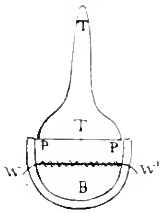


Fig. 3.

detonating tube T T, containing fulminating mercury. The explosion of this is followed by the explosion of the mine. When the mine is fired by hand, a key, somewhat similar to a Morse telegraph key, is used. Normally there is a piece of ebonite switched into the space between the contact points, so that the mine may not be accidentally fired. The whole collection displays the state of perfection to which we have attained in the art of wholesale homicide.

"Field Telegraph Equipment," "Signalling Apparatus," and "R.E. Field Company Equipment," are all well represented, but our space will not permit a description this week. We can only say that everything gives one an impression of extreme compactness and efficiency, reflecting great credit on the War Office.

A new feature in the Exhibition is the delivery of a course of lectures by Professor Sylvanus Thompson. In the first he essayed to demonstrate what electricity is, and, after describing its production and effects, he said that the late Professor Clerk Maxwell regarded electricity as the motion of the ether of space, and that it was most probably in that direction we should have to look to discover the nature of electricity.

VENTILATION BY OPEN FIRE-PLACES.

By W. Mattieu Williams.

THE most stubborn of all errors are those which have been acquired by a sort of inheritance, which have passed dogmatically from father to son, or, still worse, from mother to daughter. They may become superstitions without having any theological character. The idea that the weather changes with the moon, that wind "keeps off the rain," are physical superstitions in all cases where they are blindly accepted and promulgated without any examination of evidence.

The idea that our open fireplaces are necessary for ventilation is one of these physical superstitions, which is producing an incalculable amount of physical mischief throughout Britain. A little rational reflection on the natural and necessary movements of our household atmospheres demonstrates at once that this dogma is not only baseless, but actually expresses the opposite of the truth. I think I shall be able to show in what follows that, 1st, they do no useful ventilation; and, 2nd, that they render systematic and really effective ventilation practically impossible.

Everybody knows that when air is heated it expands largely, becomes lighter, bulk for bulk, than other air of lower temperature; and, therefore, if two portions of air of unequal temperatures and free to move are in contact with each other, the colder will flow under the warmer, and push it upwards. This latter postulate must be kept distinctly in view, for the rising of warm air is too commonly regarded as due to some direct uprising activity or skyward affinity of its own, instead of being understood as an indirect result of gravitation. It is the downfalling of the cooler air that causes the uprising of the warmer.

Now, let us see what, in accordance with the above-stated simple laws, must happen in an ordinary English apartment that is supplied, as usual, with one or more windows more or less leaky, and one or more doors in like condition, and a hole in the wall in which coal is burning in an iron cage immediately beneath a shaft that rises to the top of the house, the fire-hole itself having an extreme height of only 24 to 30 inches above the floor, all the chimney above this height being entirely closed. (I find by measurement that 24 inches is the usual height of the upper edge of the chimney opening of an ordinary "register" stove. Old farm-house fire-places are open to the mantel-piece.)

Now, what happens when a heap of coal is burning in this hole? Some of the heat—from 10 to 20 per cent., according to the construction of the grate—is radiated into the room, the rest is conveyed by an ascending current of air up the chimney. As this ascending current is rendered visible by the smoke entangled with it, no further demonstration of its existence is needed.

But how is it pushed up the chimney? Evidently by cooler air, that flows into the room from somewhere, and which cooler air must get under it in order to lift it. In ordinary rooms this supply of air is entirely dependent upon their defective construction—bad joinery; it enters only by the crevices surrounding the ill-fitting windows and doors, no specially-designed opening being made for it. Usually the chief inlet is the space under the door, through which pours a rivulet of cold air, that spreads out as a lake upon the floor. This may easily be proved by holding a lighted taper in front of the bottom door-chink when the windows and other door—if any—are closed, and the fire is burning briskly. At the same time more cold air is poured in at the top and the side spaces of the door and through the window chinks. The proportion of air entering by these depends upon the capacity of the bottom door-chink. If this is large enough it will do nearly all the work, otherwise every other possible leakage, including the key-hole, contributes.

But what is the path of the air which enters by these higher level openings? The answer to this is supplied at once by the fact that such air being colder than that of the room, it must fall immediately it enters. The rivulet under the door is thus supplemented by cascades pouring down from the top and sides of the door and the top and sides of the windows, all being tributaries to the lake of cold air covering the floor. The next question to be considered is, what is the depth of this lake? In this, as in every other such accumulation of either air or water, the level of the upper surface of the lake is determined by that of its outlet. The outlet in this case is the chimney hole, through which all the overflow pours upwards; and therefore, the surface of the flowing stratum of cold air corresponds with upper part of the chimney hole, or of the register, where register stoves are used.

Below this level there is abundant ventilation, above it

there is none. The cat that sits on the hearth rug has an abundant supply of fresh air, and if we had tracheal breathing apertures all down the sides of our bodies, as caterpillars have, those on our lower extremities might enjoy the ventilation. If we squatted on the ground like savages something might be said of the fire-hole ventilator. But as we are addicted to sitting on chairs that raise our breathing apparatus considerably above the level of the top of the register, the maximum efficiency of the flow of cold air in the lake below is expressed by the prevalence of chilblains and rheumatism.*

The atmosphere in which our heads are immersed is practically stagnant; the radiations from the fire, plus the animal heat from our bodies, just warm it sufficiently to enable the cool entering air to push it upwards above the chimney outlet and the surface of the lower moving stratum, and to keep it there in a condition of stagnation.

If anybody doubts the correctness of this description, he has only to sit in an ordinary English room where a good fire is burning—the doors and windows closed, as usual—and then to blow a cloud by means of pipe, cigar, or by burning brown paper or otherwise, when the movements below and the stagnation above, which I have described, will be rendered visible. If there is nobody moving about to stir the air, and the experiment is fairly made, the level of the cool lake below will be distinctly shown by the clearing away of the smoke up to the level of the top of the register opening, towards which it may be seen to sweep. Above this, the smoke-wreaths will remain merely waving about, with slight movements due to the small inequalities of temperatures caused by the fraction of heat radiated into the room from the front of the fire. These movements are chiefly developed near the door and windows, where the above-mentioned cascades are falling, and against the walls and furniture where feeble convection currents are rising, due to the radiant heat absorbed by their surfaces. The stagnation is the most complete about the middle of the room where there is the greatest bulk of vacant air space.

When the inlet under the door is of considerable dimensions, there may be some escape of warmer upper air at the top of the windows, if their fitting is correspondingly defective. These, however, are mere accidents: they are not a part of the vaunted chimney-hole ventilation, but interferences with it.

There is another experiment that illustrates the absence of ventilation in such rooms where gas is burning. It is that of suspending a canary in a cage near the roof. But this is cruel; it kills the bird. It would be a more satisfactory experiment to substitute for the canary-bird any wingless biped who, after reading the above, still maintains that our fire-holes are effective ventilators.

Not only are the fire-holes worthless and mischievous ventilators themselves, but they render efficient ventilation by other means practically impossible. The "Arnott's ventilator" that we sometimes see applied to the upper part of chimneys is marred in its action by the greedy "draught" below.

The tall chimney-shaft with a fire burning immediately below it dominates all the atmospheric movement in the house, unless another and more powerful upcast-shaft be somewhere else in communication with the apartments. But in this case the original or ordinary chimney would be converted into a downcast shaft pouring air downwards into the room, instead of carrying it away upwards. I need not describe the sort of ventilation thus obtainable while the fire is burning and smoking.

Effective sanitary ventilation should supply gentle and uniformly diffused currents of air of moderate and equal temperature throughout the house. We talk a great deal about the climate here and the climate there, and when we grow old and can afford it we move to Bournemouth, Torquay, Mentone, Nice, Algiers, &c., for better climates, forgetting all the while that the climate in which we practically live is not that out of doors, but the indoor climate of our dwellings, the which, in a properly-constructed house, may be regulated to correspond to that of any latitude we may choose. I maintain that the very first step towards the best approximation to this which is attainable in our existing houses, is to brick up, cement up, or otherwise completely stop up, all our existing fire-holes and abolish all our existing fires.

But what next? The reply to this will demand the whole of another short essay.

* Since the above was written, a correspondent in Paris tells me that a caricature exists, representing a Frenchman enjoying an open fire by standing on his head in the middle of the room.

THE ELECTRIC TELEGRAPH.

By W. Lynd.

THE WIRES AND INSULATORS.

THE manner in which a telegraph line is carried from station to station must be familiar to all. The conducting wires are passed through a bath of liquid zinc, by which process they receive a coat of that metal. Zinc being easily oxidised, is by the action of the atmosphere converted into oxide of zinc, and protects the iron from the influence of moisture. The wires are suspended by earthenware or glass supports called insulators, fixed upon wooden posts, at intervals of about sixty yards. As electricity has always a tendency to pass by the shortest route possible to the earth, it will easily be understood that if the wires were allowed to rest upon the wooden poles, the current would make its escape before it reached the receiving station. Wood, when seasoned with tar, is a very poor insulator. There must be some good non-conducting substance between the wire and the post. There are many forms of insulators. Glass offers the greatest resistance to an electric current of any known material, but electricians object to it on account of its hygroscopic properties—a film of moisture collects upon glass in nearly all states of the weather. On some lines ebonite is used, but there are objections even to that excellent non-conducting substance; rain wets it easily, and its surface soon becomes dirty and spongy. Brown earthenware insulators are the most common in this country; the glaze does not crack, and although they have not so great a resistance as glass, they are, on account of their cheapness and durability, used in preference to any other kind of non-conducting supports. In spite of all precautions, however, electricity finds a means of escape from the best insulated lines. In wet weather the leakage is sometimes so great that the signals on the recording instruments at receiving stations are almost unintelligible. The steel indicator of a single needle telegraph apparatus should strike against two ivory pivots on the dial plate, in order that the beats may be distinctly understood, but I have had to read off messages in bad weather when the current was so weak that the needle did not touch the pivots at all, and half the letters had to be guessed. In rainy weather, coats of moisture collect upon the wire, insulator, and post, and as a natural result the electricity flows to earth. If there is only a little loss at each post, the current on a long line is

soon weakened, and it is not unusual in stormy weather to put on additional battery power to compensate for the loss.

The electric conductivity of moist air has been a subject of dispute; some electricians have held that humid air acts as a conductor of electricity; and others have maintained that it does not. Recent experiments of M. Marangoni support the latter theory very decidedly, for he finds that a Leyden jar, heated so as to prevent condensation of moisture on its glass walls, and thus arrest surface conduction, gives a long spark as in the driest air. When, however, the precaution of heating the walls of the jar is not taken, the moisture condenses on the latter, and, forming a thin film of water, causes a silent discharge, which might be mistaken for a slow discharge through the conducting air. It follows from these experiments that the loss of electricity on telegraph lines is wholly due to surface conduction over the wet and dirty insulators, or leakage along entangled threads and branches of trees, at particular points, and not to a general discharge into the saturated air.

Lightning, or atmospheric electricity, is occasionally attracted to the wires, and passes along them, disturbing the indications of the instruments. Telegraphists are always exposed to danger during a thunderstorm. In the summer of 1859, a youth employed in the telegraph department of one of our principal railways was seriously injured by the physiological effects of an electric shock, received while he was sending a message on the double-needle instrument. A storm was raging at the time, and the clerk did not take the precaution to avoid touching the metallic parts of the instrument. At that period, the many ingenious contrivances for diverting the lightning now in use had not been thought of. I have known the fine silk-covered wires used in certain parts of the needle telegraphs fused into an amorphous mass. When the currents of atmospheric electricity were not so intense, they usually demagnetised the needles, causing them to deflect the reverse way.

The Aurora Borealis or Northern Lights will sometimes cause a disturbance in telegraphic communication. The long lines and cables are more liable to be affected by the Aurora. There was a disturbance of this kind from Aug. 11 to 14, 1880. According to the report of Privy Councillor Ludewig, of the Central Telegraph Department, Berlin, it seems to have manifested itself throughout the greater portion of the northern section of the Eastern hemisphere, sending off, however, a southerly stream in the direction of Mozambique, which reached to Natal. It does not appear that the western continent was affected. The general features of the disturbance consisted of manifestations of the presence of strange currents (earth currents as they are called) of fluctuating intensity, the durations and fluctuations varying in different localities, and the direction of the currents changing frequently. This last feature would seem to indicate a movement of revolution, or at least of approach and recess, with regard to some line or point. These disturbances were traceable alike in underground lines and in lines carried through the open air. In Germany all the longer lines were very much disturbed, in the ways mentioned, on Aug. 12, from noon till late at night. The interruptions frequently involved the omission of several signs, or even words, and in the Morse apparatus, continuous strokes appeared on the paper.

There are other causes of disturbance to which telegraph lines are liable. Kite-tails entangled in the wires will, if a shower of rain comes on, cause contact: the electricity will flow from one wire to the other, so that a message, say, from London to Birmingham, may pass through three or four different circuits, and give rise to provoking confusion and delay. I have seen the dead body of a large bird lying across the wires of a railway telegraph line.

Apropos of the feathered tribe, some interesting facts were recently brought out in a paper by M. C. Nielsen, of Christiania, on the impression produced upon animals by the resonance of the vibration of telegraph wires. It is found that the black-and-green woodpeckers, for example, which hunt for insects in the bark and in the heart of decaying trees, often peck inside the circular hole made transversely through telegraph posts, generally near the top. The phenomenon is attributed to the resonance produced in the post by the vibration of the wire, which the bird mistakes as the results of the operations of worms and insects in the interior of the post. Everyone knows the fondness of bears for honey. It has been noticed that in mountainous districts they seem to mistake the vibratory sound of the telegraph wires for the grateful humming of bees, and, rushing to the posts, look about for the hives. Not finding it on the post, they scatter the stones at its base, which help to support it, and, disappointed in their search, give the post a parting pat with their paw, thus showing their determination, at least, to kill any bees that may be about. Indisputable traces of bears about prostrate posts and scattered stones prove that this really happens. With regard to wolves, again, M. Nielsen states that when a vote was asked for the first great telegraph lines, a member of the Storthing said that, although his district had no direct interest in the line proposed, he would give his vote in its favour, because he knew the lines would drive the wolves from the districts through which they passed. It is well known that to keep off the ravages of hungry wolves, in winter, the farmers of Norway set up poles connected together by a line or rope, under which the wolves would not dare to pass. "And it is a fact," M. Nielsen states, "that when, twenty or more years ago, telegraph lines were carried over the mountains and along the valleys, the wolves totally disappeared, and a specimen is now a rarity."

Water-carrying Tortoises.—At a recent meeting of the San Francisco Academy of Sciences a fine specimen of the desert land tortoise, captured at Cajon Pass, San Bernardino County, was shown, and Professor E. T. Cox related some curious circumstances in connection with it. This tortoise, which is as large as a good-sized bucket, is a native of the arid regions of California and Arizona. On one being dissected, it was found that it carried on each side a membrane, attached to the inner portion of the shell, in which was about a pint of clear water, the whole amount being about a quart. Professor Cox was of opinion that the water was derived from the secretions of the giant barrel cactus, on which the tortoise feeds. This cactus contains a great deal of water. The tortoise is found in sections of the country where there is no water, and where there is no vegetation but the cactus. A traveller suffering from thirst could, in an emergency, supply himself with water by killing a tortoise. They are highly prized by Mexicans, who make from them a delicious soup. They are oftentimes attacked by foes both for their water and also for their flesh. They are overcome by the foxes, and killed by being dragged for miles over the country at a pretty rapid pace. Mr. Redding afterwards stated that he was on the Gallapagos Islands in 1849, when he assisted in capturing 92 land tortoises, varying in weight from 150 lb. to 600 lb. each. These they brought to San Francisco, where they sold them for more money than the whole of the ship's cargo of lumber made. They were two months on board, yet they neither ate nor drank anything, though food and water were offered them. When killed, however, considerable quantities of water were found in each of them. They lived on the high lava rocks of the islands, where there are no springs or streams, and the only dependence of animal life for water is necessarily upon the irregular and uncertain rain showers. These were of a different species from the one shown. It was generally admitted that it would be useful if the habits and peculiarities of these animals could be noted, and some trustworthy information as to how they collect and secrete their water obtained. —*Times*.

NIGHTS WITH A THREE-INCH TELESCOPE.

By "A Fellow of the Royal Astronomical Society."

OUR first object to-night shall be that beautiful and familiar double-star α Geminorum, or Castor (Map, p. 298). This, with the instrument we are employing, we shall find to be a perfectly easy object; in fact, were the young observer furnished with the means of accurately directing his telescope, Castor might be seen double in bright twilight or even in broad daylight. Its telescopic aspect, with a power of 120, is shown in Fig. 20.

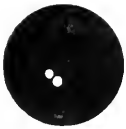


Fig. 20.—Castor.

ι Geminorum is another star which will repay examination. It will be found in the Map on p. 298. The small, purplish companion will be found above the principal star, and just to the left of the lower circle passing through it. κ (below Pollux in the same map) is a difficult and delicate pair, requiring a first-class instrument and acute vision to see the comes at all. 38 in this constellation (bottom square but one in the left-hand corner of Zodiacal Map, p. 225), though difficult, is a decidedly easier object than κ. In both these stars the contrasted colours of the companions are very fine. Many other objects will be found marked D and B in the map; but, being invisible to the naked eye, they are by no means easy to pick up without an equatorial mounting.

Cancri is not a constellation containing many objects of interest within the power of a three-inch telescope. Nevertheless the student will see ζ as a double star (it is really triple). ϕ^2 is another object, approximately as easy to see as ζ. 66 Cancri is decidedly more difficult; for, although the components are about the same distance apart as those of ϕ^2, their considerable inequality makes the comes look small by contrast. Fig. 21 exhibits it as seen when best defined with a power of 160.

Fig. 21.—66 Cancri.

ι Cancri is chiefly interesting from the contrasted colours of its components. They are, relatively, very wide apart. Should the observer possess a day eye-piece, he may put it on to scrutinise the Præsepe with. At all events, he must use the lowest power he has. The same eye-piece may be retained to look at another cluster, 67 Messier, somewhat to the west, or right, of α in the sky.

And now we arrive at a star which, while scarcely affording a crucial test, yet requires a very good eye and instrument to see it well and cleanly separated. We refer to the familiar one, γ Leonis (Map, p. 298), which, with a power of 160, should present the appearance indicated in Fig. 22.

A more difficult object, and one which will severely tax the powers, both optical and visual, of the observer, is ι Leonis (Map, p. 298). 54 Leonis is a charming object. There are a very great many small pairs in Leo; but the remarks which we have made above in connection with telescopic stars in Gemini are equally applicable here. If the student will fish about the apex of an equilateral triangle, whereof α and γ Leonis form the extremities of the base (to the left, or east, of the line joining them) with the lowest power at his disposal, he will find himself in a region rich in nebulæ.

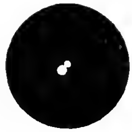


Fig. 22.—γ Leonis.

Underneath Leo in the maps will be found the foolish modern constellation of the Sextant. 35 Sextantis is worth looking at, as a curious disagreement exists as to the colour of the comes. There is a bright nebula, too, worth examination, in Sextans. It is 163 of Sir William Herschel's 1st Catalogue.

Hydra, straggling across the sky beneath Cancer, Sextans, Crater, Corvus, Virgo, and Libra contains a considerable number of interesting objects, though but few of them are susceptible of easy recognition. ε Hydræ is a fine pair, but difficult with such an instrument as we are employing, on account of the proximity of its components, and of their disparity in size. Of the objects in Crater and Corvus (two figures perched by the map-makers on Hydra's back), we need here only allude to 17 Crateris, an easy double star, with prettily-contrasted colours; and to δ Corvi, wider apart still, but exhibiting even more prominent tints in its components. About three-quarters of the way upon an imaginary line drawn from α to δ Corvi will be found a nebula, 65 of Sir William Herschel's 1st Catalogue. By this time, the incipient astronomer will probably feel that he has accomplished a fairly good night's work. Our next night we shall devote to Virgo and the neighbouring region of the sky.

A SPINNING-TOP.

IN an American paper, the *Literary Microcosm*, the following questions are asked:—

1. Why does a rapidly spinning-top, when tilted, tend to assume an upright position?

2. Why does it swing bodily and slowly around its pivot?

3. Why does this bodily motion take the direction of that part of the revolving surface of the leaning top which is nearest to the ground? And

4. Why does this bodily movement of the top become faster as its rotatory movement becomes slower?

These questions are worth careful study. The *Literary Microcosm* gives an utterly unscientific answer, based on the absurd conception that force is matter, and somehow explaining at the same time the rotation of a top and the duality of man; but the questions are really of interest, especially the first. We leave them to our readers, noting that a number of interesting experiments may be made by modifying the shape of the rotating body, and the manner of its rotation. One of these we have described in No. 11, p. 219.

The Editor.

Reviews.

ELECTRICITY AND MAGNETISM.*

A DEBT of gratitude is due to writers like Professor Thompson, who, being thoroughly masters of their subject, present, not their knowledge, but what the student wants to know of the subject, in clear and simple terms. The present book is a capital specimen of the sound popular science-treatise. It is clear, compact, and correct. It does not wander off into disquisitions about a multitude of matters too profound to be of interest to the learner; but every chapter, in every section, in every sentence, goes straight to business. The theory of electricity adopted by Professor Thompson is that electricity, whatever its true nature, is one not two, and that this electricity, whatever it may prove to be, is not matter and is not *energy*, but resembles both in one respect, that it can neither be created nor destroyed. The question whether positive electrification or negative electrification be the state in which there is more electricity than in the surrounding space is not decided, but Professor Thompson inclines to the belief that negative electrification is really the state of *excess*. The fact that the rate of dissipation of charge is greater for negative than for positive electrification seems certainly to point this way, for the law of the loss of charge is precisely the counterpart of the law of loss of heat, and it is well known that for equal differences of temperature between a body and its surroundings, the rate of loss of heat is greater at a higher temperature than at a lower, *i.e.*, the body that is really hotter loses its heat fastest.

The "Lesson on Terrestrial Magnetism" is one of the most interesting in the book. The statement that the north magnetic pole is in a particular latitude and longitude, ought, perhaps, to be modified, so as to indicate the probability, or rather the certainty, that the magnetic pole changes with the magnetic meridians. But this comes out clearly enough from other statements. The book is well illustrated and carefully printed. Wherever formulas occur, they are correctly given; this, in fact, is a characteristic of all works published by Messrs. Macmillan.

SPELLING AND PUNCTUATION.†

EVERY author and intending author, many students, and all printers, may study this little treatise with advantage and interest. It is also worth reading by those before whom the work of authors and compositors is presented in the form of printed books. "Literary men," says the late Mr. Beadnell, in the preface, "seldom pay much attention to such matters as punctuation and syllabication, often little to spelling (!); trusting to the printer, or rather his readers, to correct the errors and supply the deficiencies of their manuscripts." They ought not to do so, at least as regards punctuation; for the man who has thought out a subject should know better what he means to say about it than the compositor, who has simply to set up the author's words, and incorrect or defective punctuation often plays the mischief with the ideas which an author intends to present. Unfortunately, writers who are careless in this respect, injure those who punctuate for themselves. Printers have naturally learned to believe that an author, like Artemus Ward's "literary cuss," "can't punctooate wuth a cent"; so they punctuate for him, even when he has most carefully attended to the matter. He writes, let us say, "Rocks which are covered with seaweed are to some degree protected from the sun's heat," and by putting a comma after the words rocks and seaweed, they make him lay down two general propositions—very far from his real meaning—these, namely, that rocks are, as a general rule (1), covered with seaweed, and (2), to some degree protected from the sun's heat. So that, because many writers are too lazy to punctuate properly, the careful writer has to watch lest his meaning should be perverted by incorrect or concealed by excessive punctuation. Mr. Beadnell gives a very logical account of the comma, colon, semicolon, parenthesis, dash, &c.; it may be doubted, however, whether a little common sense is not better than the laws of logic, as a guide in this matter. In one or two places it has led our author astray. Thus, he says that where the subject of an affirmation has certain words attached to it which constitute the predicate and complement of the proposition, and are not (as they at first sight look) a thought interposed between the subject and the predicate, there must be no comma after the subject, giving as an example this sentence:—

> The French demurring to the conditions which the English commander offered, again commenced the action.

Here, he says, the Frenchmen's demurring to the conditions is not mentioned incidentally, as a parenthetical explanation, but is the principal proposition of the sentence upon which the next proposition depends. And then he gives, as a somewhat different example, to render the matter clearer, this:—

> The French having occupied Portugal, a British squadron, under Rear-Admiral Sir Samuel Hood, sailed for Madeira."

But the two cases are not only different, they are diverse. In the first sentence the punctuation is clearly wrong, in the second it is clearly right. The first is really equivalent to this: "As the French demurred to the conditions which the English commander offered, again commenced the action," which is absurd; a comma after "French" puts the sentence right. On the other hand, a comma after "French" in the second sentence makes it wrong and absurd (logically, it could then only bear the interpretation that Portugal is a British squadron under Rear-Admiral Sir Samuel Hood, which the French occupied, and then sailed for Madeira). The proper way of treating the first sentence, in order to show the importance of the words "demurring to," &c, is to do what Macaulay used to do in such cases (and very often when it was not wanted), to make two distinct sentences, thus:—

> "The French demurred to the conditions," &c. "They again commenced the action."

GEOMETRICAL EXERCISES FOR BEGINNERS.*

THESE exercises are intended to assist the young student of geometry to understand those propositions with which he has later to become familiar. They will be liked by lovers of old Euclid, as they deal more with ancient than modern geometry, and adopt the ancient methods of presenting geometrical truths. In some respects we are glad to see this, in others the ancient methods are not so well. We believe few things in Euclid, for instance, serve more to repel beginners than the general form in which the enunciations are presented. Not only—which is something—is a good deal of space wasted, but—which is

* "Elementary Lessons in Electricity and Magnetism." By Sylvanus P. Thompson, Professor of Experimental Physics in University College, Bristol. (London: Macmillan & Co.)

† "Spelling and Punctuation:" A Manual for Authors, Students, and Printers. By Henry Beadnell. (Wyman & Sons, London.)

* "Geometrical Exercises for Beginners." By Samuel Constable, Trinity College, Dublin.

much more the beginner has his attention first directed to a general proposition, which is often far from clear. The learner is always readier to understand particulars than generals; but in Euclid he has to give much thought to the interpretation of a statement couched in terms which seem to him vague and perplexing, until he has read their interpretation in the beginning of the demonstration. Then, if he really wants to understand what he is about, he goes back to the enunciation. There is no reason why this round-about course should be followed. Instead of beginning, for instance: "If two triangles have two sides of the one equal to two sides of the other, each to each, and if the angle contained by the two sides of the one be equal to the angle contained by the two sides, equal to them, of the other," and so forth—why should not the proportion proceed thus: "If in the triangles ABC, DEF, AB is equal to DE, and BC to EF, also the angle ABC equal to the angle DEF," &c. The attempt to interpret the meaning of the enunciation as a general proposition is in reality so much waste labour. It is akin to the waste labour which children at school used to be invited to bestow on such grammatical propositions as these, Where contingency and futurity are both implied, the verb should be in the subjunctive, instead of being told that such a sentence as "If it rains" is correct when you mean "if it is raining now," and incorrect when you mean "if it should rain to-morrow." Of course, the principle of these general propositions is sound enough, if we are writing for logicians, just as it is sound enough to define a plane superficies as one "in which any two points being taken, the straight line between them lies wholly in that superficies." But definitions and enunciations of logical precision are not for beginners. We wonder what Euclid would have said if he had been told his treatise would be used for learning first lessons in geometry? It is a pity, we think, that Mr. Constable did not simplify his book a little in this direction, having no enunciation distinct from the explanation of the figure. Even for more advanced mathematicians, enunciations are emphatic nuisances; the time given to interpret the roundabout phrases, necessary when a figure is not referred to, is just so much time wasted. So is the time wasted which is given to the wording of such enunciations. Often, despite the time thus wasted, the enunciation is not intelligible till a figure is drawn illustrating it. Thus "if from two diametrically opposite points on the circumference of a circle perpendiculars be drawn to a straight line outside the circle, the sum of these perpendiculars is constant." How are we to interpret this? Constant wherever the line may be, or whatever the size of the circle? or in whatever direction the diameter is drawn? or may any two of these vary? or all three! Still, this happens seldom in the book before us, and the use of enunciations is common to a great many treatises on geometry. The propositions are clearly and simply dealt with, and the work is very carefully printed and illustrated. In proposition 18, p. 135, it should be noticed that the construction fails if triangle SPR is less than one-nth part of the quadrilateral. There are, however, very few mistakes in the book.

DR. LUYS ON THE BRAIN.*

Dr. Luys has adopted a method of cerebral research which he regards as of extreme value. He makes regularly stratified sections of the cerebral tissue, and has these faithfully reproduced by means of photography: he then employs successively-graduated magnifying powers for the representation of certain details. By these new methods he has been able, he considers, "to penetrate further into the still unexplored regions of the nervous centres, and, like a traveller returned from distant lands, to bring back correct views and faithful representations of certain territories of which our predecessors caught scarcely a glimpse." After carefully surveying the elementary properties of the nervous elements, he arrives at the conclusion that it is by means of their combination, and by the harmonious co-ordination of all their truly specific energies, that the brain feels, remembers, and reacts; and that, in fact, they are the only living forces present, always underlying the infinite series of operations which the brain every moment accomplishes; and that, in fact, without them, that admirable and complex apparatus, at once so delicate and so simple, would be as absolutely without life and without movement as the earth would be, without the sun.

The present work, in which Dr. Luys thus endeavours to carry the data of contemporary physiology into the hitherto uninvaded domain of speculative psychology, is full of interest, despite its occasional too technical terminology. The book is in part suitable only for medical men, but in greater part it is easily to be understood by the general reader. We may not accept Dr. Luys' opinion that "from this time forth a true physiology of the brain has been established as legitimately as the physiology of the heart, lungs, or muscular system," but he has done much to show that we are fairly on our way towards this result, "a consummation devoutly to be wished."

Some of the facts used by our author to illustrate his views are very interesting when thus viewed. Such, for instance, are the familiar phenomena of association. "It is sufficient," as he says, "to see an object or a person—to hear a name pronounced accidentally, to smell an odour—in order to feel arising within us a series of ideas which arose at the moment when this impression was at first perceived by us. We all know how frequently in current conversation a word—a simple sound—causes the primitive direction of our ideas to diverge." This curious property, which sensorial impressions, received at the same time, possess, and which constitutes, as it were, natural families among them, is a great resource in the education of the intellect, and the methodic cultivation of the faculties.

Among the singular facts referred to in this work may be mentioned the experiments of Charles Robin, on the corpse of a decapitated individual (Ch. Robin, "Journal de l'Anatomie, Paris 1869, p. 90). They showed that the automatic activities of the spinal cord in man may continue to exhibit undiminished energy, and power of co-ordinations in the form of regularly-associated movements with a definite object (such as movements of defence made by the hand after a cutaneous excitation), these being performed with as much regularity as though the brain had directed them.

* "The Brain and its Functions." By J. Luys, Physician to the Hospice de la Salpêtrière. (London.)

GANOT'S PHYSICS.*

A book so well known and so widely valued as Ganot's Physics, scarcely requires the evidence which the issue of a tenth edition affords, to show that it is thoroughly sound and trustworthy. It is a text-book which fulfils the purpose which we have set before us in Knowledge, being clearly worded, yet exactly described. It is very well illustrated, and the various physical subjects dealt with in

* "Elementary Treatise on Physics, Experimental and Applied." For the Use of Colleges and Schools. Translated and edited from Ganot's "Éléments de Physique." By E. Atkinson, Ph.D., F.C.S. Tenth edition. (Longmans & Co., London.)

its pages—the properties of matter, hydrostatics, pneumatics, acoustics, heat, light, magnetism, and electricity are thus rendered as clear as they can be without actual experiment. But no student of science can read these pages without wishing to make, or to witness, some, at least, among the many experiments which are here illustrated and described. The tenth edition is enlarged by nearly 25 pages, and includes 24 additional illustrations. A very valuable feature of the work is the great number of numerical problems and examples in Physics. The student should not consider that he understands any section until he is able readily to work out for himself the problems illustrating that section.

WERE THE ANCIENT EGYPTIANS ACQUAINTED WITH THE MOVEMENT OF THE EARTH?

In the *Zeitschrift für Ægyptische Sprache* for 1864, the eminent Egyptologist, M. Chabas, published an essay to prove the Egyptians considered that the earth travelled in the heavens in a similar way to the sun and stars. The texts which showed this interesting fact are contained in two duplicate papyri at Berlin, numbered 2 and 4 of that collection, and as we believe no description of the narrative they contain has appeared in England, and it will assist students in arriving at a due appreciation of the scientific knowledge of ancient Egypt on the subject, it will be worth while to give a short account of its chief features, especially as we shall see in the sequel another text has been deciphered which corroborates M. Chabas' translation.*

The chief facts detailed in the Papyri are as follows: The agent of a high Egyptian functionary, of the name of Meruitens, had assaulted and robbed an agricultural labourer, who thereupon appealed to him for redress. Meruitens referred the matter to the king; his majesty, however, considering that he could best decide the question for himself, ordered him to make a proper inquiry and adjudicate on the spot. What the result was we do not know, because the Papyri are incomplete. The necessary evidence seems to have taken a long time to collect (perhaps Meruitens purposely postponed judgment), and during all this period the poor man was kept apart from his family, notwithstanding his continual appeals to the functionary to permit him to have access to his wife and children. Nearly all the papyri are occupied with the pleadings of the peasant and replies of Meruitens, and they are often so lengthy, and contain so many references to those high principles of justice and humanity, the maintenance of which were always the boast of the good Egyptian, that it seems likely the mere legal account of the case had been utilised by some scribe, who, by greatly amplifying all the arguments, contrived to inculcate many precepts of morality. It certainly seems very improbable that such interminable speeches, couched in excellent language, and touching upon subjects of great theological and scientific importance could proceed from the lips of a simple peasant. As might be expected, the suppliant endeavours by fulsome praises to soften the heart of the man who was to pronounce on his case, and among some of his hyperbolical encomiums occur the words which are of so much importance to us:—"The peasant came to supplicate the seventh time, saying"—"Great governor, my lord, thou art the helm of the (entire) earth, the earth navigates—according to thy will: Thou art the second brother of Thoth."

In order to properly appreciate this sentence, it must be remembered that in the Nile valley, where the river formed the great artery of communication, the idea of travelling or movement had from the first been associated with navigation. Hence we find that as determinatives of verbs signifying a journey, the figure of a boat, or two legs walking, are used indiscriminately. M. Chabas shows conclusively that the hieroglyphs here translated to navigate are precisely similar to those employed in speaking of the journey of the sun daily through the sky, and, among many other texts cited, quotes some referring to the motion of Mars and the apparent movement of Orion. The Egyptians, it should be remembered, always alluded to the heavens as an ocean (see also Genesis, i., 7), and spoke, in their figurative way, of the stars as personages, who sailed upon this celestial sea in sacred barks.

The title which the poetic countryman gives Meruitens, of heaven—guide of the earth—refers to the imaginary rudder which steers the course of the barque of the earth in space. The words "second brother of Thoth," are also noteworthy, he being a form of the deity, with two partly distinct phases. In one case he was the god of letters, and the primordial intelligence and order which established the harmony of the heavens, and made all the earth contained, and he it was who caused light to shine in the primæval gloom before the creation of the sun, and for ever guides the star in space as Meruitens was said to steer the earth. In the other he was a lunar deity, and pre-eminently the brother of the earth from an astronomical point of view. In concluding this account of M. Chabas' paper, it may be mentioned that the Pharaoh to whom Meruitens referred is named Neb-ka-ra. This King is the forty-fifth on the Sakkara tablet, replaced seventh in the third dynasty by Maspero. He is, therefore, a monarch who flourished before the erection of the Great Pyramids. From this and the very ancient character of the writing of the papyri, it is evident that the texts are of extreme antiquity. For how many centuries previous to their being penned the Egyptians knew of the movement of the earth, cannot, of course, be decided.

A Member of the Society of Biblical Archæology.

(*To be continued.*)

* Maspero says that, according to a Berlin papyrus, the sun itself was considered to obey the law of universal motion, and to travel in space like the wandering stars. See "Hist. Ancienne."

MALARIAL ORGANISMS IN THE BLOOD.

In the blood of patients suffering from malarial poisoning M. A. Laveran has found parasitic organisms, very definite in form and most remarkable in character. Some were cylindrical, curved bodies, pointed at the extremities, with a delicate outline and a transparent body, colourless except for a blackish spot in the middle, due to pigment granules. On the concave side a fine line could often be traced, which seemed to unite the extremities of the crescent. These bodies presented no movement. Spherical organisms were also seen, transparent, of about the diameter of a red blood corpuscle, containing pigment grains, which, in a state of rest, were often arranged in a definite circle, but sometimes presented rapid movements, and then lost their regular arrangement. On the borders of the spherules very fine filaments could often be perceived in rapid movement. These filaments were in length three or four times the diameter of a red corpuscle. Their number varied. Sometimes three or four were seen round a spherule, to which they communicated an oscillatory movement, displacing the adjacent red corpuscles. The free extremities of the filaments were slightly reflexed. When at rest, the filaments were invisible on account of their tenuity and perfect transparence. These mobile filaments appeared finally by becoming detached from the pigmented spherules, continuing, however, to move freely amidst the corpuscles. There were also bodies of spherical or irregular form, transparent or finely granular, about the hundredth of a micro-millimètre in diameter, containing dark red, rounded pigment grains, either regularly arranged at the periphery, or aggregated at some part of the spherule. The bodies and granules were both motionless. These appear to be the ultimate or "cadaveric" stage of those last described. They have no nuclei, and do not tint with carmine, a distinction from the pigmented leucocytes with which they have hitherto been confounded. Lastly, spherical elements were met with similar to those already described, but much smaller in size, and apparently representing a stage in their development. The animated nature of the mobile pigmented spherule, furnished with filaments, appears indisputable. M. Laveran regards it as a form of animalcule, which exists at first in an encysted state, and in the perfect condition becomes free in the form of mobile filaments, a mode of development not uncommon among the lower organisms. Besides these organisms, the blood of patients suffering from malarial fever contains (1) red corpuscles, which appear to be vacuolated at one or two spots, and contain pigment granules; (2) pigmented leucocytes; (3) free pigment granules, possibly proceeding from the destruction of the parasitical organisms.

These elements were first discovered by M. Laveran a year ago, and since then he has examined the blood in 192 patients affected with various symptoms of malarial poisoning, intermittent and continued fever, and palustral cachexia, and found the organisms in 180. The disease had been contracted for the most part in different regions of Algeria and Tunis. He convinced himself, by numerous and repeated observations, that these organisms are not to be found in the blood of persons suffering from diseases that are not of malarial origin. In most of the cases of malaria in which the examination yielded a negative result the patient had undergone a course of treatment with quinine, and to this fact the absence of the organisms from the blood was probably due. The addition of a minute quantity of a dilute solution of sulphate

of quinine to a drop of blood was found at once to destroy the organisms. In all the examinations great care was taken to preclude the entrance of any extraneous objects into the drop of blood examined. In general the parasitic bodies were found in the blood only at certain times, a little before, and at the moment of, the accession of the fever. In some very obstinate cases the organisms were always present in the blood. They rapidly disappeared under the influence of a quinine treatment. It is conjectured that in the apyrexial intervals the organisms probably sojourn in internal organs, especially the spleen and the liver. After death from malarial disease, pigment granules are found in great numbers in the blood, and especially in the small vessels of the spleen and liver; and they may be, in the most severe cases, so abundant that not only the spleen and liver, but the marrow of bone, and even the grey substance of the brain, are darkened by their presence. These pigment granules, which may obstruct the capillary vessels, appear to be derived from the parasitic elements, which perish after death, and become then unrecognisable. *Lancet.*

THE "SOUND" OF FISHES.

THE letter of your esteemed contributor, Mr. W. Mattieu Williams, on the subject of fish "sounds," affords a remarkable illustration of the truth of the old saying that a cobbler should stick to his last. For, so long as he confines himself to physics, his contributions are most valuable and interesting (especially the one in the issue for Feb. 10), but passing into the domain of the biologist, he must, I fear, be regarded as an intruder. The alleged mistake on the part of Dr. Andrew Wilson and the writers of anatomical works, is, I submit, no mistake at all, for that structure which fishermen and the public in general style the "sound" of a codfish is, indeed, the swim-bladder. Of course, my mere *ipse dixit* is of no more value than that of Mr. Williams, but in this case, as in a controversy between Professor Huxley and Mr. Waterhouse respecting the Scotch hares, the animal intervenes. I happen to have been to-day engaged in the dissection of a codfish, and can thus speak from actual observation to the following facts:—Firstly, the dorsal aorta is so completely hidden by the swim-bladder as to be inaccessible to any but persons accustomed to the use of the scalpel. Secondly, that it is of such insignificant size as to be utterly worthless as an article of diet. The fish upon which I operated weighed about 8 or 9 lb., and the dorsal aorta was a delicate, thin-walled tube about one-twelfth of an inch in diameter. Thirdly, that it was not "attached by its edges to each side of the under part of the spine of the fish," but that the swim-bladder, which is a "stout membranous bag," was so attached. The suggestion as to the etymon of the word "sound" appears, so far as I am able to judge, a very probable one.

OLD FOSSIL.

THE PYRAMID OF MEYDOOM.

MISS AMELIA B. EDWARDS quotes, in the *Academy*, the following letter, received by her from the Hon. J. Villiers Stuart, author of "Nile Gleanings":—"The other day I visited Meydoom. The pyramid has now been cleared down to the level of the desert, to which it descends in a series of great steps of beautifully-fitted masonry of fine white limestone. The joints are so close that it is often difficult to trace them. It is, in fact, more like cabinet-maker's work than mason's work. It must have been covered up from the remotest times, as it looks quite new towards the base. You would say it was but just finished. It comes next in size to the Pyramids of Ghizeh. The central chamber is tent-shaped—that is to say, the walls incline inward toward the roof, so as to reduce the span, and better enable it (the roof) to bear the enormous superincumbent weight. There were found in this chamber some pieces of timber, which seem to have been used to remove some heavy weight, perhaps the sarcophagus. It is, however, possible that, as in the case of the Pyramids of Ghizeh, there may be another chamber in which the sarcophagus still exists.

"When I last saw Prof. Maspero he seemed to doubt whether this was really Seneferoo's pyramid; but within five minutes' walk of the pyramid I discovered an inscription which conclusively proves that it is indeed the pyramid of that Pharaoh. The inscription occurs on the right hand top corner of the tomb of Nofre-Maat. It reads SENEFFEROO MENTE—*i.e.*, the 'resting-place,' 'abiding-place,' or 'cemetery' of Seneferoo. There is no context, and never has been any. The stone is quite uninjured, and the purpose of the inscription is to designate the locality in which the tomb is situated. It might be translated 'monument,' or 'pyramid of Seneferoo,' the name of which would attach itself to the adjoining cemetery. The hieroglyphic spelling of Mente (being the sign *Men* without the supplementary *n*) is very archaic. The inscription was probably cut during Seneferoo's lifetime. The reason why I failed to observe this important and interesting inscription on previous visits is that it is very high up; but the tomb of Nofre-Maat is now nearly filled with rubbish, which enabled me to mount close up to the hieroglyphs, and to examine them thoroughly. I saw the oval before, and figured it in 'Nile Gleanings' (p. 33), but not having an opera-glass with me, I overlooked the context. I have written to Prof. Maspero to tell him about it."

"The inscription," proceeds Miss Edwards, "copied in hieroglyphs by Mr. Villiers Stuart, consists of Seneferoo's name in a royal oval, under which, placed vertically, occur the ideographic sign, *Men*, a battlemented wall, and the phonetic sign *t*, an inverted basket. But in order to complete the word as given by Birch, Brugsch, and Pierret, not merely the supplementary *n* (a zigzag) is wanting, but also the determinative hieroglyph, a funereal couch, or bier. I regret to have to add that, at the time of writing the foregoing letter, Mr. Villiers Stuart informs me that he was suffering severely from a fall down a shaft 20 ft. in depth. He has, however, escaped without serious injury."

A PRETTY GEOMETRICAL PROBLEM.

(*Page* 229, *Jan.* 13.)

EXCEPT the propounder of the question, none of our readers have correctly solved either this problem, or Mogul's ("Given any rectangle, divide it by the fewest possible straight cuts, so that the parts can be put together to form a square"). J. Horne and ATC have given solutions of Student's problem, which at a first view seemed correct and neat. But they were not in reality sound.

Student's solution is as follows:—

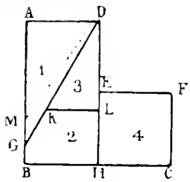


Fig. I.

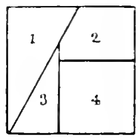


Fig. 2.

Taking AM = AE, join DM, and take AG = DM. Produce DE to H and in EH take EL = GB. Draw KL parallel to BH. Number the parts as shown in Fig. I, and fit them together as shown in Fig. 2. It is easily shown that they fit into a perfect square.

The problem cannot be solved with fewer cuts, but there are several other ways in which it can be solved with as few cuts, and an infinite number of ways in which it can be solved with five cuts.

Mogul's problem is so excellent that, supposing readers may have overlooked it, we leave it for another fortnight as an exercise.

EDITOR.

INTELLIGENCE IN ANIMALS.

MANY years ago, when a boy, I was at a country market with two friends, who each had a gig, and it was arranged that they would both drive home in one gig, while I drove the other. After journeying some four miles, I had got a considerable distance in advance, and arrived at the gate of a private avenue, towards which the horse turned, and then stopped, refusing to move another foot, although I tugged at the reins, whipped him, and even got out and tried to lead him on to the high road again, for I was quite familiar with his usual route, and had driven him on previous occasions, but move he would not. When my friends came up I was told that the horse had been accustomed of late to go through this avenue, it being a short cut.

In the year 1867 I got several monkeys at Auger Point (Java), and among them were two males, which we named Smiler and Tadpole, the former so called because when anyone went near him he showed his teeth in such a way that he appeared to be smiling, the latter because he had an unusually large head, while his body was small and weakly. Whenever these monkeys were fed, Smiler always managed to dispose of his food before Tadpole had commenced upon his reserve pouches in his cheeks. Smiler would then seize Tadpole, get him on his back, strike him under the jaw

to throw the food from the pouch into the mouth, then thrust his hand into Tadpole's mouth, and take out the half-chewed food, which he at once transferred to his own.

One day, while off the Cape of Good Hope in a gale, with a heavy sea running, our ship was close hauled under the lower topsails and fore-topmast staysail, when Smiler escaped from his house and ran aloft, perching himself on the fore-topmast cross-trees. Fearing lest he might be blown overboard, I sent a man aloft to catch him. When Smiler saw the man coming after him, he slipped down the jib halliards just beyond reach. The man shook the halliards, with a view of making him slip down to the jib-boom end, where another man was stationed to catch him, but Smiler losing his hold was blown far to leeward, and for the time disappeared.

We were all anxiously looking to leeward for him, but he was nowhere to be seen, when the cook rushed to windward, and putting his head over the rail saw him abreast the fore-rigging, and within heaving distance. An iron bolt was bent to the log-line and thrown to the little fellow, who immediately seized hold and was successfully hauled on board, smiling grimly.

Again, in the year 1869 I got a retriever pup in Yokohama, which I used to drill during the dog-watches at sea, by throwing articles along the deck for him to fetch back. Among other things was a soft felt hat, with a rather tall crown. At first this hat perplexed him much, as he usually seized it by the rim, and in running along the deck with it, tripped himself up by catching his fore-paws in the crown. After repeated trials with the same result, he laid the hat dow, and with his fore-paws and mouth rolled it up, and carried it in that manner. OCEAN.

A CARNIVOROUS PARROT.

THE remarkable bird, the *Nestor notabilis*, or Mountain Kea, of New Zealand, is a parrot of strong frame and powerful bill and claws, which were used, like those of all parrots, for obtaining a vegetable diet, until the colonists introduced sheep and pigs. As soon as this was done, the Kea seems to have abandoned vegetable food, and to have taken entirely to flesh eating. He attacks sick, or dying, or disabled sheep, and, with his powerful cutting beak, opens a passage through the back, and eats the intestines. Even healthy animals are sometimes assailed by the *Nestor notabilis*, and there are sheep-runs in New Zealand where considerable losses have been incurred through these strange birds. The specimen in the Zoological Gardens gave as much trouble to capture as an eagle, tearing the clothes of the shepherd, who knocked it down while pouncing on a lamb, and lacerating his hands. The Kea scorns cooked meat, biscuits, fruit, or seeds, and likes raw mutton better than any food. He will tear the skin and flesh from a sheep's head after the furious fashion of a vulture—leaving nothing but the bare skull. He at one time holds the morsels in his lifted claw, after the style of parrots, and at another grips them under his feet while rending with his feet like a hawk. This is a curious example of change of habit, for there is every reason to believe that before sheep and pigs were introduced into New Zealand the kea was as frugiverous in its meals as most, if not all other, parrots. He will now eat pork and beef as well as mutton, and has become, in fact, utterly and hopelessly carnivorous. It is to be feared, after this example, that temptation is often fatal to birds and beasts, as well as man. Had it not been for Captain Cook and the English sheep flocks, the *Nestor notabilis* would have lived and died innocent of crime; but now its blood-stained carcase is suspended outside many a sheepfold near Otago.—From the *Daily Telegraph*.

JACKO: A BABOON'S BIOGRAPHY.

MANY years ago, when stationed at an outpost on the Great Fish River, in the eastern frontier of South Africa, I was presented by the officer I relieved with a young baboon, which, when captured, was so young that it had to be brought up by hand.

From the first, I took a great deal of notice of it, and it became very much attached to me. It made great progress and grew up a healthy, strong animal.

Jacko was mischievous beyond expression, and the first time that I discovered that he had a temper of his own was on the following occasion:—

I had given him a saucer of bread and milk, and my wife, seeing that he had emptied the saucer, stooped, and put out her hand to remove it. He immediately flew at her, and tore her collar, making a hideous noise. I said, "This will never do, Master Jacko, you must be taught manners," upon which I handed my wife her riding whip, and desired her to whip him, holding him, myself, firmly. He howled and screamed loudly, looking round the while in search of something to fly at, but never attempted to touch either myself or my wife.

Jacko was always secured by a leather strap round his loins, to which was attached a strong steel chain, the end of which was secured by a strong padlock, which clasped an iron ring. This ring traversed freely up and down a pole, some seven feet in height, on the top of which a board was nailed, which, of course, kept the iron ring safely on the pole. This board was Jacko's favourite seat and post of observation. He was perfectly aware that the padlock and the board were his detainers; for he was constantly either picking at the padlock or working at the board, to try and loosen it, and, incredible as it may seem, he actually succeeded in disengaging the iron plate from the padlocks, compelling me to renew them frequently.

Jacko's pole was always erected close to my quarters, and I could watch his proceedings from my window unknown to him, and they were always most amusing. It is the custom in barracks for the pioneers to go round and sweep up the barrack square. One morning I saw a man, with a wheelbarrow full of straw and other rubbish, sweepings of the square, put down his barrow near Jacko's pole while he was sweeping in the immediate neighbourhood. Jacko was seated upon his high perch, apparently taking no notice of what was going on. Presently I saw the pioneer disappear to sweep round a corner. Jacko was down like lightning, capsized the barrow, and with his long and powerful arms scattered the contents in every direction, and when the pioneer appeared, was up on his perch again with wonderful celerity, looking in quite a different direction, with a face of the most ludicrous innocence.

One morning, from my look-out window, seeing Jacko come down from his perch very demurely to the ground, and slacken his chain, and then lie down, as if innocently basking in the sun, I felt fully aware that mischief was brewing. Presently I saw a fat little puppy appear on the scene, and Master Jacko's sleepy-looking eye fixed upon it most intently. When the puppy had strayed within reach, Jacko's chain was quietly tightened, and with his hind leg he seized it; and immediately clasping it in his arms, he clambered with it to the top of his high perch. For a short time the puppy was nursed and dangled in his arms, just as a woman would nurse a baby; then he began a careful search for fleas, with which the poor little thing was tormented. All at once a bright idea seemed to strike him, for, grasping the puppy by the tail, and holding it out at arm's length, and, looking, with an expression of most innocent demureness, in the opposite direction, he quietly opened his hand, and down fell the poor little animal, with a "thud," to the ground. I ran out to succour the poor little brute, and scolded Jacko vigorously for his cruelty, which, however, was perfectly useless, for he instantly assumed a pre-occupied air, and was apparently intensely interested in some imaginary object in the distance.

One morning I perceived that Jacko had loosened his perch, and witnessed his triumphant look when he had succeeded in throwing it to the ground. He now had nothing but the small top of his pole to stand on, and thus standing, he pulled up the chain, and brought the ring close to the top, but found his feet in the way. This puzzled him for some little time. At last a happy thought struck him, and seizing the ring with both hands, he jumped into the air, and the next minute was scouring the barrack square.

The rattle of Jacko's chain, and the cry of "Jacko's loose," was always the forerunner of a race for refuge and a slamming of doors among the female members of our community.

It was perfectly useless to provide him with any place of refuge or shelter, as his energies were at once at work to destroy it, in which he succeeded uncommonly well; so, at night, a sack was suspended from the top of his pole, into which he nestled himself with great comfort; and it is a singular fact, that at night I could go up to his pole and touch his sack, and he never attempted to move, but would give me an affectionate grunt of glad welcome, but if any other individual approached within a yard of his pole, Jacko was out of his nest in an instant, screaming, and prepared for battle.

On a cold, wet, rainy night I used often to take him a bowl full of hot coffee, and knowing there was sugar at the bottom of the bowl, he could not resist the temptation of plunging his hand into it to search for the sugar, although the coffee was so hot that he was obliged to cry out with the pain.

Had I not witnessed what I am about to relate, I could not have believed it. I have stood within thirty yards of his pole with my bow and arrow, and taking deliberate aim, have launched an arrow at him. Jacko would invariably catch the arrow in his hand, holding it until I went up and claimed it, when he always gave it up readily. But it is most remarkable that if any other person took the bow and fired at him, Jacko, on catching the arrow, always broke it in pieces. I need not say that on these occasions I invariably gave my worst arrows to my friends.

One wet afternoon I had chained Jacko up in the Cape Corps stable for shelter, and when the men had finished grooming their horses, I heard an unusual hubbub in that direction, and on going down to ascertain the cause, I found that Master Jacko had monopolised a jacket belonging to one of the men, and had covered himself snugly with it, positively refusing to give it up, and nobody dared to take it from him until I came to return it to its owner.

Jacko's pole was, of course, the centre of attraction to all the soldiers. They were always feeding him, or playing him tricks, which at last he repaid with a will.

He would catch anything that was thrown at him, thoroughly investigating the nature of the article he had caught. He was very partial to eggs, whether boiled or raw, and it was most amusing to see him tossing a hot egg from hand to hand, screaming the while, but never letting it go.

He was fond of his grog, weak wine and water, which was given to him occasionally in a bottle, tightly corked, and it was one of the pet amusements to see him pick out the cork, bit by bit, with his very strong nail; but to show how perfectly well he understood the use of the cork, when he had picked away as much as he could reach with his fingers, and still found himself unable to get at the contents, he would take up the bottle and crack the neck off against his pole.

To give one instance of Jacko's deep cunning, my company was on the line of march to an outpost. My wife and I were riding a few hundred yards in rear of the men, Jacko, as usual, loose and following us like a dog. We observed a Fingoe sitting on an ant-heap, about thirty yards from the roadside, with his wife standing within a few feet of him, holding in her hand a fine cob of Indian corn. All at once we saw Jacko walk up to the Fingoe and make friends with him (a most unusual thing, as he never took to the natives), and even sitting on the Fingoe's knee. Then we saw him make a spring, and, in the jump seize the Indian corn, and, running for his life, he caught hold of my stirrup and was on the pommel of my saddle like lightning. The Fingoe was much enraged, and threw his "knobkerie" at him, so I pacified the man, much to his delight, by giving him a bit of tobacco.

I have already said that the soldiers were very fond of Jacko, and, in the evening especially, they would surround his pole, playing with him; but if he suddenly caught sight of me coming into the barrack square, he would immediately go round the circle, biting every one of the men, dash up on his perch, and scream frantically, as if trying to persuade me that he was the injured party appealing to my protection.

Although brought up by hand, his intuitive perception of danger and recognition of his enemies were remarkable. If I wished to keep him up on his pole, I had only to coil a dead snake at the bottom of it, and no dainty would induce him to come down; and when I was absent from my post, and the alarm cry of "Jacko is loose" sounded, my wife had only to put a leopard's skin, with the head stuffed, in the doorway, and the quarters were perfectly safe from Master Jacko's intrusion.

On my being ordered home from the Cape, I left poor Jacko in charge of the men of my company, who said: "Never fear, your honour, we'll take the best of care of Jacko; he'll be our captain now." But soon after I left for England, the Kaffir War broke out, and in the confusion of war preparations Jacko's further fate was buried in oblivion. LIEUT.-COLONEL T. PERCIVAL TOUZEL.

CHANGES ON THE SURFACE OF JUPITER.

By PROF. C. W. PRITCHETT, GLASGOW, MO., U.S.A.

THE changes which have taken place, within the last three years, on the apparent surface of the planet Jupiter, are really wonderful. To one who has seen the giant planet but a few times in his life, and even to an astronomer, who has not noted from week to week the markings on his surface, a detailed account of their changes would be almost incredible. Perhaps the phenomenon of the great red spot, which became so conspicuous in July, 1878, and which still persistently holds its place, has awakened an unusual interest in the study of his surface; but certain it is, that never before has his disc been so closely watched, and never have so many phenomena been noted in so short a time as within the last three and a-half years.

In this note, my object is not to describe these changes, but specially to mention an instance observed here on the night of December 23. It chanced to be one of the finest nights of the whole year. The surface of the great planet was rarely ever seen under better conditions of altitude and atmosphere. Every line and marking came out with a distinctness which was a wonder even to an experienced observer. The great red spot, by the Jovian rotation, was approaching the central meridian of the disc; and I had begun my usual observation of the transit of the preceding end, when my attention was called to a condensed white nucleus situated in the north margin of the most southern of the equatorial belts. The threads of the Filar Micrometer, had been adjusted to the rotation axis of the planet, by the ephemeris of Mr. A. Marth. (Month. Not. R.A.S., vol. 41, No. 7.) One fixed thread was placed on one extremity of major axis of spot, and the movable thread was placed on the other extremity of that axis, and these threads were kept to this position by the driving clock and an adjusting screw. At 7 h. 7 m. of local mean time, the following end of red spot and the bright nucleus were on the same thread, or the bright spot was on the same Jovian meridian directly north of the following end of red spot. As it requires more than one hour for the Jovian rotation to carry the major axis of spot across the central meridian, and all changes of relative position must take place between my micrometer threads, I had a very rare opportunity to compare changes, however slight. Not twenty minutes had passed till I could see, independently of the threads, that the white spot had a rapid motion relatively to the red spot. It was so marked and proceeded so uniformly with the time that I resolved to measure it minutely. My observation of the transit closed at 8 h. 10 m. and then I estimated by the eye that the bright nucleus had gained on the following end of the red spot, in one hour, three-eights of the interval between my threads. The mean of a number of careful measures proved it to be three hundred and sixty-one one-thousandths of the interval, or 4·33" of the Jovian disc.

Now, the question comes up, was this a motion of translation? If so, we shall have to believe that a motion can take place in the Jovian atmosphere at the rate of nearly seven thousand miles per hour. As this seems scarcely credible, I prefer to think that this angular displacement is the index of a progressing transmission of light through a lower stratum of atmosphere, or else a part of an auroral display. I am the more inclined to consider the phenomenon as the result of a progressive transmission of light from the body of the planet through changing media, since the size and consistency of the nucleus changed considerably during the hour. I decline, however, to speculate on the subject, and give the fact and measure for what they may be worth.

Within the last few years many of these bright spots have been observed. That they seem to have a rapid motion has been shown over and over again. Some of them have been followed entirely around the planet.

I will add, that for the last two years the colour of the equatorial belts has remained grey or brown; for some time previously they had borne a ruddy hue. Occasionally the margins of the main belts have been tinged with red, and sometimes with a very fine blue. Within the last two years two very marked changes have occurred: 1. There are now three distinct broad equatorial belts in place of two. 2. A very conspicuous belt now stretches entirely across the Southern Hemisphere of the planet, and in apparent contact with the red spot. Its northern margin, on the finest nights, is almost blue. It has been forming for months past, but has taken its distinct outline within the last six months. It is now a prominent feature of the great disc immediately south of red spot. It would be highly instructive could all these successive changes be presented to the eye by such drawings as would show not only the correct shape and outline through successive weeks, but also the changes in consistency, continuity, and colour.

NOTE.—On January 22nd, at 7 h. 30 m., an entirely new belt was observed in the Equatorial Zone of Jupiter, situated between the middle and southern equatorial belts. It was continuous and very fine and sharp. I have never before seen a belt in this position, though I have carefully examined the planet many hundreds of times within the last few years. The space between the broad equatorial belts is usually more or less filled with irregular cloud masses. On this occasion it was entirely free from them. The middle and northern equatorial belts were very fine and even, and assumed their usual ruddy hue, while the southern equatorial belt was nearly three times broader than the other two, and was quite dark. The atmosphere was the finest I ever had for distinct and steady images.

A COLLIE'S SENSE OF DUTY.—A touching story of sheep-gathering was recently told me on good authority. A shepherd lost his large flock on the Scotch mountains in a fog. After fruitless search he returned to his cottage, bidding his collie find the sheep if she could. The collie, who was near giving birth to her young, understood his orders and disappeared in the mist, not returning for many hours. At last she came home in miserable plight, driving before her the last stray sheep, and carrying in her mouth a puppy of her own! She had of necessity left the rest of her litter to perish on the hills, and in the intervals of their birth the poor beast had performed her task and driven home the sheep. Her last puppy only she had contrived to save.—Frances Power Cobbe, in the *Cornhill Magazine*.

STARS FOR MARCH

On March 1, at 10.30 p.m.
On March 4, at 10.15 p.m.
On March 8, at 10 p.m.
On March 12, at 9.45 p.m.
On March 16, at 9.30 p.m.
On March 20, at 9.15 p.m.
On March 23, at 9 p.m.
On March 26, at 8.45 p.m.
On March 30, at 8.30 p.m.
On April 2, at 8.15 p.m.

Our Star Map.—The circular boundary of the map represents the horizon. The map shows also the position of the equator and of that portion of the Zodiac now most favourably situated for observation. For the motions of the planet Mars, now favourably situated for observation, see the Zodiacal map No. 11. In No. 19 (next number) the path of Uranus from January 25 to July is shown. Uranus is at his brightest on March 6, and would then be visible to the naked eye were it not that the moon, being nearly full, will obliterate him from view.

TO CORRESPONDENTS

Letters to the Editor.

[The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with the writers. All communications should be as short as possible, consistently with full and clear statements of the writer's meaning.]

All Editorial communications should be addressed to the Editor of KNOWLEDGE; *all Business communications to the Publishers, at the Office, 74, Great Queen-street, W.C.*

All Remittances, Cheques, and Post-Office Orders should be made payable to Messrs. Wyman & Sons.

⁂ All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.

All Letters or Queries to the Editor which require attention in the current issue of **KNOWLEDGE**, *should reach the Publishing Office not later than the Saturday preceding the day of publication.*

(I.) Letters to have a chance of appearing must be concise; they must be drawn up in the form adopted for letters here, so that they may go untouched to the printers; private communications, therefore, as well as queries, or replies to queries (intended to appear as such) should be written on separate leaves.

(II.) Queries and replies should be even more concise than letters; and drawn up in the form in which they are here presented, with brackets for number in case of queries, and the proper query number (bracketed) in case of replies.

(III.) Letters, queries, and replies which (either because too long, or unsuitable, or dealing with matters which others have discussed, or for any other reason) cannot find place here, will either be briefly referred to in answers to correspondents, or acknowledged in a column reserved for the purpose.

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

"God's Orthodoxy is Truth."—*Charles Kingsley.*

Our Correspondence Columns.

PRESERVING FOSSILS FROM THE LONDON CLAY. ENCRINITES.—VENTRILOQUISM.—STRATA ON THE GREAT WESTERN RAILWAY.—DIAMETER OF THE MOON'S IMAGE IN THE FOCUS OF A 12-INCH OBJECT-GLASS.

[293]—I can sympathise with "Lepidodendron" (query 284, p. 316) inasmuch as I once had the pain of seeing a quantity of beautiful fossils of my own from the London clay effloresce and crumble to a greenish powder. The only thing to do with those who behave in such a way is to preserve them in closely stoppered vials of water as soon as they are cleared from the matrix in which they are embedded. Sheppey flints are terrible things to decompose in this fashion. The blackish metallic nodules about which your correspondent asks is iron pyrites, or sulpherised iron, called "Copperas" by the people who collected them. They do frequently exhibit traces of vegetable remains. I am ignorant of the neighbourhood of West Drayton.

"Wilfred" (query 251, p. 316) of course knows that various forms of encrinite abound in our British mountain limestone. So far as I know though, the *Encrinus liliformis* is not found in our English rocks at all, but is confined to the German *Triassic Muschelkalk.* Its sole surviving representative is the *Pentacrinus Caput-Medusæ* in the Carribean Sea. This has a long-jointed stem fixed to the rock and supporting a cup-like receptacle containing the soft body and viscera. In the plates covering the upper part of this is an opening for the mouth. From the edge of the cup proceed fine jointed arms which ramify and ultimately terminate in articulated cirri or feelers. The joints composing the columns are pentangular, and groups of fine articulated tentacula issue from the column at intervals between the root and the top of it. It is really an echinoderm mounted on the top of a stem.

"Erin" (query 259, p. 364) will find all that is communicable in print on the subject on which he inquires in a little book called "Ventriloquism made Easy," published by Warne & Co.

In reply to query 264 (p. 364), the Great Western Railway runs over the London clay between Paddington and Reading. From Reading to Wallingford the subsoil is all chalk and chalk marl. From Wallington to Didcot we pass over the upper greensand. A few miles to the west of Steventon Station we get on to the lower greensand. After passing Uffington, we pass through the upper oolite, and at Shrivenham get on to the middle oolite. From Swindon to Wootton Bassett we are again on the upper oolite; from Wootton Bassett to Chippenham on the middle oolite, and from Chippenham to Corsham on the lower oolite. Then to Bath is through the lias strata to [illegible], which continues to Bristol. The triassic strata are next met with between Bristol and Yatton, whence a [illegible] brings us to the mountain limestone. After this, the country is covered with alluvium, until we get to Bridgewater. Between which town and Taunton our journey is mainly over the triassic deposits again, [illegible] from Taunton to Exeter we practically traverse the new red sandstone for the entire distance. "C. E. S." should look up the account of these various formations in any elementary work on Geology.

A FELLOW OF THE ROYAL ASTRONOMICAL SOCIETY.

GREAT COMET OF 1861.

[294]—I ought to have asked admission before, but perhaps it may not be too late now, for an observation of mine on the tail of the Great Comet of 1861. I was fortunate enough to see it on the night of June 31, when its magnificence was at its height, and I obtained a very interesting sketch, with a 5½ inch object-glass, of the unsymmetrical structure of the head, which seems so frequently to characterise the larger comets. But what I wish particularly to mention was the appearance of the tail, which, as represented in the graphic sketch in KNOWLEDGE, No. 5, p. 87, was spread out like a fan. The eastern edge of this, owing perhaps to trees or a rising moon, escaped my notice; but the central ray I traced for at least 90°, first curved to the left, and then straightened near Polaris. About midnight my wife pointed out to me a great separate beam, some 3° or 3½° broad, lying far W. under the square of Ursa Major, having ζ Ursæ in the lower edge, and Cor Caroli about 1° above its upper, traceable about half-way from the latter star to Arcturus, and pointing with its other extremity to the head of the comet, though, owing to the summer twilight, no connection could be made out. In about 20m. I found that it had risen higher, so as to stand midway between ζ and η Ursæ, and its termination, now much more distinct, was plainly visible near ε Bootis; but some times afterwards the beam was no longer visible. This unexpected change of position, contrary to the general motion of the comet, led me at the time to think that, notwithstanding its similarity to the rest of the train, it might be only a cirrus cloud lighted by the risen moon, and coming up with the breeze; but subsequent comparison with a drawing kindly sent me by George Williams, Esq., of Liverpool—much resembling the sketch in KNOWLEDGE—led me to the conviction that it was part of the outspread tail, and that the observed movement was the effect of perspective, this long streamer having passed so swiftly and closely over the earth, that the apparent closing up of the great fan from increasing distance had been distinctly perceptible.

T. W. WEBB.

IS SPACE BOUNDLESS?

[295]—In Clifford's Essays and Helmholtz's Lectures (Second series) are considerations on flat, spherical, and pseudo-spherical surfaces, which seem to destroy the certainty of Euclid's postulates and Kant's intuitions. Clifford is fragmentary, but his conclusion is evident; he prefers to believe that space is limited and spherical.

Helmholtz's lecture is obscure, perhaps owing to the translation. I know his first series suffered grievously in translation, but I can gather that the idea of limited space involves the idea of bodies and movements diminishing as they near the circumference of such space. Now, *could* Clifford entertain a belief involving such a condition? Helmholtz seems to hold that pseudo-spherical surfaces may be infinite, and may be imagined so. I cannot imagine them so, and it seems to me that a surface consisting of two opposite curves must come to an end by the curves re-entering themselves, just as a spherical surface does.

Could you do anything to make an ordinary intellect grasp these new and revolutionary views? Considering the transcendent importance of the subject, and that Gauss and Lobachewski are not to be attempted lightly, might you not give us a paper?

A word of congratulation on your journal. Its weekly appearance is quite an event here; it more than fulfils the high expectations which those who were familiar with your writings entertained on hearing of its being projected.

J. S. T.

PURPLE OF THE ANCIENTS.

[296]—May I ask, in reference to your article on "The Purple of the Ancients," what *colour* this ancient purple was? I once attended a lecture in which the lecturer proved, to his own satisfaction at least, that the ancient purple was *red*—a sort of vermilion

Your article, although telling us a lot about the dye, does not state the actual colour. Was not the Imperial purple of the Cæsars red? I have seen it so depicted in many paintings. A. J. Martin.

TOBACCO AND SCIENCE.

[297]—J. P. Grall (190) makes some inquiry under this head. I will make some others. When smoking a cigarette (with a mouthpiece), if, after inhaling the smoke, you remove the cigarette, holding it horizontally, you will see that a little smoke escapes from the mouthpiece and ascends; the smoke which remains in the mouthpiece does not ascend, but falls to the lower part and there remains. Why?

When smoking, no matter whether pipe, cigar, or cigarette, after inhaling puff it out immediately, and the smoke which comes out of the mouth, as well as that which gently rises from the pipe or the lighted end of cigar or cigarette is of a delicate pale blue; but if, after inhaling, you retain the smoke in your mouth for two or three seconds before exhaling it, it will be of a pale dirty-brown colour. Why should it be so? A. T. C.

THE RADIOMETER.

[298]—On exposing a radiometer to direct rays of coloured light, obtained by a sciopticon lantern and coloured gelatine sheets, I got the following results, the vanes of the radiometer in each case starting from a state of rest.

Red light	gave 39	revolutions in 1 minute.
Green „	„ 36	„ „ „
Blue „	„ 33	„ „ „

On turning up the light a little—

Red light	gave 46	revolutions in 1 minute.
Green „	„ 43	„ „ „
Blue „	„ 40	„ „ „

On turning down the light—

Red light	gave 43	revolutions of the vanes in 1 minute.
Green „	„ 40	„ „ „ „ „
Blue „	„ 37	„ „ „ „ „

These proportions appear constant, and would they not show that the red rays in the spectrum of solar light have most energy, and blue and violet the least? When I tried yellow light, produced in the same way, I got nearly as many revolutions of the vanes as with white light; I suppose this was owing to the yellow sheet allowing other rays to pass through. Can you advise any book on the subject? J. S. Gladstone.

STOVE-WARMED HOUSES.

[299]—I have read in your issue of the 3rd instant an article on the heat from American stoves. Now, sir, I trust that you will permit me to give my very long experience of these invaluable house-warmers. I lived in Canada and the United States from 1832 to 1878, in houses wholly heated by stoves. In 1854 a coal stove was brought out in Troy, State of New York, to burn anthracite coal. I procured one of these stoves and placed it in the entrance-hall of my house. That house was 64 by 38 feet, and this stove, although far inferior to the "Crown Jewel" stoves, manufactured by the Detroit Stove Company, now on view at Kensington, heated every part of my large house, which was situated on the bank of the St. Lawrence, on a bay nine miles wide, where the outside temperature was frequently 38° below zero. We kept a tin evaporator on the stove to throw off sufficient moisture, and, during the whole time this stove was in use, we never had one case of sickness. I never had one day's illness in America, and I may say that since my return to England I have been half the time sick; this I attribute entirely to the absurd way in which the English attempt to heat their houses. With the best coal fire, in an ordinary room, we are roasted on the side next the fire and cold on the opposite. Every passage and room we enter has a different temperature. We need not, therefore, be astonished at the enormous number of bronchial, lung, and rheumatic diseases so prevalent in England. Justice.

[300]—May I ask Mr. W. Mattieu Williams one question through the medium of your paper? It is not for the sake of controversy, but to satisfy my own mind on the matter of "Stove Heating." Supposing the atmosphere inside a stove-heated room to be 50° (using Mr. Williams' figures), the amount of vapour required to saturate air of this heat is sufficient to support a column of 0·361 inches of mercury, and this is easily supplied, "for an English house is enveloped in a foggy atmosphere, and encased in damp surroundings." In the open air the temperature is 32; therefore, if the fire were allowed to go out, would it not follow that because the quantity of vapour in the room would be double the amount of that in the open air, condensation of the excess of vapour would take place?—Yours, &c., G. G. D.

FOSSILS IN METEORS.

[301]—Referring to the articles "Meteoric Organisms," and "No Organic Matter in Meteors," in No. 12 of Knowledge, I beg leave to point out, in order to prevent erroneous notions about German men of science to arise among English students, especially among the readers of your truly excellent journal, that the startling discoveries of Dr. Hahn, and the extravagant theories based thereon, were, immediately after their first appearance before the public, very ably discussed and thoroughly refuted by the eminent geologist, Professor Zittel, of Munich, in a paper which appeared, if I am not mistaken, a twelvemonth ago in the *Augsburger Allgemeine Zeitung*. Dr. Hahn is, I am given to understand, not at all a geologist, but a medical man; I may be further permitted to state that he is neither half insane nor a fool, but an eager amateur, whom it would be more charitable and just to describe as possessed by a rather ludicrous illusion concerning the real value of the strange results his cherished "scientific" observations have led him to.—Yours, &c., German Friend of Knowledge.

MANUFACTURE OF GAS FROM WOOD—ARRANGED SQUARES.

[302]—To make it intelligible how wood-gas (p. 246) can take the place of coal-gas for illuminating, F. C. S. should have added, for every 1,000 feet of commercial gas, 1½ gallons of naphtha are used. This being rich in carbon makes a bright, of an otherwise dull light.

The numbers in the Villa Alboni inscription, I take the Latin to say, add up horizontally, vertically, and diagonally (the maximum number of ways) to 369. This is found to be the case, and the total to be 3,321.—Yours, &c. C. T. B.

6, Prince's-terrace, Brighton.

EYESIGHT OF DOGS.

[303]—In an article on "Intelligence in Animals," in Knowledge, No. 2, page 29, there is the following remark, which I am surprised no one has already commented on, in alluding to dogs being near-sighted:—This writer says "We believe that there could not be quoted a single instance tending to show that a dog has been able to see as well as a very near-sighted man." My experiences with dogs leads me to quite a different conclusion. I had a retriever bitch which certainly saw better than a near-sighted man. To give one instance. Out shooting one day I hit a partridge very hard, which flew over a gate across a field, and fell into a cover some 200 yards off; the retriever, with its fore paws on the top of the gate, watched the bird. As soon as it fell she jumped over the gate, and ran straight to where the bird had fallen, and in a few minutes returned with it. Surely she was not near-sighted? I have often seen dogs notice game some distance off when they could have neither smelt nor heard them. A gentleman much interested in coursing tells me that he believes that many, if not most, dogs see remarkably well. Of course, dogs like Skye terriers, whose eyes are much covered with hair, cannot see so well as those which have shorter hair. One reason which may make dogs at times appear near-sighted is, that their eyes are so near the the ground that long grass, or any other obstacle, will prevent them from seeing an object which to a person of ordinary height is distinctly visible.—Yours, &c., G. S. S.

COMPARISON OF THE SEXES.

[304]—According to Huxley, the blood of men contains a larger proportion of "solid constituents" (these include the "corpuscles") than that of women; although, he adds (instructively), "the difference of sex is hardly at all exhibited by persons of flabby, or what is called lymphatic, constitution." (Physiology, Lesson iii., Sec. 17.) According to McKendrick, whereas the diameter of a single muscle-fibre is $\frac{1}{350}$th of an inch in an adult male, in an adult female it is only $\frac{1}{450}$th of an inch ("Outlines of Physiology," p. 79). The same author, on p. 82, remarks that smallness in the size of fibres, and fineness in the distribution of capillaries, and greatness of contractility, are concomitant circumstances. Thurman, a writer quoted in Bastian's "The Brain as an Organ of Mind," says, "My own observations fully confirm those of preceding writers as to the average weight of the adult male brain being about 10 per cent greater than that of the female, i.e., about 49 oz. to 44 oz." The same writer adds (see Bastian, p. 356), "For this purpose I have examined and compared the average brain weight for men and women at the

decennial periods from twenty to sixty. . . . Whilst the brain-weight is nearly 10 per cent. less in the female than in the male, the stature is only 8 per cent. less;" [and, therefore, the weight about 23 per cent. less.—ED.]—E. D. G.

CATS AND DOGS.—SCIENCE AND RELIGION.

[305]—A kitten, just weaned, was presented, a few years ago, to a near relative of mine, who already had in his house a little Maltese terrier bitch which had never had any puppies, and was not in the way to have any. Well, imagine our surprise when this little bitch at once began to suckle the kitten, and continued to do so, to my knowledge, for weeks, showing all the time an affection amounting to jealousy for the kitten. The suckling repeatedly went on upon my own lap, so in this case I will venture to state the fact positively.

May I support your own admirable resolves and dieta on this subject by the following excerpt from the "Life of C. Kingsley," near the end of vol. ii.:—"When a friend remarked that Darwin's explanation of certain geological phenomena would hardly be considered 'orthodox,' Charles Kingsley observed—'My friend, God's orthodoxy is Truth; if Darwin speaks the truth, he is orthodox.'" E. D. G.

PARTIAL LOSS OF SPEECH.

[306]—Reading your article in No. 14 of KNOWLEDGE on "Brain Troubles," has brought to my mind a very interesting instance of the *total* loss of speech under mental excitement, mentioned by Darwin in his "Expression of the Emotions," &c., (chap. xiii., p. 324):—"A small dinner-party was given in honour of an extremely shy man, who, when he rose to return thanks, rehearsed the speech, which he had evidently learnt by heart, in absolute silence, and did not utter a single word, but he acted as if he were speaking with much emphasis. His friends, perceiving how the case stood, loudly applauded the imaginary bursts of eloquence whenever his gestures indicated a pause, and the man never discovered that he had remained the whole time completely silent. On the contrary, he afterwards remarked to my friend, with much satisfaction, that he thought he had succeeded uncommonly well."

ARTHUR REEVE.

OPTICAL ILLUSIONS.

[307]—The following is an illusion which, I think, has not yet appeared in KNOWLEDGE:

A *B* *C*

• • • • • • • •

The distance from *A* to *B* appears to be greater than that from *B* to *C*, although they are in reality both exactly equal.—Yours, &c., F. W. G.

[308]—Take a sheet of penny stamps (one stamp will show it, but not so plainly as many), and look at the ellipse enclosing the Queen's head, on which are written the words "Postage and Inland Revenue." This ellipse appears of the form of a slightly elongated octagon. I see it plainer by averted vision.—Yours, &c.,

R. A. LAW.

DURATION OF LIFE.

[309]—How does Mr. Allison mean that it "depends on ourselves, in a great measure, whether we die at 35 or 75 years," when speaking of such fatal diseases as cancer, in his article under the above heading in KNOWLEDGE, page 228? S.

OPIUM—BACILLI—MICROSCOPICAL.

[310]—If "Young Pill-Box" will study the peristaltic action of the intestines in some standard physiological text-book, he will easily find the explanation he seeks. In lead-poisoning, one of the prominent symptoms is colic, *i.e.*, spasmodic contraction of different parts of the digestive canal. Opium (and, better still, belladonna), by removing contraction, relieves the constipation. Opium is also of great service in constipation from obstruction for a similar reason.

Would any of your readers kindly inform me where to find the most recent information anent Bacilli and their kindred? I mean from a natural history point of view. Most of the Botanical text-books pass very lightly over this group. I wish to study the subject from every point of view. As your readers are well aware, these micro-organisms are of the very highest interest just now (*vide* Pasteur's researches, &c.) I have Cohn's papers.

Also, will some practical microscopist kindly inform me whether the F objective of Zeiss (=1-14 in. nearly), is sufficiently powerful with a low eye-piece for the investigation (of Bacilli, &c.)? The stage appliances I possess are of the simplest, and will not permit me to use a higher power.

I, and I feel sure, many others, would rejoice very heartily if the Editor of KNOWLEDGE would arrange for the appearance of microscopical papers similar to those on the practical use of the telescope now appearing. There does not happen to be any journal which gives poor beginners in microscope technology the aid which Mr. Proctor affords to beginners in telescope work. Will he listen to the cry of A MEDICAL WELL-WISHER?

[Our correspondent will find microscopy was introduced last week.—ED.]

TELESCOPE.

[311]—I should certainly advise "Country Solicitor" (237, p. 275) to invest his money in a silvered glass reflector. He can get one by the first maker in England, of $8\frac{1}{2}$ inches aperture and $6\frac{1}{2}$ feet focus (the largest size he could conveniently move about), well and firmly mounted, with a battery of nine or ten eyepieces, for £40. A refractor of equal power, *i.e.* about $7\frac{1}{2}$-inch aperture, would be some 10 feet long, would be almost necessarily a fixture, necessitating the erection of an observatory to put it in, and the object-glass alone, without tube, stand, or any accessories, would cost him at least twice as much as the reflector. The latter would certainly be the handier instrument of the two. He would be able to take it out of the house, and set it up in five minutes; and the silver film would certainly last, if decent care were taken of the mirror, at least two years, and, if tarnished then, would be renewed by the maker for half-a-guinea. Either the reflector or refractor would exhibit to him most of the objects in the new edition of Mr. Webb's book; though, of course, he could not expect to see the inner satellites of Uranus, or separate the close pairs in such stars as δ Equulei or ζ Sagittæ with such an instrument. H. SADLER.

VEGETARIANISM.

[312]—From letter 207, p. 251, it would appear that there are fanatics to be found amongst the opponents of vegetarianism.

The case quoted of the prisoners at Waltenburg proves simply nothing. The "theoretically nutritious" food was adopted only on the failure of the potato crop, and was certainly a poor bill of fare, even for a vegetarian. The more generous course which followed, and which created such a marked improvement in the condition of the prisoners, consisted of five articles of diet. Of these, three, at the least, enter largely into the *menu* of those who advocate the vegetarian system, and would probably have effected the same beneficial results without the aid of either meat or coffee.

ONE OPEN TO CONVICTION.

CHINESE COUNTING.

[313]—The Chinese have a method of reckoning by the aid of the fingers, including addition, subtraction, multiplication, and division, from 1 up to 100,000. Every finger on the left hand represents nine figures—viz., the little finger units, the ring finger tens, the middle finger hundreds, the forefinger thousands, the thumb tens of thousands. When the three joints of each finger are touched from the palm towards the tip, they count 1, 2, and 3 of each of the denominations aforesaid; 4, 5, and 6 are counted on the back of the finger-joints. In the same way, 7, 8, and 9 are counted on the right side of the joints from the palm to the tip. The forefinger of the right hand is used as a pointer.—Yours, &c.,

PECCAVI.

Queries.

[269]—RUST.—Why does salt water rust iron more than fresh wate.—G. W. J.

[270]—BLOWPIPE CHEMISTRY.—Could any reader of KNOWLEDGE give me the undermentioned information with reference to the blowpipe described in Vol. I., No. 7, page 137 of KNOWLEDGE, viz.: How and where the piece of brass is soldered on? also where the brass nozzle is to go; also the air balloon and the trumpet mouth-piece; and for what purpose the hole is bored through tube between A and B of drawing? as I have tried to form one and failed.—AMATEUR.

[271]—THE ELECTROPHOTE.—Some two years ago, an invention called the electrophote was announced, the name indicating, of course that rays of light could be transmitted along the electric wire, reproducing at one end of it the image of objects visible at the other. Could you, or some contributor, give your readers some information regarding this wonderful but obscure invention, or state where such could be found?—BOREAS. [There may have been some instrument by which a light-record of some sort was communicated, but nothing such as "Boreas" describes could have been accomplished in this way. The selenium electrophote story is probably referred to.—ED.]

[272]—THE MEASURES OF TIME AND TEMPERATURE.—Will you kindly inform me how we know that two portions of time are equal, and how we know that the difference of temperature of, for instance, 60 C. and 70 C. is the same as the difference of 70 C. and 80 C.?—FRANK MILLER. [We may measure time by any movements we have reason to consider uniform. As to heat measures, our degrees are arbitrary, and we know, as a matter of fact, that the expansion of mercury is not absolutely uniform for uniform increase of temperature, though very nearly so between certain limits.—ED.]

[273]—STRENGTH OF MATERIALS.—On page 217 of Anderson's "Strength of Materials" (Longman's "Text Books of Science") the equilibrium of a 30-ton crane is under consideration, and it appears that the balance of the vertical downward pressure upon the guide-rail (108 tons) and the vertical upward pressure upon the centre pillar (72) exceeds the total load upon the structure (30 tons). Is this according to mechanical principles? and if not, where is the fallacy in the reasoning which leads up to it?—LIBRA.

[274]—MISCELLANEOUS.—Will any reader briefly inform me (1) how to sort out the foraminifera of chalk for the microscope? (2) where to find a *résumé* of hypotheses on the ventricles of the brain? (3) how to get, quickly and cheaply, some notions of drawing?—EOZOON.

[275]—SALT.—On what grounds do many people object to the use of salt with food? Is this objection shared by any of our high medical authorities?—SPECULUM.

[276]—PHOTOGRAPHIC.—Would "A. Brothers" or any one else kindly give me their opinion on the following? 1. How to prepare iodized collodion, giving the proportions. If this would be too troublesome, as I have heard it is, where and at what price could I buy it? 2. Where can I buy the metal plates so much used by travelling photographers, how are they prepared in the conditions they are sold, what further is necessary for taking portraits by them, and what price are they? 3. How are those papers prepared sold with an imitation camera, and a small quantity of some white crystals to be dissolved in warm water, when the papers are dipped in the solution, hideous faces, &c., are developed; and what are the crystals?—W. E. F.

[277]—LUMINOUS PAINT.—Of what is it composed?—W. E. F.

[278]—SMELL FROM BURNING GAS.—What is the precise cause of smell noticed in room where gas has been burning for a time? It is noticed chiefly where a globe is used on the bracket. Is it bad consumption, to be remedied by better burner or more air, or is it caused by burning of particles of dust accumulating on the globe?—ALPHA.

[279]—STAMINA.—Would Mr. Grant Allen kindly explain the origin and use of the circle of pale green *cups* which surround the stamens of the Christmas rose?—TYRO.

[280]—ACTINIUM.—Can any reader tell me where I can get zinc white pigment which turns black when exposed to sunlight, as I have not seen any sample of zinc white turn black, when exposed to light as mentioned by Dr. T. L. Phipson, F.C.S., in *Chemical News*, some months ago?—ERIN.

[281]—REFLECTING TELESCOPE.—Which would be best, a metal or a glass mirror, and what metal would be best, as I am thinking of making a reflecting telescope?—T. JONES.

[282]—Is the use of smelling salts beneficial or otherwise? Why is it they are freely used by women, but not by men?—F. M.

[283]—GLASS.—At what temperatures do *crown* and *flint* glasses such as are used for telescopes come to the state of fusion necessary for purification from striæ and bubbles? What materials are best suited for crucibles and moulds, resisting the high temperature of the molten glass and unattacked thereby?—Q. L.

[284]—PENCIL-POINT PROTECTORS.—Will any reader kindly inform me how to restore the silver appearance to small articles made of the same material as pencil-point protectors?—ECONOMIST.

[285]—SCIENTIFIC TERMS.—Wanted, the name and the publisher's name of the best dictionary of scientific terms?—PRESTER W.

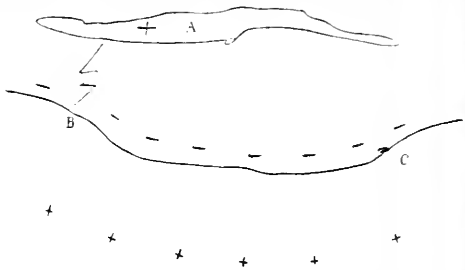
[286]—ELECTRICITY.—Which is the simplest and best text-book published on electricity and magnetism? What is the best kind of galvanic battery for strengthening the nerves; and what would be the price of same? I do not want an expensive one.—W. H.

Replies to Queries.

[illegible]—ABSTRACT REASONING.—In thirty years' reading on logic and philosophy I never met with a distinction between "objective" and "subjective" reasoning; I am inclined, therefore, to doubt if such a distinction is generally recognised. Again, Mr. W. I. Abbot asks for a "definition of abstract reasoning." I suppose such a term might mean either (*a*) reasoning about abstract subjects, such as mathematical problems; and (*b*) reasoning about any subjects, abstract or otherwise, according to the laws of pure logic, as distinguished from analogy, and perhaps, too, "inductive" logic.—E. D. G.

[259]—VENTRILOQUISM.—My qualification in offering explanations in this regard consists in the fact that I have long been a student of, and am the author of papers on, the subject of speech-production. The word ventriloquism means, strictly, "belly-speaking," a phrase which is really most inapt. Ventriloquism is, essentially, semi-speech. The tongue, which is the chief agent of speech, may be said to possess, practically, an anterior or vertical root, and one of a posterior or horizontal character. When the tongue is drawn back abnormally upon these bases, the lower jaw and lip will be found to protrude, and to assume a fixity of condition by reason of the correlation of the muscular powers centred near the larynx. In producing speech under the above conditions the main secret of the ventriloquist's art is readily discovered. Imitation of natural sounds is effected in modulating the voice, either by contraction of the larynx or by disposition of the nasal cavities. The only real difficulty lies in mastering or modifying the lip consonants. As in all other matters, perfection is only to be gained by continual practice.—H. W.—[Answered by several others.—ED.]

[260]—LIGHTNING.—"Secretary" appears to have had a very good illustration of the effect of a "return stroke." When a charged body is brought near a neutral one, it has the power of polarising it. Thus, the ground immediately under a heavily-charged thunder-cloud becomes highly polarised. When the lightning passes, the neutral condition is suddenly restored, sometimes doing more mischief than the lightning. Suppose the cloud, A, to be charged



+ly, then, of course, the surface of the earth under it will become − the + electricity being repelled as far as possible from the inducing body (the cloud). When the charge in A is too great, the insulating air is pierced, and the flash passes, say to B. The cottages, C, being above the general surface, are more −ly charged than the ground, and feel the "return stroke." Tyndall's "Lessons in Electricity" will help "Secretary."—EUCLID.

[268]—PHOTOGRAPHY.—Will "Anon" say what purpose he has in view, whether portrait or landscape photography, or the application of photography to some special purpose, when, as an amateur of experience, I shall be most happy to advise him. *En passant*, it is as well to remark that the practice of photography has been completely revolutionised during the last two or three years, and it is almost impossible to name any work on the subject suitable for a beginner, since the information he would need still remains squandered through the various technical publications of the last few years, not having been as yet condensed into *handbook* form.—E. T. W.

[317]—WILD FLOWERS.—The nearest approach to E. Taylor's requirements is Rev. C. Johns' "Flowers of the Field," 5s. (S.P.C.K.), containing a number of illustrations, as well as descriptions.—EUPTERIS.

BREWING.—To add to list of books on brewing:—"Brewing, Practical and Scientific," by E. R. Southby, M.R.C.S., F.C.S., published by us (*Country Brewers' Gazette*); "Brewing," by J. Herbert, Burton-on-Trent; and "The Art of Brewing" (Cornish, Holborn).—C. DEWSTON.

Answers to Correspondents.

**** All communications for the Editor requiring early attention should reach the Office on or before the Saturday preceding the current issue of KNOWLEDGE, the increasing circulation of which compels us to go to press early in the week.*

HINTS TO CORRESPONDENTS.—1. *No questions asking for scientific information can be answered through the post.* 2. *Letters sent to the Editor for correspondents cannot be forwarded; nor can the names or addresses of correspondents be given in answer to private inquiries.* 3. *No queries or replies savouring of the nature of advertisements can be inserted.* 4. *Letters, queries, and replies are inserted, unless contrary to Rule 3, free of charge.* 5. *Correspondents should write on one side only of the paper, and put drawings on a separate leaf.* 6. *Each letter, query, or reply should have a title, and in replying to letters or queries, reference should be made to the number of letter or query, the page on which it appears, and its title.*

S. S. Question answered at p. 91, No. 5.—R. MORTIMER. Not by me. Other matter (about probable explosions) suitable for daily papers—next time you think they are likely to occur.—TYRO ASTRONOMICUS. Subject of earth measurement not so simple as you seem to think.—SCIENCE AND ART. Fear most of our readers do not care twopence about the rules and regulations of the Science and Art Department; so can hardly find space for article showing that they "muchly need correcting."—J. MURRAY. My *dear* sir, it would take a page six times as large as ours to present your diagrams properly. The sun may expand our "utmostphier" on the one side, which gives to it a rotating motion, and "our earth approaching the sun down the decrescendo line," may be "the cause of solar time," but these profundities are not for us.—T. MACKENZIE. Thanks.—F. J. B., JOS. R. GREEN. Thanks; but those subjects already provided for.—REDDITUS. You calculate without considering interest; that might be the brokers' way, but it is quite incorrect.—ALL ROOTS. I would rather not "extract for you the fifth root of 5053632, or the eleventh root of 11, or the thirteenth root of 1·9, &c., &c." A rule can be given for such cases, but no one ever works them out otherwise than by logarithms.—J. ORMTHWAITE. I do not know "why Government did not appoint" me "Astronomer Royal"; nor can I conceive why I was not appointed to the command of the Channel Fleet. Perhaps it may have been that a special training is required for these offices, and that I have had none. Or it may be that I ought to have offered my services in one or other of those capacities. When I do, I shall expect a favourable response.—ENQUIRER. Not 8″·6, nearer 8″·88. For the calculation, try this:—Earth's equatorial radius (or say, 3,960 miles), at sun's distance, sustends 8″·88. Hence the full circumference of a circle having sun's distance as radius

$$=3{,}960 \text{ miles} \times 360 \times 60 \times 60 \div 8{\cdot}88,$$

and earth's mean distance is equal to this distance, divided by $2 \times 3{\cdot}14159265$.—A. RISCHGITZ. Fear the geometrical proof is familiar to most mathematicians.—T. J. POTTER. Thanks.—T. R. ALLINSON. Thanks for vegetarian great men. But wait till list of carnivorous great men is published. Why do you include the notorious "eccentric" Sir Richard Phillips? Two-thirds of the names you give are of such a class that we might fill ten numbers of KNOWLEDGE with the like, if once we began.—T. WOLSTENHOLME. Thanks; but instances of animals nursing young of other species now sufficient in number.—WERNER. There is "no plane of oscillation of the earth." Cannot understand question 2.—M. M. You will have noticed the misprint 37·75 inches for 0·3775.—E. T. WHITELOW. Thanks; but Mr. Brothers has promised such papers.—J. P. SANDLANDS. I am sorry that you think you have been treated unfairly; but if you remember the rule you infringed you will see that I had reason to complain, not you. You consider that "Found Links" and my own article on "Luck," touch on religious questions; therein I am disposed to agree with you. No subject of which we treat in these pages can fail to do so. But you wish to emphasise, and I decline to *have* emphasised, the difference of view which may exist according as these matters are viewed from different standpoints. Leaving Professor Wilson to deal with "Found Links," let me illustrate my point of view from my articles on "Luck." There can be no doubt that some minds object to a consideration of that subject in which the influence of Providence is left, *ex necessitate*, out of consideration: but you are not to consider that the entire absence of any direct discussion of Providence signifies an absence of recognition, still less a denial of its effect. You wish me to say what my views are in this respect; but this I decline to do, beyond such general reference to the matter as you find in my last article on the matter, page 311. If I allowed myself this privilege, whether my views are on your side or not, I could not refuse the same privilege to others. Believe me, I have no wish to act unfairly, and I do not think I can be justly charged with so doing.—IGNORAMUS NO. 1. You are right, see editorial letter on the subject in last number.—E. CLODD points out that as Prof. Tennant is dead (he died while I was abroad), letters addressed to him as suggested will probably not reach him.—C. J. C. On Sunday night, Feb. 19, at 7.43 P.M., saw a magnificent meteor. colour deep orange, path very short, leaving a trail of fire about 5°.—IGNORAMUS NO. 2. If we take De Vico's estimate, the day of Venus is, as you say, very nearly of the same length as our own, and, as you also say, she certainly has no moon whose tide-raising action could have lengthened it. But the sun would raise high tides on Venus, supposing her oceans like ours. An orb's tide-raising action varies inversely as the cube of the distance. Now, roughly, Venus's distance is seventy-two hundredths of the earth's, and cubing this proportion, we find that the tide-raising action of the sun on Venus exceeds his action on the earth in the same degree that 1,000 exceeds 373. Whence it follows, that as the sun's tide-raising action on the earth is two-fifths of the moon's, his action on Venus exceeds the moon's action on the earth as 2,000 exceeds 1,865. G. A. NEWCOMEN. Thanks. Answer a little too difficult.—DISAPPOINTED. Are we too miscellaneous or too astronomical? We can't be both, you know. However, in our last number, we were more astronomical than we have yet been or are likely to be again.—V. S. says that in most ships now the "forecastle" is under a raised deck forward, which is called the "top-gallant forecastle."—M. WELB, WEBB, or GELB? We *must* insist on the rule that letters and queries be kept separate and properly arranged for the printers, or we cannot deal with a tithe of the correspondence.—GEO. M. Not correct.—E. MORTEM. Thanks, question sufficiently answered already.—K. Thanks, picture almost too rough to give any idea of what the objects may have been.—F. W. CORY. Probably you are right in thinking that "A Constant Reader" would not find the weather prognostics for his district so often erroneous if he combined his observations with those made by others. Your letter is somewhat too long for insertion. We fear your suggested increase in the numbers of meteorological observers would only have the effect "of adding" (to use Sir Geo. Airy's expression) "millions of useless observations to the millions which already exist."—J. J. J. Some of those Planchette experiences are, I know, very singular, and very difficult to explain as phenomena illustrating the influence of imagination on corporal actions, which, nevertheless, I cannot but think they must be. I could not well open the matter for discussion here, simply because we could have no assurance of the truth of the accounts sent us. You are not to imagine that I question your statements—in fact, I have had somewhat similar experience within my own family circle.—H. A. BULLEY. Have given more than due space to moon's influence, which has been proved to be slight.—H. A. B. Lightning conductors do not act by repelling lightning, but the other way.—H. MUIRHEAD. The brain-wave theory must be put on a more scientific basis before it can be regarded as a part of knowledge. We have no experience of ethereal waves acting directly on the brain. What is the organ by which such waves affect the brain, and how are they conceived to act?—J. MACKENZIE, M.D. Nay. Admitting the abstract of an able lecture by an eminent doctor does not mean that we open the pages of KNOWLEDGE to the discussion of a medical subject. A paper sent us about vaccination, whether for or against, would be submitted to a competent medical adviser, and if regarded by him—first as sound, secondly as suitable—it would have a good chance of being admitted, if it were also short.—LESTOR FRANCIS. If you notice, we are beginning microscopic matters—but at the beginning; presently we shall come to details. You see the difficulty of getting all in at once? H'SETT. Your views seem near the truth, as far as at present seen, though it is not considered that evolution is necessarily advancing. We thoroughly agree with you that argument with persons unacquainted with geological evidence is utterly useless. We would insert much of your letter if our space were not so crowded. Note promise and hopes expressed in our last, however.—MARSHALL LEIGH. The manufacture of artificial wine hardly in our line.—J. A. S. We are likely to have chemical papers soon, but for your purpose the *Chemical News*, edited by Mr. Crookes, is the very thing. J. B. DIMBLEBY. The time measurements of your association are wrong, but how can one show it? Transits do not always occur in pairs, as your society seems to think; and some of your teams (poor Venus in teams) would be very lop-sided.—JOHN GLEDSTONE. The moon can nearly always be seen in that way, in clear weather, when new. In America I have seen the unilluminated part (or rather the part not illuminated by the sun) when the moon has been nearly half full. The darker part is illuminated by earth-light. Our earth would appear as a large nearly full moon in the sky above those parts of the moon.—R. MORTIMER. Yes, your warning reached the office of KNOWLEDGE before the colliery accident: but we did not see it till afterwards, and we have no daily issue. Such warnings, to be of use, should be sent to the daily papers.—WILLIAM MILLER. We cannot answer questions through the post, nor give in KNOWLEDGE an opinion as to articles advertised therein: that is, not in reply to questions. Consider how open to abuse such a course would be.—H. H. The tides raised by

the satellites of Mars would be very slight. Singularly enough, I considered this point, but I cannot now remember where (know it was when I edited the Monthly Notices of the Astronomical Society), before the small moons were discovered. W. WILSON, M.A. Your rejoinders, re-rejoinders, &c., remind me much of Boum, Puck, and Prince Paul, in "La Grande Duchesse." "Il doit être en train de monter," says Boum; "il traversera—et descendra—il retraversera, remontera, redescendra, reretraversera." "Reremontera," says Puck. "Reredescendra," says Prince Paul. "Et cætera, et cætera," says Puck. Don't you think we may say so too? We are not likely to agree. To take your illustrative case (so good of you to treat me as you do "the duller boys in your school"), I should say the sun sets even in a scientific treatise—not, "the earth rotating carries us out of view of the sun."—NEWTON CROSLAND. Dr Ball ought truly to be ashamed of himself, "falling into the same egregious blunder as Sir Isaac Newton," and I, too (being "in my senses, and especially as a mathematician"), for allowing him to fall "into the old jog trot nursery error." But my "own paper on the 'Menacing Comet' is almost as absurd as Dr. Ball's discourse on the moon." One step further, and I suppose I shall be "as great an idiot" as Sir Isaac Newton himself. But, my dear sir, if we are so foolish as you say, do please notice how generous we are; we might, by aspersing Newton and trying to detract from his great reputation, seem to assert our own superiority, and get people to say of us, with Bunthorne,

If that will not suit *thee*, which would very well suit *me*,
Why, what very, very sapient men these sapient men must be!

Instead of this, we are content to follow in Newton's footsteps as far as our feebler forces will take us. How happy it must make you to be able to say, "Until astronomers adopt my theory of polarity, the science of astronomy, or rather its exposition, must swarm with fallacies, contradictions, crudities, and nonsense."—Rev. H. H. HIGGINS. Many thanks. We have forwarded business part of your letter to publishers. The "absurd mistake" is, as you say, very amusing. Wallenstein was singularly fortunate; for astronomers have not hitherto seen Jupiter in Cassiopeia. Napoleon's referring to Venus, seen in the daytime, as *his* star, was scarcely less absurd, though in another way.—M. S. RIDLEY. The book was reviewed in an early number of KNOWLEDGE.—W. C. Because the stars are so far away. On the other point—emphatically, No; we will *not* discard the motto beginning, "There is no harm in making a mistake." It was one of the finest things ever said. Of course, if you choose to misunderstand it to mean that there is no harm in a mistake, left uncorrected, that is not our fault. Read it in conjunction with Faraday's remark on fixity of opinion, and you will find it contains a most useful lesson. Of course, a mistake is in itself unfortunate; but as no man can ever reach the truth without making mistakes, Liebig's saying remains true, even in its baldest sense. But equally of course, what he means is, that we should all be ready frankly to admit our mistakes. I believe, for my own part, there is no more useful scientific rule. Nay, I will go so far as to say that a mistake made by a well-known student of science, and frankly acknowledged, does at least as much to advance science as the discovery of a truth. But these short, pithy sayings, "jewels, ten words long, that on the stretched forefinger of all time sparkle for ever," are not for the prosaic mind. What do you suppose the old Greek philosopher would have said, if, in response to his "Know thyself," some one had answered, "I don't *want* to know myself; I would rather know somebody better worth knowing?"—SIR FR. B. Letter forwarded to publishers; there may be a day or two of delay. Letters addressed to editor are not opened at the office, and, in some cases, are kept a week before being opened at all.—H. A. BULLEY. Fogs like the dense London fogs are never seen where there is little smoke, so that there must be some connection between smoke and fogs of this sort. We did not notice the paragraph in the *Lancet*, but as you report it, it seems decidedly opposed to all the evidence.—W. H. H. SOAMES. I think, if you consider the matter carefully, you will see that whatever unfairness there may seem in my reply arose from your own departure from a rule which has been laid down, after very careful consideration, for the guidance of contributors and correspondents. You must know well that a very large proportion of the men of science of our time regard the account to which you refer as only a well-meant but utterly erroneous attempt to explain some of the mysteries of the universe. An astronomer like Sir George Airy says the geology may do pretty well, but "the astronomy is quite wrong"; a great geologist thinks the astronomy may be right, but the writer certainly knew little about the earth's crust; and so in every single branch of science which can be named. Again, it is certain that while they think that way, many estimable persons, and some of them scientific, too, think differently. The former may be quite wrong, and these latter right. Or you may be right in the extreme view you take, that not only is the account correct, but that it was intended to enlighten men as to scientific matters, and that we ought, therefore, to take the apparently plain statements in the account as part of our working material. But whoever may be right or wrong, or whether all are right in some degree, and all in some degree wrong, has really nothing to do with us. We simply decline to have inconsistencies asserted here, or attempts at harmonising made here. We want to get at scientific truth, by scientific research, observation, and experiment, and in no other way. If you are right, and the account which you deem plain (but many do not) is correct, it is absolutely certain that the views to which we shall be led by the, perhaps more roundabout, perhaps more direct, route of scientific inquiry, will agree with that account in the long-run. If the way really is longer, the exercise will do us all good.—A REMONSTRANT. When you wrote saying we had nothing but astronomy, you must have been trying some of the things which writers on Brain Troubles describe as causing mental hallucinations. We have before us the contents of Part IV., and we find, besides correspondence, notes, mathematics, whist, and chess, no less than thirty-two non-astronomical subjects. What can you mean?—J. HARKINSON. You tell me (I fancy I have heard it before) as bearing on the inferiority question, that "a woman may not be able to sharpen a pencil or throw a stone at a hen, but she can pack more articles into a trunk than a man can." Do you refer, in a roundabout way, to tight-lacing?—MORFIT. More fit for others than for us. What is new is not strictly true. You incorrectly define clouds as a collection of watery particles in the state of vapour, then correctly defining vapour.—P. A. FOTHERGILL. Thanks for very pleasant letter. The Petersburg problem is one of the most perplexing problems known. I believe I took the logically correct view in the old discussion; but if I were asked what I would pay for the chance, you may depend I would *not* offer what may be proved to be the just price, viz., infinity. It would be a very interesting subject for discussion.—ALDEBARAN. Putting x as the distance of the earth from sun after time t, we have

$\frac{d^2x}{dt^2} = -\frac{G}{x^2}$ where G = sun's gravity at unit of distance. Hence, multiplying by $2\frac{dx}{dt}$ and, integrating, we have $\left(\frac{dx}{dt}\right)^2 = C + \frac{2G}{x}$

Now when $x = D$ (the sun's distance), $\frac{dx}{dt} = 0$. Hence $C = -\frac{2G}{D}$, and therefore,

$$\left(\frac{dx}{dt}\right)^2 = 2G\left(\frac{1}{x} - \frac{1}{D}\right)$$

Integrating this you will find

$$t = \sqrt{\frac{D}{2G}}\left\{\sqrt{Dx - x^2} - \frac{D}{2}\text{vers}^{-1}\frac{2x}{D} + C^1\right\}$$

And since when $t = 0$, $x = D$, $C^1 = \frac{D\pi}{2}$

we have then, when $x = 0$ (that is, neglecting the slight difference of time between the earth's reaching the centre and the surface of the sun)

$$t = \sqrt{\frac{D}{2G}}\,\frac{D\pi}{2}$$

If you put in this the correct values for D, G, and π, you will get a result very near Young's. You have D = 92,885,000 miles, G = 324,000 g. (Earth's radius)^2 (roughly), where g = terrestrial gravity at Earth's surface; and we must reduce all the distances to feet, put g = 32·2, and then t will be given in seconds.—HALLYARD. I received your long paper, and preserved it, proposing to return it when stamps should be sent. Because, it really was too long "for any use," even if my "plan were that of the E. M., so that I might be glad of "a humble paradoxer or two to pad." You certainly did not in any way offend by discussion in 1877. By your own account I was the offender. If, indeed, I snubbed you "in a way no undergrad would stand from a don," I can assure you it was quite unintentional. Perhaps in those days I did not so well know the proper course to pursue. What I meant for good-humoured fun was mistaken for sarcasm, which is, in truth, quite out of my line. I agree with what Dickens says in one of his letters (I think), that it will not do to adopt a tone which might even be mistaken to signify, See how clever I am, and what fun everyone else. Thanks for note about the zodiacal light. I have never seen it well in England. I saw it very well in Bloomington, Indiana, in the spring of 1880. The atmosphere could not under any circumstances act as a telescope to enable you to see Venus as a crescent. More probably some atmospheric peculiarity distorted Venus into apparent crescent form.—H. B. SHAW. Many thanks. Your suggestions seem excellent.—H. WILLIAMS. Sorry any paper remained unacknowledged. We

insert your reply, rather shortened.—TOURIST. Question why Irish outside cars are so peculiar to Ireland, and when they were invented, scarcely scientific. It is noteworthy, by the way, how definitely national tastes seem often to determine favourite forms of vehicle. The outside car seems singularly appropriate to Irishmen (still more, perhaps, to Irish girls). ED. C. TOUNE. In their present form your theories would hardly suit the pages of KNOWLEDGE.—CHARLES GROVER. Thanks, but except the greater distinctness of satellite II.'s shadow (as compared with I.'s) your note scarcely warrants insertion, now that interesting configuration is passed. We shall be glad to hear further from you, especially with reference to the great markings on Jupiter.—A. BARTHÉLEMY. Your queries are for a general literary paper, not for one whose chief object is science. Think you will have some difficulty in finding biography of Lady Austen (Cowper's friend). Biographies of Poe numerous; good sketches of George Eliot's life, &c., in the *Nineteenth Century* (last November, I think). —G. C. D. M. Astronomers know very little about the origin of rotatory motion in nebulous masses; but the general idea is that it arose somewhat as eddies arise in a stream. If two nebulous masses met under their mutual attraction, there would be a whirlpool motion, unless they met precisely full, which would be very unlikely. It is pleasant to be asked for more astronomy; but you can understand not only why I do not bring it to the front, but why I prefer to invite others to write astronomical articles. As you wish me to answer, myself, your question about variable length of day, I ask you to allow me to reserve it, hoping very soon to give an answer, illustrated as you suggest. If I insert it now as a query, I shall have a number of answers, some excellent, others, perhaps, not quite so well, and correspondents who have been at the pains to formulate a reply may not be very well pleased to see their labour wasted. The moon does not "lie on her back" at every lunation; she only does so when her path—soon after "new"—is carrying her north of the equator; for then, when a crescent, she is on the western sky as nearly above the place of the sun below the horizon as she can be in our latitudes. The configuration is pictured in my book on the "Moon."—VOLCANO. We really must not insert queries relating to medical matters in KNOWLEDGE. We have many medical men among our readers, and correspondents who would, no doubt, reply to them; but others might reply without the necessary information. Then your question is too vague; heat spots on the forehead, and why (you ask) on the forehead? If they are heat spots, cooling medicines ought, one would say, to be good.—W. GRANDY. See answer above to G. C. D. M.—SIR E. B. Phrenology would never have been suggested, I apprehend, if there were not reason for associating particular confirmation of the skull (apart from external influences) with particular mental or moral qualities. A "phrenologia sana" such as that we may admit. It is the theory that beneath the "bump" places are the cerebral organs of those affections or qualities, which is rejected by anatomists. I cannot quite agree with you that "if a single man in the world can give true judgments of character from heads, all the anatomists in the world cannot prove phrenology wrong." A hundred correct judgments would not do so much to establish phrenology (in the form in which Gall and Spurzheim advanced it) as a single failure would do to negative it. The way in which failures (you admit that most of the phrenologists failed sometimes) are explained by the advocates of that system belongs simply, as Wendell Holmes well puts it, to the system "heads I win, tails you lose."—BISHOP. Like Gilbert's "dancing man," I would answer you, "right reverend sir, in half-a-crack," if it were altogether fair; but with Browning, Dollond, Wray, and other thoroughly excellent opticians, to choose between, I could not recommend any as *the best* maker you could go to for a 3-inch telescope.—C. S. BENTLEY. Thanks. Will review if space permit. You attribute to Isaac Walton the saying about strawberries (page 338 KNOWLEDGE), referred to Cotton Mather (Walton's "Complete Angler," chap. 5, page 109, Chatto's edition, p. 114; Simpkins). They tell the story in America of Mather.

[Two columns of Answers to Correspondents crowded out.—P.D.]

NOTICE.—For "Nos. 2 and 3 are *not* out of print," in Answers to Correspondents, No. 17, read "Nos. 2 and 3 are *now* out of print." Part I. is now entirely out of print. Those who wish to complete the series would do well to get the Parts which are still in print, and to add their names to the list of applicants for Part I., so that, should any copies be returned, they may be distributed in due order.

Letters Received.

J. Rae. Lepidodendron, answered. Also by F., H. Courtenay, W. B. K., J. M. Carr, W. D. C., Geo. Biddell, J. S. Thorne, Harvard, Colonial, J. W. Emery, T. K. Snood (?), L. Empson, J. E. T., M. Prang, E. R. T———n, Ellicott, Petersburg, Ante-Prandial, Jorkins, L. M. S., Fraulein, A. Accrington, Emeritus, Peter Parley, S. Pritchard, M. Hosking, L. Hoare, Surbiton, Tricycle, Medicus, N. Hatherley, General Lambert, M. Soper, Pro-di-gious (very), Duncan D., Professor, Baptist, L. S. P., Half-Sprung.

Notes on Art and Science.

THE language of the Seychelles is a curiously corrupt French, in some respects similar to *argot*. Thus a common expression among the natives is *Moi ne cont pas*, for *Je ne connais pas*; similarly, they frequently interpolate a medial vowel, and say *gelisser* for *glisser*, *belouse* for *blouse*, and so on. These singular linguistic peculiarities deserve the attention of philologists, for no explanation of them has yet been given, except the purely negative one that no mixed race ever retains purity of tongue; but although this axiom accounts for the existence of many mixed languages—notably our own—it does not explain how the pure French of the original aristocratic exiles became degenerated into a tongue similar to that which is popularly supposed to be spoken only at young ladies' academies and by English tourists on the Continent. —*Graphic*.

A FOG BOW BEFORE SUNRISE.—The phenomenon of the ordinary rainbow is familiar to every observer of nature. White fog bows, or "fog eaters," as they are called by sailors, are frequently visible in localities favourable for their formation; and they are generally regarded as indications of clearing weather. A fog bow was observed, writes Mr. H. C. Hovey, on the morning of Jan. 8, from my residence on Fair Haven Heights, near the estuary of the Quinnipiac River, and about 100 ft. above the sea level. No rain was noticeable in any quarter, but the valleys were filled with fog, above which the hill tops stood like islands. At exactly ten minutes before sunrise (due at 7.26 a.m.), on looking north-west I saw a brilliant arch of prismatic colours spanning the East Rock Range, the highest point of which is 350 ft. above the sea. As the sun arose, the arch diminished in height and vividness, and by the time the orb was visible in the morning sky, the fog bow had vanished.—*Scientific American*.

WARMING ROOMS.—"S. S." asks me how to warm a room 13 × 13 ft., which has no chimney or any outlet for stove-pipe. I cannot tell how to do this satisfactorily. If obliged to occupy such a room, I should economise my own animal heat by wearing a thick top-coat, double woollen socks, &c. I have warmed a small conservatory, requiring merely protection from night frosts, by burning a few common, cheap paraffin lamps, distributed so as to equalise the temperature. A gas-stove would have killed the more delicate plants; the difference is due to the fact that the mineral oil is so pure a hydro-carbon that it produces only water and carbonic acid by its combustion, while the gas contains bisulphide of carbon and other impurities, which, by their combustion, produce irritant or actively poisonous compounds, to which the plants are more sensitive than we are. If "S. S." uses such lamps, or one of the paraffin stoves sold for the purpose, he should place them on the floor, or as low as possible, in order to economise their heat. Each ordinary lamp will give him about as much heat and carbonic acid as a human companion.—W. MATTIEU WILLIAMS.

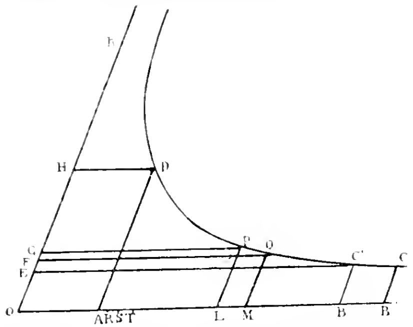
NOVEL HELIOMETERS.—At a recent meeting of the Royal Dublin Society, Mr. Howard Grubb, F.R.AS., described some novel heliometers which are at present in process of construction in Dublin for the Belgian Government, to be used at the coming transit of Venus, the design of Professor Houzeau, the Belgian Astronomer-Royal. A heliometer is generally made from a single objective cut in two, with mechanical arrangements for traversing one-half with respect to the other. In Professor Houzeau's arrangement the two half-objectives are of very different foci—one of about 14 feet, the other 6 inches only, but so placed that both form their image on the same plane. As the apparent diameter of Venus and the sun are about as 28 to 1, it follows that the image of Venus, as formed by the large objective, will be about the same size as that of the sun formed by the small objective, and, consequently, coincidences can be made, not by bringing the limb of Venus to touch that of the sun, or a micrometer line, but by superposing the image of the sun as formed by the small objective on the very slightly larger image of Venus, as formed by the large objective, and thus it is hoped that all the inconvenient and perplexing phenomena of irradiation, "black drop," &c., will be completely eliminated. Mr. Grubb exhi-

lated several portions of the instrument, and mentioned the various difficulties likely to be encountered, and the means provided for dealing with them.

Strength of Materials.—At the late fair of the Massachusetts Charitable Mechanic Association, at Boston, examples were shown of tests of materials made by the machine lately erected in the United States Government Arsenal, at Watertown, for the proving of structures of full working dimensions. A steel wire cable, 1¼ inches diameter, was shown, which had withstood a pull of 75 tons, when the fastenings by which it was held gave way, although the cable itself remained sound. A hammered iron bar, 5 inches in diameter, was shown to have concealed a crystalline formation of the fibres, and it consequently parted with a loud report under a strain of nearly 723,000 lb., or 36,900 lb. to the square inch. A smaller wrought-iron bar drew down and broke with a fibrous structure under a pull of 51,340 lb. per square inch. Some pine-wood columns were also shown which had been tested by compression. The first of these, originally 12 feet long, yielded at a pressure much below its estimated strength, in consequence of a large knot in the side, which acted as a comparatively incompressible wedge. Another column was a spar 12 feet long, 7¼ inch butt, and 6¾ inch top. This stick was a perfect sample, and gave way by splintering at its smaller end. A seasoned hard pine girder, 11 inches square and 10 feet long, bore a load of 751,000 lb. —*Scientific American.*

Our Mathematical Column.

Find the area intercepted between the hyperbola, an asymptote, and two ordinates parallel to the other asymptote.



Let OAB, OHK be the asymptotes; AD, BC drawn parallel to OK to meet the curve in D, C. In OAB take OR greater than OA, but so that AR is very small; take points S, T .... L, M ... &c., so that OA : OR :: OR : OS :: OS : OT .... OL : OM ...., and let B' be the nearest of such points to B, so that B'B is small. Suppose that OB' is thus divided into n parts AR, RS, ST, &c. ....., and let LM be the $(r+1)$th such part. Draw LP, MQ parallel to OK to meet the hyperbola in P and Q, and draw DH, PQ, QfF, C'E parallel to OB, QF meeting PL in f. Then, since OA : OR :: OR : OS :: OS : OT, &c. ....., and that LM is the $(r+1)$th of the parts AR, RS, ST, &c., *i.e.*, M is the $(r+1)$th of the points of division R, S, T, &c.

$$\mathrm{OA} : \mathrm{OM} :: \mathrm{OA}^{r+1} : \mathrm{OR}^{r+1} \qquad \text{(i)}$$

and similarly $\quad \mathrm{OA} : \mathrm{OL} :: \mathrm{OA}^{r} : \mathrm{OR}^{r}$

$$:: \mathrm{OA}^{r+1} : \mathrm{OA}.\mathrm{OR}^{r}.$$

Hence $\quad \mathrm{OA} : \mathrm{LM} :: \mathrm{OA}^{r+1} : \mathrm{OR}^{r}.\mathrm{AR}$

But $\quad \mathrm{DA} : \mathrm{MQ} :: \mathrm{OM} : \mathrm{OA} \qquad \text{since } \mathrm{OA}.\mathrm{AD} = \mathrm{OM}.\mathrm{MQ}$

$$:: \mathrm{OR}^{r+1} : \mathrm{OA}^{r+1} \qquad \text{from (i)}$$

Thus $\quad \mathrm{OA}.\mathrm{AD} : \mathrm{LM}.\mathrm{MQ} :: \mathrm{OR} : \mathrm{AR}$

or parallelogram $\quad \mathrm{OD} : \text{parallelogram } \mathrm{LA} :: \mathrm{OR} : \mathrm{AR}$

i.e., all the parallelograms inscribed as LQ is are equal: thus parallelogram OD : sum of inscribed parallelograms :: OR : n AR

But sum of parallelograms = ADC'B' = ADCB ultimately, when AR is taken indefinitely small.

$$\text{Hence ADCB : parallelogram OD} :: n.\mathrm{AR} : \mathrm{OR} \qquad \text{(ii)}$$

But ultimately $\mathrm{OA}^n : \mathrm{OR}^n :: \mathrm{OA} : \mathrm{OB}$, *i.e.* $\left(\dfrac{\mathrm{OR}}{\mathrm{OA}}\right)^n = \dfrac{\mathrm{OB}}{\mathrm{OA}}$ or $n \log.\dfrac{\mathrm{OR}}{\mathrm{OA}}$

$$= \log.\frac{\mathrm{OB}}{\mathrm{OA}}$$

$$\text{Thus } n = \frac{\log.\dfrac{\mathrm{OB}}{\mathrm{OA}}}{\log.\dfrac{\mathrm{OR}}{\mathrm{OA}}} = \frac{\log.\dfrac{\mathrm{OB}}{\mathrm{OA}}}{\log.\left(1+\dfrac{\mathrm{AR}}{\mathrm{OA}}\right)} = \frac{\log.\dfrac{\mathrm{OB}}{\mathrm{OA}}}{\dfrac{\mathrm{AR}}{\mathrm{OA}}}$$

$$= \frac{\mathrm{OA}}{\mathrm{AR}} \text{ ultimately } \log.\frac{\mathrm{OB}}{\mathrm{OA}}$$

Hence $n.\mathrm{AR} = \mathrm{OA} \log.\dfrac{\mathrm{OB}}{\mathrm{OA}}$, and (ii) becomes

$$\mathrm{ADCB} : \text{parallelogram } \mathrm{OD} :: \mathrm{OA} \log.\frac{\mathrm{OB}}{\mathrm{OA}} : \mathrm{OR} \text{ or } \mathrm{OA}$$

since OA = OR ultimately

$$\therefore \mathrm{ADCB} = \log.\frac{\mathrm{OB}}{\mathrm{OA}} \text{ parallelogram } \mathrm{OD}.$$

Editor.

25, p. 307. There appears to be an error in your solution of this question. You have taken the number of months as 29, instead of 120, £$\frac{6}{5}$ for £$\frac{7}{6}$.

The following appears to be a solution:—If r = the rate of interest per cent. per month, then r has to be found from the equation—

$$\frac{1-(1+r)^{-120}}{r} \times 1\cdot16 = 100.$$

$$\text{or } \frac{1-(1+r)^{-120}}{r} = 85\cdot7143.$$

It will be found by logarithms that ·005918 is a very near value of r in this equation. Hence £·5918, or 11s. 10d. *per cent.* per month is the rate of interest realised.—J. McGowan.

[Query No. 200, p. 278]—Leases.—W. Cahill's query in No. 15, in connection with James Gregg's previous query:—

	£	Decimals of £.
Assuming the interest to be 5 per cent., the premium of £1,050 paid at commencement would amount at the end of 14 years to	2,078	928
An annuity of £250 would amount at the end of 10 years to.. .	3,144	173
The interest on same for 4 years (to end of the 14)	677	657
An annuity of £300 would amount at the end of 4 years to	1,293	038
Total value of all at end of 14 years	7,194	096
The present value of this amount being	3,633	5
Which would buy an annuity or the lease at a peppercorn rent of	367	075
Deducting the £300 rent paid each of the last 4 years, the premium to be paid at the end of 10 years is the present value of 4 years' annuity of £67·075, which amounts to	237	815

—J. W. *The Answer.*

Our Whist Column.

By "Five of Clubs."

AN ILLUSTRATIVE GAME.

Clay, in his charming little treatise on "Short Whist," gives the following interesting instance of the danger of continuing a forward game, when early indications show that the promise of a great score was fallacious. (In passing, one may note that in cases such as this information of weakness may prove exceedingly useful to the stronger partner, by showing him the necessity of caution; it is in this respect that the ordinary game differs from dummy play, when the danger is indicated at once. Some dummy players are apt to overlook this negative advantage of intimation of weakness in partner's hand, and to consider only the more obvious positive advantage which necessarily accrues to the adversaries):—

"I dealt," says Clay, "and turned up a Queen, along with which I held two small trumps. My partner—nor was he a bad player—held the Ace and four of the smallest trumps, and, so to speak, the whole of another suit. With this strength, assisted by my Queen, he promised himself, reasonably enough, a great score, if not the whole game. But the first two tricks showed him that he would be overtrumped. He should have submitted to this, and as it happened he could have made a good score, but he was unable to dismiss the idea of a strong attack. He trumped the second trick with his Ace, led a trump,—and we made no other trick. Thus with Ace, Queen,

eight trumps, five of which were in one hand, between us, we lost twelve tricks out of the thirteen."

The following game is made up to show how this might happen :—

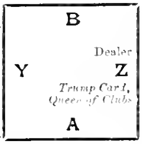
THE HANDS.

A.

Clubs—K, 10, 9, 8.
Hearts—A, K, 10, 9, 4, 2.
Spades—10, 3, 2.
Diamonds—none.

B.

Clubs—Kn.
Hearts—Kn.
Spades—A, Q, 8, 7, 5, 4.
Diamonds—5, 4, 3, 2.



Y.

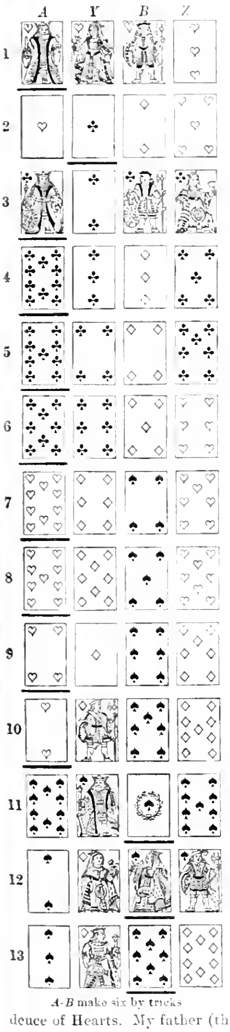
Clubs—A, 6, 4, 3, 2.
Hearts—Q.
Spades—K.
Diamonds—A, K, Q, Kn, 8, 6.

Z.

Clubs—Q, 7, 5.
Hearts—8, 7, 6, 5, 3.
Spades—Kn, 9.
Diamonds—10, 9, 7.

S— Love all.

NOTE.—The underlined card wins trick, and card below it leads next.



A-B make six by tricks

REMARKS AND INFERENCES.

1.—*Y* knows that *B* is not playing a false card in his (*B's*) partner's suit, so that *B* will be able (probably) to over-trump second round.

2.—*A*, of course, continues his suit. If he did not know that *B* can trump the suit, he would not force the adversary, being himself strong in trumps. His play should suggest to *Y* that *A* is strong in trumps, and he should give up the line of play he had intended to follow. He must yield to the force; if he declines, he will be forced again next round, and must either yield then under less favourable conditions, or let *Z* ruff, who must be weak in trumps. It is better to throw the lead at once into *Z's* hand. If he had done this, *Z* would have made the second trick with trump, have played the Ace of Spades, and then forced *Y* with the Queen. *Y* might then have led Diamonds, in order to force *A* (which, as it happens, would come off in the first round) leaving *A* either to lead trumps under unfavourable conditions, or to force *Y*, which *Y* could accept, being able to force back with his Diamonds, when *Z* would be left with length in trumps. As it is, *Y*, after throwing away the commanding card in trumps, is absolutely powerless.

3.—The rest of the hand plays itself. *Y's* discard of the Diamond Ace at trick 9 is intended to show his partner that *Y* has entire command of the Diamond suit, but *Y* gets no chance of leading Diamonds or any other suit.

HE THAT WILL NOT WHEN HE MAY, &c.—The following singular combination of cards is worth recording, as it may be made to point a moral. It came under my observation at the Portland, Clay and my father being partners. The game was four-all. The dealer turned up a small heart. Clay led a Diamond. The second hand had Ace, King, Queen, Knave, ten, nine, and two, of trumps. With these cards, the problem is how to lose the odd trick.

The second hand contrived it in this way. He had no Diamond, and trumped the card led, with the deuce of Hearts. My father (third hand) also had no Diamond, and only one trump, the three with which he overtrumped. In the end, the holder of the sixième major only made his six trumps, his adversaries having six winning cards in the unplayed suits, which neither of the opponents could trump. They therefore lost the odd trick and the game. Had the second player (*B*) trumped with the nine originally, he must have won the game, however the cards lay. For, his partner being dealer, held the trump card, and consequently *B*, by then leading trumps, must make seven tricks, even if all the remaining trumps are in one hand against him. No doubt *B* regarded the chance of the third hand's having none of the suit in which he himself was void as practically *nil*. Nevertheless, he might have made the game practically sure.

The moral is: Never throw a chance away.

"CARD TABLE TALK," "CAVENDISH."

ALL THE TRUMPS IN ONE HAND. A correspondent (J. Heaton, Surrey) asks what are the odds against all the trumps falling in the dealer's hands, and whether it has ever happened. Two cases were recorded a few years ago in the *Westminster Papers* (we will look the case up), and the editor made the remark that this showed mathematicians to be wrong in stating that the odds were, in round numbers, 159 thousand millions to one against such an occurrence. We cannot see it. It would not be very much out of the way to suppose that among all the whist-playing nations of the earth a million whist-parties play per diem; and, say that in each case there are twenty deals. Then it would require only 7,950 days, or not much more than 20 years, to give 159,000,000,000 trials, which, of course, would give an even chance that any particular hand would be turned up once at least. [This is not quite correct, there are two possible results in tossing a coin, but it does not require two trials to give an even chance of tossing head once at least. Evidently my papers on chance should soon be started. Let me note that the exact odds against the dealer having thirteen trumps are

158,753,389,899 to 1.

Pretty long odds.—ED.] The odds against the occurrence must, we should think, be diminished by the circumstance that when a ruffing game has been played, there are several cards of the same suit arranged one in each of several sets of four cards, after tricks are gathered. Supposing them to occupy the same position in each set, which might readily happen, that there is very little shuffling, and that the same suit is trumps in the next hand, it will easily be seen that four or five trumps might be already *en train* to fall to dealer, so that the chance of the remaining trumps falling to him alone would have to be considered. [Say the chance of this happening in the case of five trumps, besides the turn-up card were only 1-1,000. There are thus 20 cards disposed of in the five tricks supposed to have come together, in this special manner, in dealing. There remain 32 cards, one of which is the turn-up. Out of the 31 cards, 7 are trumps, and form one set of 7 out of

$$\frac{31 \cdot 30 \cdot 29 \cdot 28 \cdot 27 \cdot 26 \cdot 25}{1 \cdot 2 \cdot 3 \cdot 4 \cdot 5 \cdot 6 \cdot 7}$$

possible sets of 7, or 2,629,575. Hence the chance of both events coming off and all 13 trumps falling into one hand is one-2,629,575,000th, or the odds only 2,629,574,999 to 1 against the event.—ED.]

FIVE OF CLUBS.

A CORRESPONDENT ('Why') asks whether certain whist rules presented in doggrel rhyme are sound as far as they go. "They appeared in *London Society*, he says, some time ago, and were said to have been copied from some provincial club wall." They are Pole's, and are sound as general rules. But scarce one of them may not on special occasions be departed from with advantage. Supposing, for example, you want the odd trick to win, and have five small trumps, viz., one four-card weak suit and two suits of two cards each. It would be absurd in this case to follow Pole's rule respecting trumps—"When you hold five" 'tis always right to lead them.

FIVE OF CLUBS.

J. TOMLINSON.—Surely by not leading trumps when he gets the chance, *Z* shows unmistakably that he has not been wanting trumps led, and therefore he has not signalled.

GRADATIM.—Yes: from Ace, six others in trumps lead Ace. The lead of Queen from Queen, Knave, nine, and others (three others you specify) is now generally rejected. Hoyle advised it, with the object of finessing the nine, on the return of the lead. This might do in long whist, but not in the game as now played.

FIVE OF CLUBS.

W. F.—In Problem 1. *B* leads trumps fourth round, because his partner, not knowing what *B* knows, would be at a loss how to play after making the successful finesse in Diamonds. If he continued the Diamond lead, *B* would have to lead from his tenace in Hearts. The lead of trumps manifestly puts *Y* at a disadvantage. He must

lead either through B's tenace in Hearts, or up to A's tenace in Clubs. He also, as you say, requires information about honours.

A Strange Return Lead. A leads a small card, which B (second hand with Ace King, Knave) wins with Knave, and immediately returns King, which A (B's partner) trumps. At the end of the hand B has the effrontery to ask A why he trumped his King. Might not A have retorted with more reason, why did you put on Knave?

W. F. With much better reason. B said, as plainly as whist language can speak, "I hold the King only, and play it so that, if you have the Ace, you may save it." But why did you head your letter "an omitted case where Ace should be led before King"—this is a return lead, and I have considered no return leads at present.

Five of Clubs.

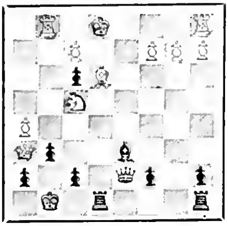
Our Chess Column.

Endings from actual games played by Mephisto:

No. 23.

Amateur.

White.



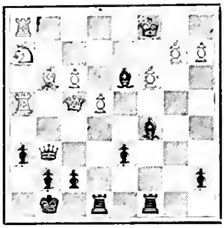
Black.

Mephisto.

No. 24.

Amateur.

White.



Black.

Mephisto.

Mephisto won as follows:—

No. 23.

	B. to Kt.5(ch.).
P. to B.3 (a).	Q.R. to Q.sq.
Kt. to K.2 (b).	Q. to Q.6.
Kt. to Q.4.	R takes Kt.
Resigns.	

(a) If K. to B.sq., then Q.R. to Q.sq. Now, if Kt. to Q.3., Q. to R.6 wins; or, R. to K.sq., Q. to Kt.4(ch)., Kt. to Q.3. R. takes Kt., wins.

(b) Black threatens

	K. to B.sq.
Q to Q.7(ch).	Q. to Q.8(ch).
R. takes Q.	
R. takes R., mate.	

No. 24.

	Q. takes B.
Q. takes Q. (a)	B. to K.6(ch).
K. to Q.sq.	Q.R. takes B.P.
P. takes R. (b)	R. to Kt.sq.
Resigns.	

(a) Had Amateur seen his danger, and not taken the Queen, then Mephisto would win by being a piece ahead.

(b) This was unexpected: he must take the Rook.

SOLUTION.

Problem No. 17. End Game by B. Horwitz, p. 330.

1. Kt. takes B.P. / P. takes Kt.
2. P. to Kt. 6. / Q. to Kt.2. (a)
3. K. to Q.7. / K. to Kt.sq. (a)
3. K. to K.6. / Q. to R.sq., or K. to R.sq.
4. B. takes P. / and wins.

(a) If Q. to Kt.sq., B. takes P., wins.

CHESS BY CORRESPONDENCE.

Mr. M. J. Harding has set the ball rolling. Having at his request inserted a notice to play by correspondence in No. 16, we have received about a score of replies, and, as "our true intent is all for your delight," we shall continue to introduce the players to each other, for the benefit of Chess and of the State revenue. Letters received from

A. B. Palmer	G. Woodcock
H. T. Holden	R. G. Brothers
A. Woverton	A. F. Baker
"Fusee"	W. G. Jones
Chas. W. Tilbe	H. Percival
G. Priestman	R. S. Tulloch

whom we have paired in the above order.

Of course, we cannot be held responsible for introducing the Lamb to the Lion, as we do not know the strength of our correspondents. Should, however, two Lions meet, and a good game or position result, we shall be happy to publish the same.

We make a beginning this week, and publish the moves of two games between Chief Editor and Chess Editor.

Remove Black's King's Bishop's Pawn. Pawn and two moves.

GAME I.

Chief Editor.	Chess Editor.
1. P. to K.4. 2. P. to Q.4. 3. P. to K.B.4.	2. P. to K.3.

GAME II.

Chief Editor.	Chess Editor.
1. P. to K. 4. 2. P. to K.B.4. 3. P. to Q Kt.3.	2. P. to K.3.

ANSWERS TO CORRESPONDENTS.

*** *Please address Chess-Editor.*

Postcard.—We know Staunton's "Companion, 1849," but you cannot call that a "modern treatise"; some of its merits have been "improved away."

J. F. B.—You must not move your King into check. The principle is, that the player who captures his opponent's King first has won the game.

G. W.—Remark about ending correct.

H. Percival.—The games in the Berlin Tournament have not "all" been published.

Henry Planck.—Solution of No. 20 correct.

R. S. Standen.—Solution of No. 14 correct.

L. E. Whitely.—Problem 20. The first move is not 1. R. takes R.P., but 1. B. to R.4(ch.).

A. B. Palmer.—Solutions correct.

Leanard P. Rees.—Solutions correct. Problems received with thanks, and will receive our attention.

G. Priestman.—Problem No. 21 incorrect. 1. B. to R.4 (ch.).

D. Cudmore.—Solutions correct.

Contents of Knowledge *No.* 17.

NOTICES.

The Back Numbers of Knowledge, with the exception of No. 2 (Nov. 11, 1881), and No. 3 (Nov. 18, 1881), are in print, and can be obtained from all booksellers and newsagents, or direct from the Publishers. Should any difficulty arise in obtaining the paper, an application to the Publishers is respectfully requested.

The following Monthly Parts of Knowledge are now to be had (Part I. being out of print):—

Part II.—(December, 1881.) Containing five numbers. Price 1s. Post-free, 1s. 2d.

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KNOWLEDGE
AN ILLUSTRATED
MAGAZINE OF SCIENCE
PLAINLY WORDED-EXACTLY DESCRIBED

LONDON: FRIDAY, MARCH 10, 1882.

CONTENTS OF No. 19.

TRICYCLES IN 1882.

BY JOHN BROWNING

(Vice-President of the Tricycle Association).

ABOUT five years since the first modern tricycle was built on the lines of the spider bicycle. This new build, known as the Lever Coventry, was regarded with amused curiosity by the general public, and with utter contempt by most bicyclists; yet it wanted only a strap-brake—that is, a band working on a drum—to make it a thoroughly safe and efficient tricycle.

Soon after this, Starley, the Stephenson of the tricycle, who contrived the Lever Coventry, produced the Salvo. This machine had a rotary motion obtained by means of cranks, as in the bicycle, which was communicated by means of a chain to the driving wheels.

But the great peculiarity of the Salvo was that both wheels were driven so long as the machine was moving in a straight line, while either wheel was free to stand still, or even run backwards, when the machine was turning. There are now at least 150 tricycles, or rather machines known by that number of different names, but these are principally of three or four types, and all those belonging to the largest and possibly the best type, are modifications, sometimes only in name, of the "Salvo."

Less than two and a half years ago the two principal tricycle clubs of the metropolis, the London and the Finchley, arranged a fifty-mile ride for the Tricycle Championship. This was ridden over the hilliest road out of London in four hours fourteen minutes. The rider, Mr. Lacy Hillier, is the amateur champion of the bicycle and tricycle. The machine he rode is known as the Humber, and it was brought out for this ride. Looked at sideways, it has the appearance of a bicycle; but it may be described as a bicycle with two front wheels. The rider sits on a saddle between them, and steers by means of a cross-bar which turns them both. This is probably still the fastest tricycle, but it requires some practice to ride it at all well. It is not a good luggage carrier, it will not turn easily or in a small circle, and it requires careful riding, particularly down hill. For these reasons it is not so generally adopted as its good qualities deserve it should be.

About twenty or thirty novel tricycles have been introduced this year. Of these about a dozen possess great originality, and three or four considerable merit. The Rucker, the National Arms Company's National Tricycle, the Improved Omnicycle, the Improved Devon, the New Rotary Coventry, and the Monarch, are, in my opinion, the best machines, the Monarch being the most original brought out this season. The whole of the machines I have named, with the exception of the Coventry, are double-drivers. Of these the Rucker and the Monarch have the small steering wheel behind, and thus are perfectly open in front. I do not say that such machines are by any means less liable to accidents than those which have the steering wheel in front, but if accidents should occur, they are not likely to have such serious results, as the rider may jump out or fall out of the machine without falling on to or being caught by the steering gear or front wheel.

About two years since, most of the tricycles which were driven by means of a chain were geared-up—that is, the driving wheels were made to go round faster than the pedals. In some letters I then wrote to the *Cyclist*, I asserted that this was a mistake. Now, most of the best riders agree that tricycles should be geared down—that is, the pedals should go round faster than the wheels. In such an arrangement, of course, power is gained and speed is lost. When the gearing down is carried to a great extent, the pedals make two revolutions for one revolution of the driving-wheels. A machine so geared can be ridden up a steep hill easier than a bicycle. The chain is a source of trouble in a tricycle, though it has been improved of late. It is liable to stretch and so slip over the cogs, and I believe, occasionally it has brought a machine to a dead standstill for an instant, by not passing round with the cogs freely. The result of this has been that the rider has been thrown out of the machine. Some tricycles have two chains—one to each of the driving-wheels; these, with the exception of the Monarch, are the only true double-drivers, and are the best for mounting hills. The machines which profess to be double-drivers, through the inter-mediation of what is called a balance action, are double-drivers only so long as the resistance to both wheels is the same. As soon as one wheel experiences more resistance than the other, it ceases to drive, just when its driving power is the most wanted to overcome an obstruction.

Probably the best size for the driving-wheels of a tricycle is from 48 in. to 50 in. diameter. If smaller than this they rise with difficulty over any small inequalities in a rough road. If larger, unless very stoutly made, they are weak, and if strong they make the machine unnecessarily heavy. The weight of tricycles with from 48 in. to 50 in. wheels varies from 75 lb. to 120 lb., but very few are less than from 95 lb. to 98 lb. This weight is more than it should be for machines intended for riders weighing from 8 to 10 stone.

Manufacturers ought to make machines of different degrees of strength and weight. At present, all machines are, as a rule, strong enough to carry a man who weighs 16 stone. There are, however, two new machines this season which are excellent in this respect. The National Tricycle Co.'s machine weighs only 70 lb., and the Monarch only 63 lb. Until the Humber Tricycle was produced, tricycles were of very inferior workmanship to bicycles; but that machine, the Cheylesmore, the Coventry Rotary, and the Premier are now of first-rate excellence.

Generally speaking, the worst points of a tricycle, both as regards contrivance and workmanship, are the pedals, and

these require to be the best. The whole weight of the rider is thrown on these in riding up hill, and the friction of badly made bearings is very great. I have seen such bad workmanship in the pedals of otherwise fairly well made machines, that an ironmonger would have been ashamed of such work in a set of the commonest kitchen fire irons.

At present there is only one good brake. This is a circular or semicircular band of steel, which, by means of a lever, can be made to clasp a drum. It is in a few instances, and should always be, applied by means of two straps and drums and to both wheels. If one band or drum were to give way in descending a hill, the other would then suffice to prevent an accident.

Great improvements have been made recently in double tricycles, or, as they are generally called, Sociables. Last year the Sociable Salvo was the best machine of the kind, but now the Premier Sociable is both lighter and a far higher class of work. Machines of this kind, to carry two riders side by side, need not weigh nearly double the weight of two single machines of the same make. From being much wider, they are safer than single machines, and they run lighter than single machines when they are driven by two practised riders. A better speed can be obtained on them than on a single machine. A good rider can carry a lady on the seat beside him with very little assistance from his fair companion. Indeed a problem has been proposed: Given a lady and gentleman driving a Sociable, to find the amount of the pressure the lady puts on the pedals?

But either with a lady or a gentleman, riding on a Sociable is by most experts admitted to be the most enjoyable form of this the most enjoyable of all sports.

THE GREAT PYRAMID.

By the Editor.

WE have seen that the Great Pyramid is so perfectly oriented as to show that astronomical observations of great accuracy were made by its architects. No astronomer can doubt this, for the simple reason that every astronomer knows the exceeding difficulty of the task which the architects solved so satisfactorily, and that nothing short of the most careful observation would have enabled the builders to secure anything like the accuracy which, as a matter of fact, they did secure. Many, not acquainted with the nature of the problem, imagine that all the builders had to do was to use some of those methods of taking shadows, as, for instance, at solar noon (which has to be first determined, be it noticed), or before and after noon, noting when shadows are equal (which is not an exact method, and requires considerable care even to give what it *can* give—imperfect orientation), and so forth. But to give the accuracy which the builders obtained, not only in the orientation, but in getting the Pyramid very close to latitude 30° (which was evidently what they wanted), only very exact observations would serve. Indeed, if a modern astronomer, knowing nothing about the Pyramid, were asked how the thing could be done without telescopic aid, he would be apt to say that no greater accuracy than (for instance) Tycho Brahe obtained with his great quadrant at Uranienburg could have been secured. Now, the orientation of the Great Pyramid approaches much closer to exactness than the best observations by Tycho Brahe with that justly-celebrated instrument.

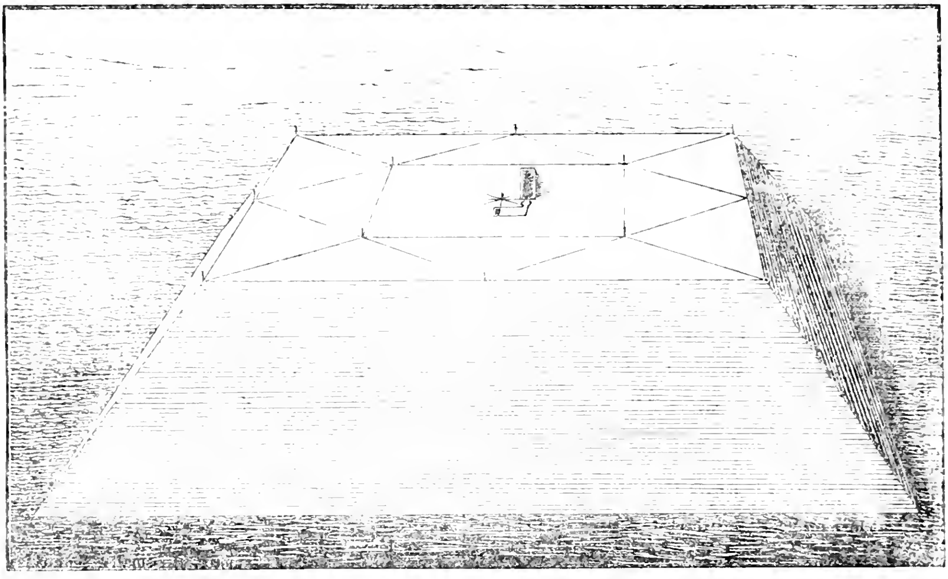


Fig. 1. The Pyramid Observatory, showing the object-end of the great observing tube.

Seeing this, and observing that the ascending and descending passages are just such as the astronomer would make to secure such a result, we may accept, without a particle of doubt, the belief that they were made for that purpose.

Then we saw that the features of the Great Ascending Gallery were not such as would be essential, or even desirable, to increase or maintain the accuracy of the orientation, as layer after layer was added to the Pyramid, but are precisely such as would be essential if the Pyramid was meant to subserve (as one, at least, of its objects) the purpose of an observatory.

But persons unfamiliar with astronomy will say (several have said so in letters addressed to me), This great ascending gallery would only enable astronomers to observe stars when due south, or nearly so, and only those which, when due south, were within a certain distance above or below the point towards which the axis of the Great Gallery is imagines that to be the chief observing instrument. The comparatively unobtrusive transit circle seems far less important. But the time observations, which are far and away the most important observations made at Greenwich, are all made, or at least, all regulated, by the transit observations. So are the observations for determining the positions of stars.

When the equatorial is used to make a time or position observation, it is used as a differential instrument, it is employed to determine how far east or west a star may be (theoretically, how much it differs in right ascension measured by time) from another; and again, to show how far north or south a star may be (theoretically how much it differs in declination) from another, whose right ascension and declination have already been determined by repeated observations with the transit circle. Similarly, the altitude and azimuth instrument is used in direct subordination to the transit circle.

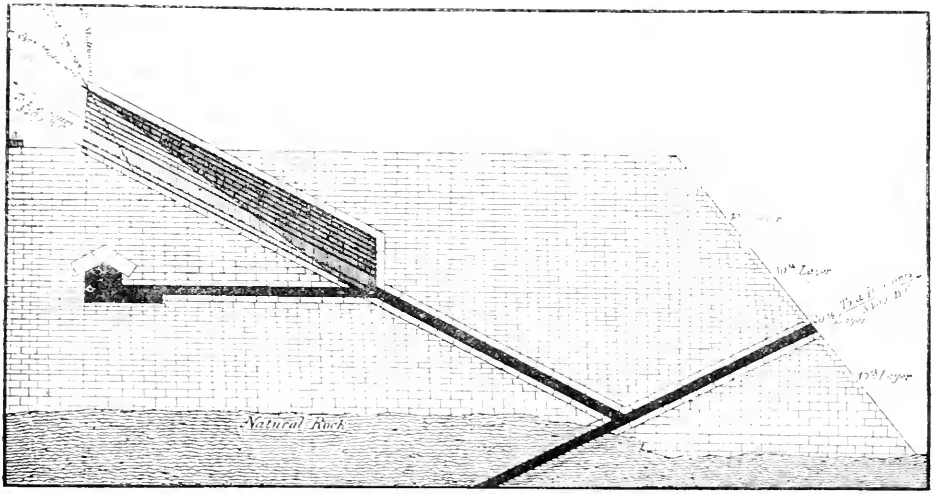


Fig. 2. Vertical Section of the Pyramid Observatory through the plane of the passages and gallery, showing the range of view of the great observing tube.

directed. Were all the other stars left unobserved? And again, we know that the Egyptians, like all ancient astronomers, paid great attention to the rising and setting of the heavenly bodies, and especially to what was called the heliacal rising and setting of the stars. In what way would the Great Gallery help them here?

Now, with regard to the first point, we note that the chief instrument of exact observation in modern observatories, the one which, as it were, governs all the others, has precisely this quality—it is *always* directed to the meridian, and has, indeed, a very much narrower range of view on either side of the meridian than the Great Gallery had. And though it is indeed free to range over the whole arc of the meridian from the south horizon point through the point overhead to the north horizon point, it is mainly employed over about that range north and south of the celestial equator which was commanded by the Great Gallery. The visitor at Greenwich sees the great equatorial, and

The astronomers who observed from the Great Pyramid doubtless made many more observations off the meridian than on it. They made multitudinous observations of the rising and setting of stars, and especially of their heliacal risings and settings (which last, however, though we hear so much of them, belonged *ex necessitate* to but a very rough class of observations). They no doubt often used astrolabes and similar instruments to determine the positions of stars, planets, comets, &c., when off the meridian, with reference to stars whose places were already determined by the use of their great meridional instrument. But all those observations were regulated by, and derived their value from, the work done in the Great Ascending Gallery. The modern astronomer sees that this was the only way in which exact observations of the heavenly bodies all over the star-sphere could possibly have been made; and seeing the extreme care, the most marvellous pains, which the astronomers of the Great Pyramid took to

secure good mechanical work, the astronomer recognises in him a fellow worker. He says, with the poet:—

> I am as old as Egypt to myself,
> Brother to them that squared the Pyramids:
> By the same stars I watch.

And now consider what was this great observatory of ancient Egypt the most perfect ever made till telescopic art revealed a way of exact observation without those massive structures. A mighty mass, having a base larger than the square of Lincoln's Inn, rising by just fifty layers to a height of about 142 feet, and presenting toward the south the appearance shown in Fig. 1, where the mouth of the Great Gallery is seen opening southwards, and the lines are shown which have been already indicated as "observing directions" in the picture on p. 315. The Pyramid observatory is shown in section in Fig. 2. It will be noticed that the successive layers are not of equal thickness. There are just fifty between the base and plane of the floor of the King's Chamber. The direction lines for the mid-day sun at midsummer, midwinter, and the equinoxes are shown: also the lines to the two stars, Alpha Draconis and Alpha Centauri, are given at the subpolar meridional passage of the former and the meridional passage of the latter, at the date when the descending and ascending passages thus commanded both these stars. Within fifty years or so on either side of this date, the Pyramid must, I should think, have been built. The later date when Alpha Draconis was at the right distance from the Pole, 2170 B.C.,* is absolutely rejected by Egyptologists—not one being ready to admit that the date of the Pyramid King can have been anywhere near so late.

* Some may be disposed to reject a change which they may imagine displaces the Pleiades from the position which Professor Piazzi Smyth assigned to that interesting group at the date when he supposed the Pyramid was built. But there never was the least real significance in that position. If the mistaken idea entertained by many, and repeated by Flammarion, Haliburton, and others, that the Pleiades at their meridian shone down the Great Gallery at the very time when the Pole Star of 2170 B.C. shone down the descending Gallery, had been correct, there might have been some reason to be struck by the coincidence. But it should hardly be necessary to tell the reader what every astronomer knows, that the Pleiades never did or could shine down the Great Gallery, and in the year 2170 B.C. were thirty-eight degrees (!) north of that position.

PHOTOGRAPHY FOR AMATEURS.

By A. Brothers, F.R.A.S.

Amongst the readers of Knowledge must be a large number to whom the production of a photograph is a mystery, and there must be many who desire information which would enable them with little trouble and expense to become so far expert, that although in some respects the mystery will be as great as ever, they may practice the art with some success.

Less than fifty years ago, the only means we had for recording the appearance of natural objects was by drawing by hand, assisted in some cases by the *camera lucida*, an instrument which enabled the artist to have a correct outline of the object, whether portrait or landscape, but all detail of light and shade had to be filled in by hand in the ordinary way. The black profile portrait, or *silhouette*, is an example of what the photographer has superseded.

It is proposed, in a series of short papers, to give an outline of the history of photography, and details of some of the processes which will be sufficient to enable anyone to take negatives and to print from them. It must be remembered, however, that, although the processes are not difficult to master, some care and skill are required if the results are to be of any artistic value.

Without going deeply into the history of the subject, it will be sufficient to say that one of the chemical substances now commonly used in photography was known in the sixteenth century, and an alchemist, named Fabricius, found that *luna cornea*, as chloride of silver was then called, was so affected by light that an image produced by a lens became improved in light and shade on the surface prepared with the *luna cornea*. This must be considered as the first discovery of photography, but the discoverer failed to see the importance of it, and it was not until nearly two centuries had elapsed that the curious effect caused by light on the chloride of silver was re-discovered by Scheele in 1777. This chemist also failed to see the value of what he had observed, and it was not until 1802 that experiments were made at all analagous to what we now call photography. In the year last named, Thomas Wedgwood, assisted by Sir H. Davy, produced pictures on white leather and paper, but, as no means were known by which the images could be fixed, none of these early photographs exist.

The process of photographic printing now in universal use, is based on these early experiments of Wedgwood and Davy, and it may be interesting to repeat an experiment, to show what kind of pictures they produced. Take a piece of smooth writing paper, float it in a solution of common table salt (sodium chloride) for a minute or two, then hang it up to dry, or dry it by the fire. When dry, float the paper on a solution of silver nitrate, say 30 grains to the ounce of water (rain or distilled water should be used), or the silver solution may be evenly brushed over the surface with a large camel's-hair brush, or by means of a glass rod, so as to avoid using a large quantity of silver solution to float the paper upon. The prepared paper may be dried by the fire, or hung up in a darkened room or cupboard. When quite dry, a leaf, piece of lace, or any other suitable object may be placed on the paper, then covered with a piece of glass to keep paper and object in close contact, and then placed in sunlight. As soon as the paper is completely blackened it will be found that the picture of the leaf or other object will be printed on the paper as a negative—that is, the dark parts will be white, and *vice versâ*. The paper still remains sensitive to light, and, of course, must be viewed only by weak daylight or by artificial light. This, then, is the reason why the earliest photographs are not now in existence. Later, those *photogenic drawings*, as they were termed, were immersed in plain water, by which the silver salt was partially removed, but the pictures were not properly fixed, and therefore were not permanent. In a future paper we shall see how perfect fixation is effected.

We have no record of advance in photography until 1814, when J. Nicéphose, or Niepce, succeeded in producing permanent figures in bitumen of Judæa; the time required rendered this process impracticable. About this time, and leading up to 1839, Daguerre was engaged in researches which resulted in the discovery of the process which bears his name; and this process for many years was most extensively used, and the pictures produced remain to attest the beauty and value of the method. Daguerrotypes are permanent if protected from the atmosphere, which tarnishes the silver surface on which the pictures are produced. This tarnish, however, can be removed by chemical means, and the picture remains on the surface as perfect as ever.

THE CRYSTAL PALACE ELECTRICAL EXHIBITION.

Fifth Notice.

The Exhibition may now be considered practically complete. Marvellous progress has been made during the last few days, and the Palace is crowded with visitors. Swan's exhibit is magnificent, but as it is our purpose to consider next the various systems of incandescent lighting, we refrain from making any further reference to it this week.

Telegraphy is well represented, the lead being naturally taken by the British Postal Telegraph department. It is somewhat remarkable that this is the only branch of applied electricity in which any serious effort is made to exhibit a chain-like series of historic relics. The display, however, lacks many interesting objects, in consequence of the foreign Administrations not having loaned any of their apparatus.

The Post-office exhibits may be divided into two classes—historic and modern. In the modern collection, interest is chiefly centred in the Wheatstone Automatic Instrument, by which messages may be transmitted at the rate of 200 to 250 words per minute. It is used on circuits which are required to carry a large amount of work, but it is being gradually superseded on ordinary commercial wires by duplex and quadruplex apparatus. Its applicability to news work, that is, the transmission of long messages for newspapers, is unequalled by any other form of apparatus, and as this kind of work frequently amounts to about 500,000 words forwarded from the Central Telegraph office during a single night, there is little prospect of its being relegated to the "historic" collection.

Various other forms of telegraph apparatus are exhibited, including duplex apparatus, by which messages may be transmitted simultaneously in opposite directions; but the quadruple apparatus, by which four messages may be sent at a time (two each way), is not exhibited by the Post-office. The familiar single-needle instrument is shown, as well as more the complicated fast-repeater apparatus, for both single and duplex working. These latter sets are used where it is desired to increase the working capacity of long lines. Practically, the repeater halves the length of the line. For instance, suppose we have a wire from London to Glasgow, and find that through certain causes, due to our uncertain climate, we can only work at the rate of, say, sixty words per minute. A station about mid-way, such as Leeds, is asked to insert his repeater apparatus in the wire. He does so, and, by an automatic arrangement, the current which leaves, say, London, only goes to Leeds, where, by moving the soft-iron armature of an electro-magnet, another circuit is completed, and a current from the Leeds battery goes on to Glasgow.

The historic collection is very interesting, even to the least curious of visitors. It includes the oldest known piece of telegraph apparatus, viz., Ronald's electric telegraph, which was laid as an experiment in Sir Francis Ronald's garden at Hammersmith, in 1816. There are also specimens of the Morse type, cast in 1832, when it was supposed to be impossible for human fingers to manipulate the apparatus in such a way as to be able to distinguish between long and short signals.

One of the most interesting objects is the "Fossil" underground telegraph (1837). Fig. 1 is a diagram representing a section of this line; it consisted of a kind of triangular wooden rod, with five grooves, into each of which a copper wire, covered with cotton and pitch (for insulating purposes), was laid. The grooves were then filled up by strips of wood. When thus finished, it was laid underground. Such a line contrasts most remarkably with modern wires. It, however, serves its purpose here, and takes the mind back really a few years, but, apparently, many a century. The

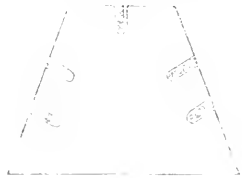


Fig. 1.

line was laid in connection with Cooke and Wheatstone's five-needle telegraph, which was used at Paddington and West Drayton in 1838. Fig. 2 represents the dial, or reading portion, of the apparatus. There were five magnetic needles, under the influence of as many coils of wire, each coil being in a separate circuit, including one of the wires represented in Fig. 1. The sending portion of the apparatus

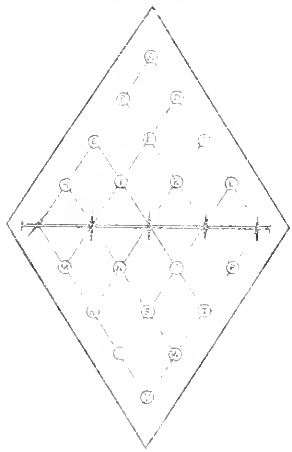


Fig. 2.

allowed of the current being sent from the galvanic battery in either direction, so that each needle could be deflected to the right or left. The letters of the alphabet (omitting those which were deemed of least importance or necessity) were marked on the dial as shown in the diagram. When it was required to telegraph a letter, two needles were deflected in such a way as to make both point to that particular letter. In the diagram the letter "B" is thus pointed out or telegraphed.

In 1840 the five needles were reduced to four, but two years later a most important change was effected. The instrument only had two needles, and instead of their being made to point out the letter, a code was arranged by which movements of either or both needles to the right or left a given number of times indicated the letter desired.

Another interesting and important relic is Bain's Chemical Telegraph (1850), in which the ordinary green ribbon used in the Morse inker is replaced by white paper,

prepared with yellow prussiate of potash (potassic ferrocyanide) and ammonic nitrate. The current, passing through a style pressing on the paper as it passed over a revolving metal wheel, decomposed the compound, and left a mark on the paper. There are very many other relics, far too numerous even to mention, including specimens of the earliest laid cables, insulators, etc.

What may be regarded as an interesting curiosity is a specimen of a Norwegian telegraph pole, which has been pierced through by woodpeckers in search of insects. The birds are supposed to have been deceived by the humming of the wires, a sound which may generally be heard near the poles, more particularly in still, calm weather.

OUR ANCESTORS.

By Grant Allen.

II.—THE CELTS.

While the dark-haired and dark-skinned little Euskarians were living unmolested in the western coasts and islands of Europe, hewing patches out of the forest with their stone hatchets, building great barrows over their dead chieftains, and fighting among themselves from valley to valley, like the North American Indians of later days—a fairer and taller race was growing up unnoticed away to the east, among the great central tablelands of the Asiatic plateau. This fair-skinned, yellow-haired, and blue-eyed folk is known to us by the somewhat fanciful name of Aryans; and from it the chief conquering peoples of the whole Eastern hemisphere are derived. The Aryans spoke a language whose nature we can infer from the numerous modern dialects derived from it; and this language enables us in part to form some conception of the state of culture attained by the people who used it. In their earliest known condition, while they still all lived together among the high plains of Asia, they were hardly, if at all, superior in the arts of life to the Euskarians of Britain. They were ignorant of the use of metals, and armed only with weapons of polished stone. They fed their flocks like the semi-nomad tribes which still inhabit the same regions, and they tilled a little grain of some coarse cereal kind. Altogether, if we regard them with calmly impartial eyes, and not with the excessive filial piety of some German thinkers, we shall probably be forced to admit that the primitive Aryans were, on the whole, about as good and as bad as most other barbaric peoples at the same period of the world's history. Stronger than the neighbouring nations they certainly showed themselves to be, but wiser or better there is no sufficient reason to suppose that they were.

From their original Central Asian home, these warlike Aryans began to disperse themselves as fighting colonists on every side, probably some five or six thousand years since. One great branch, now speaking the Celtic variety of the common language, moved westward across the face of Central Europe; and its members spread themselves, long before the beginning of written history, over all the western coasts of the continent as a conquering and superior race. Though at first they were only armed, like the Euskarians amongst whom they came, with stone hatchets and flint-tipped arrows, yet, as they were tall, big limbed, powerful men, while the Euskarians were comparatively short, squat, defenceless folk, they seem easily to have overrun almost the whole of what is now France, Spain, and the Low Countries, and to have established themselves, at least, as a rough aristocracy of chieftains among the conquered and servile Euskarian population. But in some places the Euskarians, and their kinsfolk the Ligurians and Aquitanians, appear to have maintained their independence; while in others, though the Celts were masters, the dark-skinned aboriginal people yet survived in vast numbers. It was only in the most thoroughly conquered parts of the continent that the pure-blooded Celts themselves formed the principal mass of the population. The independent dark tribes of the extreme west retained their native language, which lives on to our own time as the Basque tongue; but the vanquished and enslaved Euskarians of the central French and Spanish regions learned to speak the dialect of their Celtic lords, as they afterwards learned to speak that of their Roman conquerors.

As yet the Celts had not attempted to attack Britain, which had long since been isolated from the continent, and could now only be invaded by a fleet of boats crossing the silver streak of sea. Before they took that last step in the conquest of Western Europe, they had learned the use of bronze, from which they manufactured beautiful axes, spears, and shields, besides producing many tools for more peaceable purposes. The employment of bronze enabled the Celts to make such improvements in ship-building that they could cross the Channel to Britain, which they found inhabited only by the small dark Euskarians, who were now at a still greater disadvantage, seeing that they were only armed with stone tomahawks, while their big assailants were armed with "weapons of precision," in the shape of bronze battle-axes, lances, and spears. The consequence was that the Celts soon overran nearly the whole island, and quickly subdued the better part of it to their own dominion. In the south-eastern plains, near the Continent, they apparently settled in great numbers, so that when the Romans came they found that part of Britain mainly inhabited by a tall, fair-haired, light-skinned Aryan Celtic race. But in the west, the Celts only settled in comparatively small numbers, as lords of the soil, holding in subjection a large servile population of dark Euskarians; while in South Wales, and apparently in parts of the Scotch Highlands, the dark people remained wholly independent, as the inhabitants of those regions long afterwards did at the time of the English settlement. The South Welsh tribe of Euskarians were known as Silures, and they preserved their nationality intact down to the period of the Roman Conquest.

Now, what sort of people were the pure-blooded Celts who first came to Britain? No doubt it may be a shock to many readers to be told so, but they were undoubtedly a light-skinned, fair-haired, blue-eyed, and round-headed race—in fact, typical Aryans of the same sort as the modern Germans, and possessed of exactly those peculiarities which we ordinarily speak of as Anglo-Saxon. About this there can be no manner of mistake. Their barrows, known both by their shape and by their bronze implements, contain round skulls, quite different from the long skulls of the Euskarians; and the universal testimony of the Roman writers, whose knowledge of the Celts was obtained while they still lived in comparative purity in Gaul and South-eastern Britain, makes it quite certain that they had light hair, white skin, and blue eyes. How, then, comes it that most of us think of the Celtic type as essentially dark and black-haired? The reason is simply this. When the Celts conquered Britain, they left large numbers of Euskarians alive, in the northern and western part of the island, at least; and it is the mixed Celtic and Euskarian descendants of these people who now form the so-called Celts of the Highlands, Lancashire, North Wales, and Cornwall. Moreover, it is

certain that the Euskarians of the conquered districts soon learned to speak Celtic alone, just as the Irish are now fast learning to speak English alone: and so after a short time they became as indistinguishable from the true Celts, as Normans and Danes in England have become indistinguishable from the rest of the community. Even the Silures, who maintained their position as an independent Euskarian tribe in South Wales, seem to have acquired the use of the Celtic Welsh tongue before the date of the Roman invasion. When contrasted with the Teutonic English, all these Celtic-speaking peoples came naturally at a later period to be regarded as Celts.

Thus, at the date when Britain first became known to the civilised southern world by the Mediterranean, and before any Englishmen had yet settled in the land, its ethnical arrangement was something of this sort:—Along the southern and eastern plains, from Hampshire, by Sussex and Kent to East Anglia, Lincolnshire, and the vale of Yorkshire, there lived a light Aryan Celtic race, with more or less of subject or enslaved Euskarians doubtless, a good deal intermixed, as negroes, mulattoes, quadroons, and whites, still are in the Southern States and the West Indies, though the light Celtic aristocracy probably kept up the purity of its own blood in the female line, as also happens in the analogous modern case. Further west and further north, among the hills of the Devonian peninsula, the West Riding, Cumberland, and the Highlands, the number of pure Celts was comparatively smaller, while the number of dark Euskarians was comparatively greater. And in Wales itself, the Silures remained as unmixed Euskarians, without a single drop of Aryan Celtic blood; while another small Euskarian principality seems also to have held out in the Athol district of Scotland. It is this compound mass of pure Celts, mixed Celt-Euskarians, and pure Euskarians, all speaking various Celtic dialects, that we ordinarily describe as Celtic, in contradistinction to the Teutonic English, who came to the country at a later date. As to Ireland, the primitive Celtic immigration there was very slight; and the mass of the population, though it acquired the Gaelic dialect of Celtic as its language, remained almost entirely Euskarian in blood up to the date of the Danish invasions, as it still remains in all except the northern and eastern coast. How far these arrangements of the various race-elements were upset by the English (or Anglo-Saxon) settlement, we shall have to inquire in our next paper.

NOTES ON ROWING.

BY AN OLD CLUB CAPTAIN.

A PROFOUND knowledge of the theory of propulsion through fluids is not essential to skilful oarsmanship, and is probably not possessed by one oarsman among ten thousand; I may go further, and note that even a perfect acquaintance with the principles of rowing may be found in company with singular inaptitude for the practical application of those principles. Shall I ever forget, for instance, how ably No. 4 in our "tub" (when I was as yet a beginner in college rowing) would discourse about the proper way of rowing, taking, if occasion suggested, a fire-shovel wherewith to illustrate the beginning, middle, and end of the stroke? but alas! in practice he could never row a mile without catching a most monstrous crab.

It is not, then, with any idea that the general run of oarsmen should study the mechanics of propulsion or the true theory of rowing either, that I pen these lines. But the subject is an interesting one, especially just now, when Oxford and Cambridge are preparing for their annual struggle; and a good oarsman is likely to be none the worse for some consideration of the *rationale* of his art.

According to the system of boat propulsion adopted in most civilised countries (but the gondolas of Venice are propelled differently) the oar is a lever of the second class, in which the fulcrum is at one end, the force is applied at the other, and the weight is somewhere between the two. The fulcrum is not fixed as in ordinary applications of the lever, for the water yields to the oar in some degree; but apart from this, the principle is precisely that of the second class of levers.

Many, however, who consider the problem of boat propulsion in this way, are perplexed by the circumstance that the oarsman himself is in the boat, and forms part of the propelled weight, while, again, the central line of the boat's breadth does not correspond with the place where the driving force is actually applied to the boat. Thus, if A B C is the oar, the rowlock at B, the place where the resistance of the water against the blade may be supposed to act at C, and the power of the oarsman applied at A, we know that in reality the weight of the boat is under A, not under B; while, again, the power P is applied within the boat itself, and whatever effect the pulling of the oarsman produces in one direction, must be exactly counterbalanced by an effect in the opposite direction. It is, in fact, the leverage alone which gives a balance of propulsive

effect. The weight of the boat is really felt at B, so far as the particular oar A C is concerned; the oarsman's strength is applied at A, and is met by an equal resistance there, but the propulsive effect at W is greater, in the same degree that the arm A C is greater than the arm B C. Thus, if an oarsman pulls at his oar with a force such as would suffice to lift one hundredweight, and if A C is 12 ft. long and A W 3 ft., then the propulsive effect at W corresponds to 112 lb. multiplied by 12 and divided by 9, or to $149\frac{1}{3}$ lb. But the boat is not urged forward by this propulsive effect, only by the excess of this amount over the force actually exerted by the oarsman at A, so that the balance of propulsive action on the boat with its crew corresponds to a force which would raise a weight of $(149\frac{1}{3}-112)$ lb. at the same rate as the oarsman moves his end of the oar.

It is evident that the actual leverage increases as A W is increased, supposing the oar's length to remain unchanged. But at the same rate that the leverage is increased, the velocity with which the oarsman's action tends to move the boat is diminished. Supposing C to remain at rest, and the end, A, to move with a given velocity V, the point W (at which the propulsive action is really exerted) only moves with velocity V diminished in the ratio of C W to C A. It might seem, then, that the use of outriggers diminished rather than increased the propulsive power of the oarsman—increasing his leverage, which one would have said did not need to be increased when the boat was at once made lighter and sharper—and diminishing the velocity with which his action tends to urge the boat onwards. But at the same time that the rowlock was thrown somewhat farther from the handle of the oar (not nearly so much farther as many imagine, for the old boats were wider in the beam, and their rowlocks were carried well out), the oar itself was lengthened. Owing to the diminished resistance, too, as the boat passed through the water, there is less slip of the oar through the water, which thus supplies a more perfect fulcrum. Yet the increase of velocity in light, outrigged boats is due more to the way in which they maintain their speed between the strokes than to any increased power of propulsion obtained by the oarsman. Being of smaller beam and lighter than the old racing boats, and also without keel and without laps, they maintain their velocity almost unchanged between the strokes.

And here arises a question which has been very summarily, but in my opinion very incorrectly, disposed of by many writers on rowing. It is often said that the principles of rowing are just the same now as they were in the time of the old lap-streaked inrigged racing boats. The old rules for the action of arms and body—of legs also, except in so far as the sliding seats modify their action—are therefore repeated, as if no change whatever had been rendered necessary by the changed style of boats. In other words, though it is quite certain that the new racing boats behave quite differently, though it is manifest that as they move more quickly through the water they must receive a sharper propulsion, though it is clear that with the greater leverage obtained from the use of outriggers there arises a different amount as well as a different degree of propulsion at each stroke, oarsmen (we are told) should row in the same style now as before these changes were introduced.

It would be as reasonable, I venture to say, to assert that the style of stroke suitable for a coal barge must be the best also for a wager boat. It is manifest there must be some changes, and tolerably clear what those changes should be. And as a mere matter of fact, it is seen that those rowing clubs wherein the old style of rowing is clung to, get persistently beaten, or only win

when they have a great superiority of brute force, while those who adopt a style suited to the requirements of the light racing boats as systematically win often against great superiority of sheer strength. Again it is found that those who on their own waters are obliged to use the heavier boats, and, therefore, the old-fashioned style, are always, or almost always beaten, and that too by weaker men, when they row in the lighter boats, even when they have thoroughly mastered all such difficulties as arise merely from the relative crankiness of the canvas-covered outrigged craft.

In my next I shall show what are the differences of style which theory suggests for the lighter boats. I shall maintain the apparently paradoxical positions (1) that the stroke must be longer, yet shorter in the water; (2) that it must be quicker, yet fewer strokes be taken per minute. I shall show how these requirements are to be secured, and I shall give evidence, which I take to be convincing, that, when they are secured, a style is acquired which utilises the oarsman's strength in the best way for contests in these cranky craft.

(*To be continued.*)

THE *SPECTATOR'S* COMET.

(From the *Saturday Review.*)

"*Sir Andrew Aguecheek.*—Why, this is the best fooling, when all is done."

SOME time ago—on January 28, to be particular—the mantle of Mother Shipton fell upon the *Spectator*, and she began to prophecy concerning the end of the world. Fifteen years more of life was about the exact period [*sic*] which the *Spectator* was inclined to allow to plants and animals, including man. The convulsion produced in the religious world, or rather in a part of it, was curious and pleasant to observe. The Bishop of Manchester naturally did not let such an excellent opportunity pass unpreached on, and pavid correspondents sent their views to the *Spectator*. But meanwhile Mr. Proctor has withdrawn his celebrated Menacing Comet. This phenomenon was entered, it appears, for the Scientific Sensation Stakes by the *Spectator*, "without the assent" of its owner. We really do not think that Mr. Proctor has behaved quite kindly to the *Spectator*. The *Spectator's* ambition was partly like that of the Fat Boy, as expressed in his celebrated remark to the Old Lady:—"I wants to make your flesh creep." Our contemporary was also anxious, if we may say so, to score off the Positivist and the unbeliever. But there was also manifest a very creditable desire to give Mr. Proctor and his new book "a hand." Every fellow likes "a hand," as Mr. Foker has said, with his usual artless wisdom. The *Spectator* gave Mr. Proctor "a hand," but he does not seem quite grateful. In the February number of KNOWLEDGE, a journal occupied with such topics as the "Use of Fleas," "How Spiders Fly"—[Now, dissatisfied ones, where are you, who say KNOWLEDGE is too much given to astronomy, biology, botany, and mathematics? Here is the *Saturday Review* to tell you that Mr. Mattieu Williams on the "Use of Fleas," and Professor Young on the "Flight of Spiders," out-top all other subjects]—Mr. Proctor has scratched or withdrawn that "Menacing Comet" which the *Spectator* had fondly made its own. . . . . Mr. Proctor's most significant remark in his book, we take to have been this—[here follows the reference to the outburst in the Northern Crown, in 1866.] There is a well-known sentence in an unpublished novel—"'Here is a blasted flare-up,' said the princess, whose girlish modesty had hitherto kept her silent." Mr. Proctor had been explaining that what the bashful princess called a "blasted flare-up" had occurred in the new star of the Northern Crown in 1866. And he had said that the "flare-up" was probably caused by the motion of the comet followed by a meteoric train. . . . What conclusion could the *Spectator* draw from all this?—and from Mr. Proctor's refusal to say "that there is *absolute* danger in the case of our own sun when the comet of 1843 shall be absorbed by him?" "What conclusion could be drawn, except that Mr. Proctor thought the comet a serious matter and the odds against the destruction of life in the world very short odds indeed? And now Mr. Proctor distinctly declares (and we are glad to have his assurance), that "there is not the slightest reason to fear that the comet will do any harm to the solar system when finally absorbed." Mr. Proctor, in KNOWLEDGE, says that he has elsewhere shown that "all comets of the destructive sort"—all "rogue comets," if we may adopt a term from the Jumbo controversy—have long since been eliminated from the solar system. Mr. Proctor gives pictures of comets of the rogue and peaceable varieties. The menacing comet, or, as we may now call it, the domesticated comet, is a very slim and scanty one. Then we have a likeness of a comet which might have been dangerous if it had gone the wrong way. Then we have a portrait of a comet . . . . with a gigantic eye in its head, and a bushy and furious tail. However, that is the look-out of some other solar system, and not ours. Our solar system, we again repeat [*sic*], is all right . . . . unless a comet like Donati's "gets its head," bolts, and comes straight for the sun. As for the comet of 1880, in future it will be known as "the *Spectator's*," or perhaps "the Bishop's comet."

NEWTON'S VIEWS AS TO A MENACING COMET, AND AS TO THE HEAT OF THE SUN'S INTERIOR.

AMONGST the MSS. referring to Sir Isaac Newton, in the possession of the Earl of Portsmouth, is a paper in the handwriting of Mr. Conduitt, who married Catherine Barton, Newton's favourite niece; it purports to be notes of a conversation held with Sir Isaac about three years before his death, and contains a conjecture with respect to the ultimate fate of the comet of 1680, which bears a striking resemblance to the prediction which has recently been attributed to Mr. Proctor.

Mr. Conduitt, who was Newton's successor at the Mint, had the intention of writing a life of Sir Isaac, and commenced making notes for the purpose, but he had but little literary talent, and the project was soon abandoned. The notes, however, have been preserved, and though they have never been printed at length, the paper from which the following extract is taken is given in Turnor's "History of Grantham," a rather rare book, which is not in the Library of the Astronomical Society. Mr. Conduitt says—

"I was on Sunday night, the 7th of March, 1724-5, at Kensington with Sir Isaac Newton in his lodgings, just after he was come out of a fit of the gout, which he had had in both his feet, for the first time, in the eighty-third year of his age. He was better after it, and his head clearer, and memory stronger, than I had known them for some time. He then repeated to me, by way of discourse, very distinctly, though rather in answer to my queries, than in one continued narration, what he had often hinted to me before, viz., that it was his conjecture, he would affirm nothing," . . . . . . [I omit a paragraph with respect to the planets] that "a comet after certain revolutions by coming nearer and nearer to the sun would have all its volatile parts condensed, and become a matter fit to recruit and replenish the sun (which must waste by the constant heat and light it emitted), as a faggot would this fire, if put into it (we were sitting by a wood fire) and that that would probably be the effect of the comet of 1680 sooner or later, for by the observations made upon it, it appeared, before it came near the sun, with a tail only two or three degrees long, but by the heat it contracted in going so near the sun, it seemed to have a tail of thirty or forty degrees, when it went from it; that he could not say when this comet would drop into the sun; it might, perhaps, have five or six revolutions more first; but whenever it did, it would so much increase the heat of the sun, that this earth would be burnt, and no animals in it could live. That he took the three phenomena seen by Hipparchus, Tycho Brahe, and Kepler's disciples, to have been of this kind, for he could not otherwise account for an extraordinary light as those were, appearing all at once amongst the fixed stars (all which he took to be suns enlightening other planets, as our sun does ours) as big as Mercury, or Venus, seems to us; and gradually diminishing for sixteen months and then sinking into nothing."

In the light of our present knowledge with respect to the mass of comets—and the probable origin of the sun's heat, such a theory is, of course, untenable, but the speculation is of interest, as it serves to show that Newton must have suspected the existence of a resisting medium in the neighbourhood of the sun. If he had not, as is, possible, satisfied himself from the observations of the total eclipse of May, 1715, that the solar corona then observed* had a real existence, and was not merely an optical illusion.

Long before this eclipse, it is evident that Newton suspected an atmosphere outside the photosphere. In the "Principia," Book III. (I quote from Davis's translation, published in 1803, at Vol. II., p. 307), he says: "The comet which appeared in the year 1680 was in its perihelion less distant from the sun than by a sixth part of the sun's diameter, and because of its extreme velocity in that

* Several drawings were made of the corona observed during this eclipse—woodcuts from two of them are given in Edleston's correspondence of Cotes and Newton. From the account of the eclipse in the *Memoirs de l'Académie*, it is evident that several drawings were made by French observers. Newton himself no doubt observed this eclipse, though he does not appear to have done so in company with his friend Halley and other members of the Royal Society, who observed it from the roof of the Society's House in Crane-court, out of Fleet-street. From a letter of Cotes, it is evident that Newton was not at Cambridge at the time of the eclipse.

proximity to the sun, and some density of the sun's atmosphere, it must have suffered some resistance and retardation; and therefore, being attracted something nearer to the sun in every revolution, will at last fall down upon the body of the sun."

Another fact with respect to Sir Isaac Newton's correct judgment as to the internal heat of the sun is worthy of being mentioned, especially as a contrary theory with regard to a cool dark body within the photosphere survived down to the time of Sir John Herschel. In a letter dated 16th April, 1681, Sir Isaac Newton says, "Now though the inward part of the sun were an earthy gross substance, yet, if the liquid shining substance, which Mr. Flamsteed supposes to swim upon it, be then hot, it will heat the matter within as certainly as melted lead would heat an iron bullet immersed in it. Nor is it material whether the liquid matter on the sun be of any considerable thickness. An iron bullet would heat as fast in a quart as in an ocean of melted lead, this difference only excepted, that the bullet would cool a small quantity of lead more than a great one. If, then, the liquid matter swimming on the sun be but so thick as not to be cooled by the central parts (as it must be), it will certainly heat the central parts, for it imparts heat to the contiguous matter as fast as if it were thicker, and keeps all cool environing mediums (the instrument of cooling things) from coming near the central parts to cool them. By which means the central parts must become so hot, as if the hot fluid matter surrounding it equalled the whole vortex. The whole body of the sun, therefore, must be red hot," &c.

A. C. Ranyard.

INTELLIGENCE IN CATS.

Correspondents of Knowledge, in treating of cats, do not seem to have remarked some acts of intelligence which may be observed daily in the streets of London. At the cry of the cat's-meat man all the cats are in commotion, but all are not excited by the cry of the same man. A dozen men may walk up and down a street with the tempting morsels, calling "meat, meat!" but only at those houses which they are accustomed to serve will the cats be roused by the call. No sooner does the proper man arrive in a street than every cat he is accustomed to serve rushes frantically to the door, or, if allowed, into the street, running mewing towards him, rubbing against his legs, or sometimes sitting in a begging attitude before him, but never, as far as I have observed, attempting to steal from the open basket.

One day I noticed a cat whose man had either forgotten her portion or had been unable to make her mistress hear, and so had passed on. The cat, however, insisted upon being attended to; she ran after him, mewing piteously, and when at last she made him understand, she ran back to the house before him, where by that time the mistress was ready to take the delicacy so much prized by all London cats, however well fed. I have often watched this act of discrimination in our own cat. Tom would sit quietly dozing whilst man after man went by with the familiar cry of "Meat, meat," but presently he would jump up, rush to the window, and remain in a state of great excitement, and soon after a distant cry of "Meat" might be heard, and we knew that Tom had recognised his own man long before we had heard him. As the cry drew nearer, Tom's excitement increased, and he would almost fly to the door. A singular fact remains to be told. On Saturdays the man would leave two portions, as he did not go his rounds on Sundays. These were often thrown into the area, to which Tom had access. He would always greedily devour the one portion, but never touch the other, although they lay side by side. This cat would also open the latch of the kitchen-door, as observed by several of your correspondents, and would also open the shutters in the drawing-room (closed but not fastened), in order to look out of window. I have, however, been told of a cat who would open not only a latch, but an ordinary drawing-room door, rather loose, by taking the round knob between her paws and twisting it round and round till it opened.

The fact of cats distinguishing between one meat man and another seems to me to disprove the oft-repeated assertion that cats attach themselves only to places, and not to persons; for here we see them able to pick out a certain man by his voice alone, even at a great distance.

A. W. Buckland.

WOOD-GAS.

If I may be permitted to do so, I should like to say a few words in reply to Lewis Arundel's "elucidation" of my abstract on "*Wood-gas*," *not* "Carbon Monoxide," as he construes my meaning.

If, on line 12 of the abstract, page 216, the word *can* between the words "*we obtain*" had been inserted, Lewis Arundel might have been saved the trouble of elucidating my description (or more strictly speaking my informing the readers of Knowledge that there was such a thing) of "Wood-gas." I am sorry I did not make my meaning clearer.

The "dangerous, obnoxious, and otherwise objectionable products" referred to are N; S; H_2S; CS_2 and tar. N in the form of ammonia, which is virtually absent. Little sulphur can exist in any form, while there is but a trace of sulphuretted hydrogen or of carbon bisulphide, and there is not a trace of tar.

I did not previously enumerate these for fear of taking up, needlessly, too much space in your paper.

Lewis Arundel evidently mistakes the acid taste (or, perhaps, the tingling in the nose, when inhaled through that organ), of CO_2 for an odour; that is to say, if he is dealing with CO_2 in a *pure* state when he perceives the odour about which he is so certain. He might as well say that hydrogen had an odour, because he himself had never obtained it odourless. I cannot think he could be in earnest when he said (*re* CO_2), "whatever our text-books may *copy* from one another." Is it likely that the leading chemists of the day, in writing their text-books, simply "copy from one another" without being themselves thoroughly conversant with the peculiar properties of the subject under their notice?

What would Lewis Arundel say to the man who told him that the snow they were looking at was green, because he (the man) was unaware that he was looking at the snow through green glasses? This is not a bit more ridiculous than to say that chemists "copy from one another," *re* the odour of CO_2.

The blue flame of CO is hardly observable over a *sluggish* fire; on the contrary, it is seen on the top of a "glowing" fire, with a background of red-hot bricks. Lewis Arundel would see how the CO is formed by referring to my text-book.

He now asks, "How, then, can it confer on 'an inferior coal-gas' a 'great candle-power'?" This, I own, is not quite clear, my mistake being that, in trying to be concise, I was too concise—when I say that the coal and wood-gas are mixed with *naphtha vapour*, Lewis Arundel will see how "the great candle-power" is conferred. I may say, before concluding, that "after the first charge of wood has been carbonised, the charcoal, instead of being drawn, as with coke, is pushed back into the retort," &c., &c. This is repeated with the second charge, and so on.

Anyone who has to make the gas, carbonic oxide, will see the caution given in all text-books, *i.e.*, its very poisonous property.

F. C. S.

FOR NEXT WEEK.

Next week, an interesting paper by Dr. Ball, on the Future of the Earth and Moon, will appear, and one by Dr. Carpenter, on Dr. Siemens' recent communication to the Royal Society, respecting a possible use of the seemingly waste energies of the sun. In response to a number of queries respecting the motions of planets with respect to the earth, and the explanation of the advance, retrogradation, and stationary points of the planets, I have determined the distance and bearing of the planet Mars from the earth at intervals of ten days from the opposition of 1875 to that of 1892, and, having set them down in a chart, have carried through them the various convolutions representing the path of Mars during those seventeen years. The result (which is rather curious, and represents much more labour than many would perhaps suppose) will be presented next week in a two-page chart.

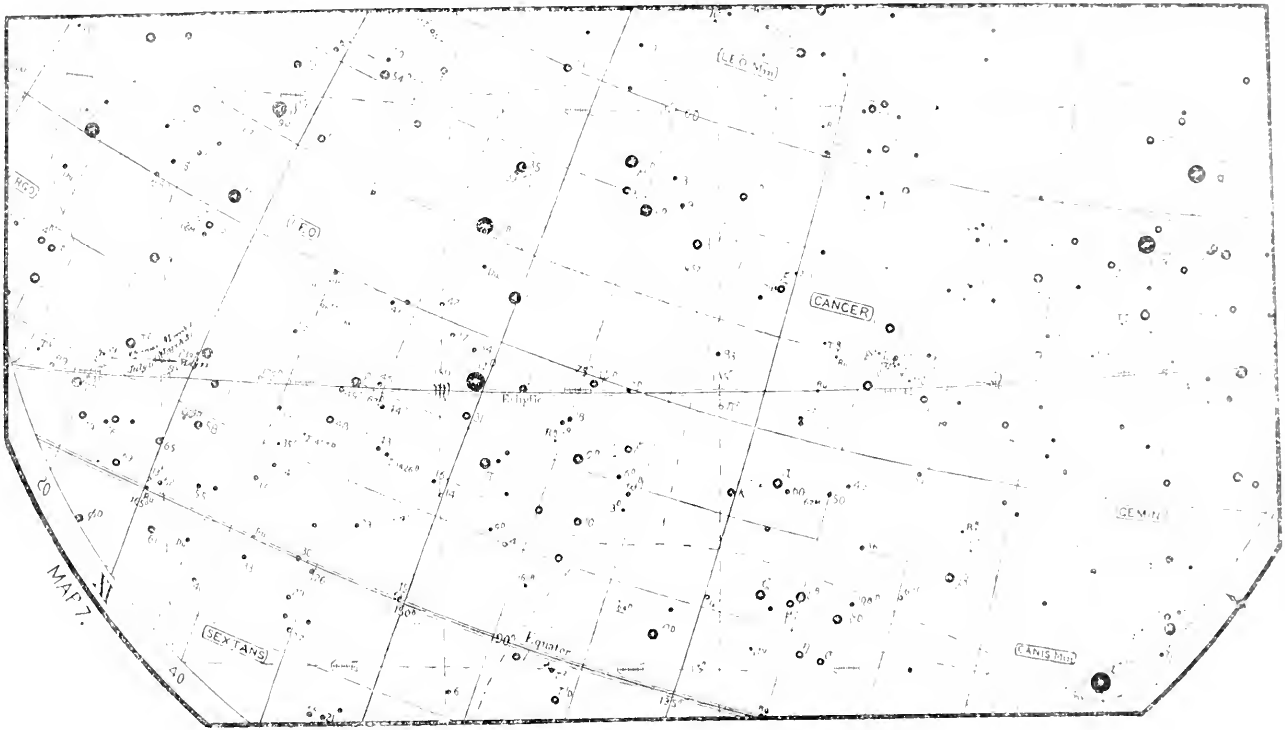
Richard A. Proctor.

METEOROLOGICAL REPORTS.

I propose from and after the vernal equinox, at latest, to publish weekly reports of the weather, based on the daily records obligingly supplied from the Meteorological Office. As yet, the plan on which these reports will be given has not been decided upon, nor can we at present assign the space we can afford to give to the subject.

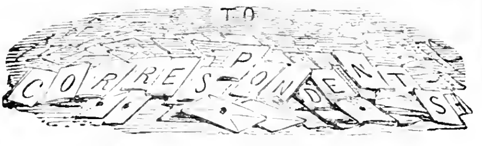
OMISSIONS AND CORRECTIONS.

We omitted to notice in the foot-note relating to the book by Dr. De Luys, on the Brain, that it forms one of the International Scientific Series, published by Messrs. Kegan Paul & Co., and that its price is 5s. The treatise on the Sun, by Professor Young, has been included (since our review appeared) in Messrs. Kegan Paul & Co.'s announcements as belonging to the same series, at the same price. For the *Nineteenth Century*, in the "Answers to Correspondents" for last week, p. 393, 1st col., line 17, read the *Century* (which, we believe, was what was originally written).



ZODIACAL MAP.

We give this week our second Zodiacal map, showing, in enlarged scale, the part of the Zodiacal region best suited for observation during this month and the next. The path of Mars during March is just outside this map in Gemini, and for it the reader is referred to our first Zodiacal map in Part III. (Number 11). The path of Uranus during its present opposition passage is shown. No other planets occupy this portion of the Zodiac at present.



Letters to the Editor.

[The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with their writers. All communications should be as short as possible, consistently with full and clear statements of the writer's meaning.]

All Editorial communications should be addressed to the Editor of KNOWLEDGE; *all Business communications to the Publishers, at the Office, 74, Great Queen-street, W.C.*

All Remittances, Cheques, and Post-Office Orders should be made payable to Messrs. Wyman & Sons.

⁂ All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.

All Letters or Queries to the Editor which require attention in the current issue of KNOWLEDGE, *should reach the Publishing Office not later than the Saturday preceding the day of publication.*

(I.) Letters to have a chance of appearing must be concise; they must be drawn up in the form adopted for letters here, so that they may go untouched to the printers; private communications, therefore, as well as queries, or replies, to queries (intended to appear as such) should be written on separate leaves.

(II.) Queries and replies should be even more concise than letters; and drawn up in the form in which they are here presented, with brackets for number in case of queries, and the proper query number (bracketed) in case of replies.

(III.) Letters, queries, and replies which (either because too long, or unsuitable, or dealing with matters which others have discussed, or for any other reason) cannot find place here, will either be briefly referred to in answers to correspondents, or acknowledged in a column reserved for the purpose.

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

"God's Orthodoxy is Truth."—*Charles Kingsley.*

Our Correspondence Columns.

VEGETARIANISM.

[314]—In the last number of your interesting journal, I observe that F.R.A.S. desires a question answered relating to myself (whom he has referred to by name as a reputed advocate of vegetarianism); to wit—Am I also a vegetarian by practice?

Most unwilling as I am to obtrude unnecessarily any opinions of mine, still more to speak publicly of my personal habits, I cannot permit any such misapprehension respecting them to pass current, as the foregoing inquiry implies.

First, as to my opinions respecting an exclusively vegetarian diet for man: these have been already so distinctly stated in a little work, entitled "Food and Feeding," which has had a wide circulation, that I am astonished to learn that any one can describe me as a vegetarian; supposing that term to denote one who desires to restrict himself or others to food composed of cereals and vegetables. If the term does not mean that, it is wrongly used; for no consumer of eggs, milk, cheese, or butter has the slightest claim to the title—a remark which ought to be quite unnecessary, but is not so.

To this little book permit me to make the briefest possible reference. At page 21, I give my reasons why man should be regarded as an "omnivorous animal," and why there is no *à priori* ground for "limiting his diet to products of either kingdom exclusively." I go on to show that the relative amounts of animal and vegetable constituents in diet vary according to the climates, hot or cold, in which man lives. But at page 27, I express a belief that Englishmen generally eat more animal food than is desirable for health; that meat, fish, eggs, and milk are generally essential for those who perform much mechanical labour; while fish and lighter flesh are better adapted to brain-workers, &c.

To this I need add nothing here.

Secondly, as to practice. It is quite true that I think it worth while to devote some attention to the growth of vegetables, and to furnish from my garden an ample supply of fresh produce, such as I can obtain nowhere else. But although thus enabled to enjoy the luxury of fresh green food in variety daily throughout the whole of our long winter, I am glad to consume fish and flesh of all kinds in addition, although, probably, in smaller proportion than most people believe to be necessary. Let this suffice in reply.

But now, let me further add that nothing can in my opinion be more unwise than to debar ourselves from the right to utilise food of any and every kind. Human stomachs differ so widely—why not as widely as facial physiognomy?—that an unlimited dietary is really the last thing a man who has so much to do with digestive laws and tastes (my own and others) can afford to dispense with. For diet, to be wholesome, should be varied for all; but it must also differ for each, in relation to his habits, sedentary or active, whether he is occupied with muscle work or brain work, &c. Diet, too, must differ, not merely for the young and old, but has to be changed for different epochs of life, as any one who has observed the subject closely, or himself has lived a good many years, must of necessity have learned. How much might be said on this head. And, in presence of various ailments of the body, how complex becomes the subject sometimes! How little all this seems to be taken into account by people who would limit our resources in the matter of food. It follows from all this, also, that I should be the last to deny that some constitutions thrive better on vegetables and cereals alone, than by admixture with animal food, since this is one of the many variations which nature produces. And I think I might add that an exclusively animal diet would probably (in our temperate zone, observe) be still more rarely found the best for any man, although it might, perhaps, be so for a very few.

So many persons hastily conclude that what is best for them is necessarily best for all the rest. There is no greater error, and none of us can be too watchful against its influence.

35, Wimpole-street. HENRY THOMPSON.

[315]—I am much surprised that "A FELLOW OF THE ROYAL ASTRONOMICAL SOCIETY" (letter 215, page 362) should consider that the idiosyncrasies, or some of the idiosyncrasies, of the Irish are due to their feeding on potatoes. This seems to me a very absurd statement to make, but I can put no other construction on his words. I am sorry that he is not more definite, and does not say exactly in what way this diet can influence Irish character. If this statement were worth consideration, I should ask him to show that the Irish character has changed since the introduction of this tuber. I believe that whether you feed a man on potatoes or on beef, if an Irishman he remains an Irishman, and if an Englishman he has still the characteristics of an Englishman.

Supposing the charges made against potatoes and rice were substantiated, it would not militate against vegetarianism. I consider that they are very unsuitable articles of food to live almost entirely on, and one reason is this: We require a certain amount of nitrogenous food (of which albumen may be taken as the type). Dr. Lyon Playfair prescribes 18 parts of carbo-hydrates to 1 of nitrogenous and 1 of fatty matter. This, in percentages, is 78, $17\frac{1}{2}$, $4\frac{1}{2}$ respectively. Now, I do not think fat necessary, as the carbo-hydrates can and are converted into it in the body. This makes $82\frac{1}{2}$ of carbo-hydrates and fat, to $17\frac{1}{2}$ of nitrogenous matter. Neglecting the water, indigestible fibre, and mineral matter, the proportion of these two in a few foods are—Potato, 88 to 12; Patna rice, 92 to 8; fine Scotch oatmeal, 82 to 18; wheat flour, 86 to 14; lentils and haricots, 70 to 30. It will be seen that potatoes and rice are deficient in nitrogen, so that, to supply a sufficiency of this, a superabundancy of other matter has to be taken.

A FELLOW OF THE CHEMICAL SOCIETY.

INTELLIGENCE IN THE DOG.

[316]—I have a very fine, large dog—a cross between a Newfoundland and a retriever—that really at times astonishes me by his intelligence. I have also—to make the story complete—a Skye terrier, that is not over kind with his mate. Last summer, during hay-time, a neighbour came to assist me one day in carting hay, bringing with him a sheep-dog. This dog, whenever the cart stopped in gathering up the hay, went and lay down under it. The Skye terrier thought it might do the same, but this was too great an intrusion to be endured by the sheep-dog, so a furious fight ensued under the cart, the sheep-dog eventually dragging the Skye into the open field. Standing about fifty yards from the cart, and thinking the little rascal about to be badly used, I began to run for the scene of danger. The large dog, who was near me, saw the danger in which his mate was involved as well as I did, bounded off at full speed, up with his right fore leg, and hit the cur such a stroke over the side as sent him rolling twice over. The Skye flew to me, evidently thankful to his powerful friend. The cur retired beneath the cart.

The same dog, only a few days ago, showed to my mind a wonderful amount of sense. Two curs were fighting furiously, the blood flying in all directions. Two men were trying to separate them, each seizing one by the tail. No sooner, however, were they let go, than they at it again. This dog was at the distance of a hundred yards at least, with a hedge between him and the combatants, but so placed that he could see them. He looked for a

while, but seeing the effort of the men ineffectual, he bounded off at full speed, and by means of his powerful forelegs he sent one dog flying to the left and the other to the right in wild confusion. There he stood, with the dogs at a distance, as much as to say—Dare to come again, either of you! No further attempt at fighting occurred. He is a very powerful dog, about 7 stone weight, and is excellent in clearing the road of cattle and sheep before the trap. He rarely uses his teeth, unless attacked, his forelegs being his chief weapons. Thomas Fawcett.

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[317]—Let me add to the instance given on page 269 that a dog living at a boarding-house, at 51, Sussex-gardens (where I used to live), laughs in the most human fashion, showing all his teeth. This, with him, is a greater sign of pleasure than wagging his tail.

P.S.—Is it not worth your while to correct the impression that your correspondents have, that Darwin is to be accredited with being the first biological evolutionist? Cannot you put Herbert Spencer's position in this regard right? J. H. B.

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[318]—I have a little black dog, which, if put out of the room, the door shut, and a ball, or even a stone, hidden, will, when let in, make a thorough search for it, first making a circuit of the room, smelling and looking the while, until he finds it, which he accomplishes in a very short space of time, being as "proud as Punch" when he has recovered it. The second time he comes in he will immediately go to the place where the stone was last hidden. Will he not reason that as the stone was there last time it will be there now also? The other day I hid it in my pocket, which he found in due time. Two days after I hid it again; as soon as he was let in he put his nose right into my pocket. One can imagine him finding a ball by its smell, but how is it that he can find a stone sooner than a human being could? Gertrude A. Fryer.

ELECTRICAL IMAGES (228).

[319]—We have

$$P_b\left[q^2_{ab}-q_{aa}\,q_{bb}\right]=E_a\,q_{ab}-E_b\,q_{aa} \qquad \text{(i.)}$$

and expanding in powers of b, and neglecting b^3

$$q^2_{ab}=\frac{a^2b^2}{c^2}\,;\quad q_{aa}\,q_{bb}=ab+\frac{a^2b^2}{c^2}+\frac{a^2b^2}{c^2-a^2}$$

$$\therefore\; q^2_{ab}-q_{aa}\,q_{bb}=-\left[ab+\frac{a^2b^2}{c^2-a^2}\right] \qquad \text{(ii.)}$$

and again

$$E_a\,q_{ab}-E_b\,q_{aa}=E_a\left[-\frac{ab}{c}-\frac{a^2b^2}{c(c^2-a^2)}\right]-E_b\left[a+\frac{a^2b}{c^2}\right] \qquad \text{(iii.)}$$

Hence

$$P_b=\frac{E_a\left[\frac{a}{c}b+\frac{b^2a^2}{c(c^2-a^2)}\right]+E_b\left[a+\frac{a^2b}{c^2}\right]}{ab+\frac{a^2b^2}{c^2-a^2}}$$

$$=\frac{E_a}{c}+\frac{E_b}{b}\left[\frac{a+\frac{a^2b}{c^2}}{a+\frac{a^2b}{c^2-a^2}}\right]$$

$$=\frac{E_a}{c}+\frac{E_b}{b}\left[1+\frac{ab}{c^2}-\frac{ab}{c^2-a^2}\right]$$

$$=\frac{E_a}{c}+E_b\left[\frac{1}{b}-\frac{a^3}{c^2(c^2-a^2)}\right] \qquad \text{(iv.)}$$

which is the required result. R. Staveley.

DARWINISM.

[320]—If the opponents of Darwin's theory will consider with care the following, from the pen of Prof. Fiske, of Harvard College, they may, perhaps, be convinced. He says, in "Cosmic Philosophy," that the Darwinian theory, when analysed, resolves itself into eleven propositions, nine of which are demonstrated truths; the tenth a corollary to the nine predecessors; the eleventh a perfectly legitimate postulate.

1. More organisms perish than survive.
2. No two individuals are exactly alike.
3. Individual peculiarities are transmissible.
4. Those individuals whose peculiarities bring them into close adaptation with the environment, survive and transmit their offspring.
5. The survival of the fittest thus tends to maintain an equilibrium between organisms and their environment.
6. The environment of every group of organisms is steadily, though slowly, changing.
7. Every group of organisms must, therefore, change in average character, under penalty of extinction.
8. Changes due to individual variation are complicated by the law that changes set up in one part initiate changes in another.
9. These are further complicated by the law that structures are nourished in proportion to their use.
10. The corollary follows:—That the changes thus set up and complicated must alter the specific character of any group of organisms.
11. The only postulate] Let it be granted that, since the first appearance of life, time enough has elapsed to produce all the variation of species now seen. C. T. B.

ARRANGED SQUARES.

[321]—Your correspondent, E. V. R. (letter 231, page 273), says: "I believe these squares may be arranged by placing the diagonal numbers in what I may call their *natural* squares in the first instance, and working up to them; but I have only *succeeded* with the square of 4."

As I have succeeded with the squares of 5 and 6, I send my solutions, which, I believe, will be found correct.

A number in any place, added to the number in the corresponding place on the opposite side of the square, will make 26.

1	20	16	23	**5**
15	**7**	12	**9**	22
21	18	**13**	8	2
4	**17**	14	**19**	11
21	3	10	6	**25**

The perpendicular, horizontal, and diagonal lines will make 65.

A number in any place, added to the number in the corresponding place on the opposite side of the square, will make 37.

1	20	33	34	17	**6**
32	**8**	30	7	**11**	23
24	35	**15**	**16**	2	19
18	10	**21**	**22**	27	13
5	**26**	9	28	**29**	14
31	12	3	4	25	**36**

The perpendicular, horizontal, and diagonal lines will make 111. Emma C. Hooton.

GHOSTS.

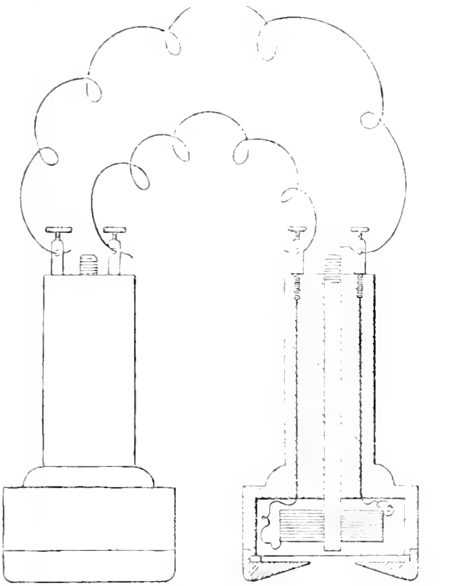
[322]—Perhaps some of your readers may be able to supply details as to the War Office ghost. All I now remember is the fact which gave the case its name and notoriety, viz., that as there was a discrepancy between the date of the apparition and that of the official return of the death, further inquiry was made, at the instance of the relatives, by the War Office authorities, with the result that the actual date of death coincided with the time of the apparition, and that the former official return was incorrect.

I do not see that instances of mistaken identity, and so on, have much bearing on a case like this. Of course, it is the strong and not the weak cases with which science has to do. T. D.

TELEPHONE.

[323]—There is no great skill, or extensive apparatus required, in the construction of the ordinary Bell telephone; and I can promise Mr. Mortimer (Query 222, p. 303) complete success, if he will carefully work out the following instructions. The telephone consists of a small permanent magnet, a coil of fine silk-covered copper wire, a diaphragm of very thin iron, and a case for containing and fixing the whole.

The magnets may be from 4 to 6 in. long, and $\frac{3}{8}$ to $\frac{1}{2}$ in. in diameter, and will cost per pair from 2s. to 2s. 6d. The coils of fine wire, the same as magnets, and the iron, which is known as ferrotype, about 1d. per pair. The cases, if bought ready turned, will cost about 6s. the pair. Now, as this is the most expensive item, I will show that with a little ingenuity this may be overcome at about one-tenth the above price. At any chemist's procure a couple of empty violet-powder boxes, at the outside fourpence, and see that the lids fit tightly. Then, in the centre of the lid, bore a hole $\frac{3}{8}$ of an inch in diameter, then pare away carefully all around the hole until you get a shallow, funnel-shaped mouthpiece. Finish off with a piece of glass-paper. Now cut a ring of cardboard, $\frac{1}{4}$ of an inch wide, that will just fit inside the lid of your box. Take one of the pieces of iron, and, with a pair of scissors, cut a disc that will also fit inside the lid of box, drop in the ring of cardboard, and next the iron disc, and see that the cardboard is thick enough to prevent the disc from touching the lid anywhere but at the edges, the middle being free to vibrate. In the bottom of the box, bore a hole just large enough to take the magnet easily. You now want some sort of a support for the rest of the magnet, and also to form a handle. This can be formed from a round piece of wood about 3 in. long and 1 in. in diameter, with a hole through the



centre lengthwise to take the magnet. This must be glued to the bottom of the box, the holes corresponding; or, should you find any difficulty in shaping the stem, take a large cotton reel, the end of which will form a sort of flange, and will be better for glueing. You are now ready for putting the various parts together. Push the magnet up through the reel until the end projects within the box, and stands almost level with the edges of the box without the lid; slip the bobbin of wire over end of magnet in box, and fix it so that about $\frac{1}{16}$ of an inch of the magnet is above the reel or bobbin. Pass the two ends of the fine wire through bottom of box, and solder to two brass binding screws fixed somewhere on the case, the proper place being at the end of them. Put the lid (with diaphragm and ring in place) on the box. The diaphragm will now be firmly pinched between the lid and box, and should be just clear of the end of magnet. Your telephone is now complete, and can be polished, or varnished with a spirit varnish, and when dry is ready for use. To connect, simply take a wire from each binding-screw on one telephone, and connect to each binding-screw on the other, and the instruments will speak. But beware of a very common error, and don't try to speak into one and hear your own voice in the other. This is impossible. Have at least a dozen yards in each screw, and leave one instrument with a friend while you take the other into another room. Speak distinctly into the mouthpiece, and when hearing keep the mouthpiece tight up to your ear. For connecting, use cotton-covered copper about No. 20.

Should the voice seem very distant, move the magnet nearer the diaphragm. If no voice is heard, you will most likely find the magnet sticking to the diaphragm, the proper distance being about the thickness of a piece of stout paper.

As now used and sent out by the different telephone companies, the telephone is never used both as a receiver and transmitter. The transmitting instrument being a microphone of peculiar construction, this adds greatly to the utility of the instrument, as you are able to carry on conversation without removing the receiving telephone from your ear, all the speaking being done at the microphone.

I shall be most happy to send a description of the microphone, both simple and as a transmitter, as made by G. E. V.

PROBABILITIES IN CARD DRAWING.

[324]—Chas. A. Edes writes, with reference to the problem in probabilities at p. 301, that the cards are not shuffled between the cuts, so that the cutter, if he fails the first time, has a rather better chance next time, because he will not cut in the same place again, and similarly for the third trial. To solve the problem in this form, treat it as though the card cut were removed from the pack at each failure. The solution would then run thus:—

The chance that a winning card will not be cut the first time is $\frac{10}{13}$. In that case there remain 51 cards, of which 12 are winning ones. Thus, the chance that there will be a second trial, and that a winning card will then not be drawn, is $\frac{10}{13} \times \frac{39}{51}$. There then remain 50 cards, of which 12 are winning ones; and the chance that there will be a third trial, and yet again a winning card not be drawn, is—

$$\frac{10}{13} \times \frac{39}{51} \times \frac{38}{50}$$
$$= \frac{38}{85}$$

The odds in favour of drawing one of the winning cards at one of the three trials are therefore 47 to 38, instead of the smaller odds 1,197 to 1,000. It is obvious, of course, that in the closing sentences of the examination of the problem at page 301, I should have written "in favour of *A*" instead of "against *A*." EDITOR.

CENTRIFUGAL FORCE AND AN OPTICAL ILLUSION.

[325].—When a piece of twine is attached at one end to a ring, and the other end, held between finger and thumb, twisted and untwisted rapidly, the ring, when it has acquired sufficient velocity, spins horizontally in either direction, according to the twist given to the twine, and the eye is deceived by the appearance of a second string attached to the opposite side of the ring, the ring and two strings forming a perfect cone, whose apex touches the finger and thumb. A gutta-percha ring, about 2 in. in diameter, shows this simple experiment to advantage. E. C.

[This is a very pretty illusion. If the weight of ring is small, we see a nodal chord, generally a third of the way up. The apparent cone is not perfect. It is easily seen why the cone seems bounded by two distinct strings.—ED.]

SIGN OF "BEAR AND RAGGED STAFF."—Shakespeare refers to this as the crest of Warwick, "the king-maker," in 2 Henry VI., act v., sc. 1, where Warwick is made to say:

"Now, by my father's badge, old Nevil's crest,
The rampant bear chain'd to the ragged staff."

—W. MIDDLETON BUTTERS.

INTELLIGENCE IN ANIMALS.—About twelve years since, when I was living at Richmond, I had a fine black retriever. She was an exceedingly docile, tractable animal with people, but always shunned animals, even those of her own species. One day, walking with her through the streets of the town, she was persecuted by a large, coarse, ill-bred, bull-dog; she snapped at him several times. Presently I turned down to the water-side; to my surprise, here she began playing with the dog, and soon after she gambolled about in shallow water with him, occasionally swimming. All at once he got out of his depth, and, to my astonishment and dismay, she got him by the nape of the neck and kept his head under water. I shouted "Down, Prin, down!" but of this she took no notice, until she had almost suffocated him; then she let him go, and he dragged himself out of the water, and slunk away with his tail between his legs, and went his way, a sadder, and I hope a wiser, dog.—J. BROWNING.

Queries.

[287]—[illegible] A Powder.—Can anyone give me Dr. [illegible]'s prescription for inhalation of [illegible] bismuth, acne a powder and [illegible] to be taken in the form of snuff?—Affected S[illegible].

[288]—Plants in Bedrooms.—Are growing plants unhealthy in the bedroom? If so, should be glad to know the reason.—J. C. L.

[289]—The Burial of Moses.—Where can I obtain the poem entitled "The Burial of Moses," or where does it occur?—J. C. L.

[290]—Tertiary Fossils.—Can any of your readers explain how it is pyritous fossils from tertiary beds are so apt to fall to pieces, while those from secondary strata remain unaffected by exposure to the air? I have examples of ammonites, &c., in my cabinet that have been there for years, and, although quite "brassy" in appearance from pyrites, yet remain entirely unchanged.—W. D. C.

[291]—Razor.—Why does a razor cut better after it has been dipped in hot water? This query answered on philosophical principles will oblige.—R. Joseph Boulton.

[292]—Silver.—How can I melt the residue of old silver baths so as to obtain pure silver? Can it be done in any way without using a crucible? How much can I obtain a crucible for?—F. A. B.

[293]—Smelling Salts.—Is the use of smelling salts beneficial or otherwise? Why are they resorted to profusely by women, but not by men?—F. M.

[294].—Exceptional Seasons.—How is the exceptionally severe winter of 1880-81, and the contrast in the exceptionally mild winter of 1881-82, to be accounted for?—F. M.

[295]—Apparent Paradox in Probabilities.—A bag contains an indefinitely great number of marked tickets, the nature of the marks being unknown. One hundred tickets are drawn. These all bear letters of the alphabet, viz., 50 bear A, 30 B, and 20 C. Then (v. Lubbock and Drinkwater Bethune's "Probability," p. 27), the chance that the next ticket drawn will bear a letter of the alphabet is $\frac{101}{102}$; but the chance that it will bear A or B or C is $\frac{103}{104}$. That is to say, the chance of the next mark being one of the 3 letters A, B, C, is greater than the chance of its being one of the 26 letters A to Z. Where is the fallacy in the reasoning?—Gradatim. [In the former case we have the probability that an event will be either of one kind shown to be prevalent in a certain degree, or will not be of that one kind. In the latter we have the probability that it will be one of three kinds shown to be prevalent in certain degrees, or will not be of those three. The evidence for three distinct kinds of marking gives stronger reason to believe that the next will be one of those, than the evidence of one sort of marking gives in favour of the next belonging to that one kind. The paradox seems to arise from this, that in calculating the chance of the next being a letter, we do not take into account the evidence tending to show that there are three prevalent letters in the bag. See also De Morgan on "Probabilities."—Ed.]

[296]—Photographic Studio.—I wish to construct a reasonably inexpensive glass-house, and shall feel obliged by a few hints as to (a) dimensions, (b) material for walls and roof, and (c) arrangement of light, having regard to the fact that the studio will have to join the back of my house, which has a S.W. aspect. A reference to any work on the subject will also oblige.—Amateur.

[297]—Limelight Apparatus.—Where could I best obtain one cheap, suitable for illustrating lectures to workmen with, and what would be about the probable cost of the instrument complete?—W. R. L.

[298]—Our Ancestors.—We are told that the palæolithic man who inhabited Britain was black in Mr. Grant Allen's interesting article. How is this known to be so? There is no evidence offered in the article, and I should be glad to know how this can be proved.—Homo Palæolithicus.

[299]—Magic Lantern.—Is it possible to make a magic lantern of wood suitable for an ordinary room? If so, could you kindly give directions, or mention some book (with price) on the subject?—Amateur Carpenter.

[300]—Does the learning by heart of prose or poetry improve or impair the faculties?—A. C.

[301]—Magic Lantern.—I want to make a lantern for scientific demonstration. Can any reader tell me the size, focal length, and distance apart of the glasses to be used in making one to take photographic slides?—F. D. H.

[302]—Vegetarianism.—Can you tell me of a good book hereon? I know not what vegetables to eat, or how to cook them.—John Allen Ollard.

[303]—Destruction of Sodom and Gomorrah.—A clergyman recently, in [illegible] stated that it had been almost conclusively proved, on scientific grounds, that these towns were destroyed by a shower of meteors on July 31, 1898 B.C. Is there any proof of this, and, if so, what is the proof?—A. N.

[304]—Geology.—Can any geological reader recommend a good work on the carboniferous limestone formation, which contains reliable information as to fossils; more especially as regards the North Wales and Shropshire rocks?—A. N.

[305]—The Calculus.—What book should be read on the calculus after Todhunter and Williamson? Is Price's "Infinitesimal Calculus" a good book?—Mathematicus.

[306]—Scholarships at Cambridge.—Are any open to candidates over twenty years who have not already entered?—Mathematicus.

[307]—Descriptive Geometry.—Required, a work on this subject, sufficient for a first class in fourth stage mathematics, that is, fulfilling the following syllabus:—Representation of points, straight lines, and planes, by projections and traces on two orthogonal planes. The use of auxiliary projections and rabatments. Graphic solutions of problems concerning straight lines and planes, their intersections, inclinations, &c. Problems on trihedral angles. Your paper supplies a want in educational literature.—Self-Taught.

[308]—Quicksands.—Why do persons sink in quicksands deeper than they would in water, and why can they not float as in water? Does the sand in a quicksand float in the water, and how is that compatible with the relative specific gravities of water and sand? If not, why does not the sand sink in the water?—John R. West.

[309]—Effects of Tobacco.—Will Dr. Muir Howie kindly explain why persons suffering from asthma are sometimes recommended to smoke tobacco? I gather from his interesting paper that the use of the narcotic would tend to lessen the reserve of vitality so necessary to resist the effects of violent attacks of coughing.—J. W. Brookes.

[310]—Quartz in Coal.—I have in my possession a specimen of coal to which a piece of quartz is closely attached, several layers of the same rock also running through the coal. How can this be explained?—J. W. Brookes.

Replies to Queries.

[220]—Chemical Analysis.—Might I say that before buying the new editions of Fresenius on Qualitative and Quantitative Analysis—which, as one of your readers has rightly observed, are rather too descriptive to be of use to a beginner—he should try and obtain the previous edition, which the translator cut down to about half its original volume, thereby making it as valuable to the novice as it is to the chemist. Your correspondent must not be surprised if the second-hand books should be as expensive as the new books.—F. C. S.

[258]—Drying Wild Flowers.—"H. R. S." should take to Egypt and Palestine with him a quantity of botanical drying paper (to be had of any scientific dealer), and two or more strong wooden boards of same size as paper; also strong leather straps. The plants should be spread out as naturally as possible between the sheets of paper. Then lay all between the boards, and put one or two large and heavy stones (to be had at most places) on the top. Try to keep some of the paper not in use, so that it may be changed for that in use, which becomes damp from the moisture in the plants. This changing should be made every second day, if possible. When travelling from place to place, strap the whole firmly together.—F. W. G.

[258]—Drying Wild Flowers—page 361.—I should recommend "H. R. S." to get a small 5s. book by J. L. English, entitled "A Manual on the Preservation of the larger Fungi and Wild Flowers," just published by A. B. Davis, of Epping. There is a small collection of wild flowers in the Norwich Museum preserved by this process, in which both colour and form are beautifully retained.—R. S. Sandon.

[261]—Letts's "Popular Atlas Geographical Map of England and Wales" (with leading railways), will give "Strata" the information he asks for. Also Professor John Phillip's "Geography of Oxford and the Valley of the Thames" will be of great assistance. A very bright and clear geographical map (of England and Wales) is prefixed to Mr. H. B. Woodward's "Geology of England and Wales." Letts's map is a very cheap one, and is the work of Mr. Bristow: it is founded on Murchison's map. Should be glad to help "Strata" in any way I can.—Jacurat.

[267]—Thoracic Integrity.—The statement in "Science for All" is perfectly authentic. It is really not correct to regard the chest cavity as being air-tight, as, of course, can at once be under-

stood when we think of the ease with which air can be drawn into and expelled from it. The lungs are to be regarded as elastic bags which are not connected with the walls of the chest, both they and the walls being covered with a smooth membrane called the *pleural membrane* (wh ch, when inflamed, produces pleurisy). Now, the space (or rather separation, for they are nominally in contact), between the two layers of pleural membrane, *is air-tight*, so that when the chest cavity enlarges (by movement of ribs and descent of midriff, or diaphragm), the elastic lungs follow its walls, and so air rushes in through the wind-pipe to fill the largar space so produced, and we are said to inspire. If, however, a hole be made into the space (or separation) between the lungs and chest wall, then, of course, when the chest cavity enlarges, the lungs will not expand, but air will simply rush in at the hole that has been made. But the space between one lung and the chest-wall is quite distinct from that between the other lung and the chest-wall, so that if only one be opened into, the man still lives and breathes with his other lung; but if both spaces be opened into death must at once occur. Now, the heart is, as it were, quite distinct from these air-tight spaces, so that if a hole exist (as has been known in certain cases) through the chest-wall opposite that organ, it will not open into either of the spaces, and so will not interfere with breathing.—F. W. G.

[268]—PHOTOGRAPHY.—"Anon" has need to be careful in meddling with photography. It is an expensive pastime. For general work (landscape and portrait) consult Abney's "Instructions in Photography" (2s. 6d., Piper & Carter), or Hughes' "Photography" (1s., Simpkin). If only for portraits, Heighway's "Practical Portrait Photograph" (1s., Piper & Carter) will best suit him. Full instructions and chemicals required are given in above works, and the advertisements will indicate where to get them. The "cheapest articles" are not to be advised. Failure and disgust are sure to follow the use of cheap and necessarily nasty articles. Better give a higher price and get a good article. If it is intended to use dry plates, Eder's 'Modern Dry Plates" (3s., I think, published by Piper & Carter) will be useful.—F. M.

[269]—Carbonic acid was shown by Calvert to be necessary to the rusting of iron, besides oxygen. Sea-water contains more carbonic acid than average fresh water.—C. T. B.

[277]—Balmain's luminous paint is calcium or barium sulphide, made by heating either gypsum or heavy-spar with coal.—C. T. B.

[285]—SCIENTIFIC TERMS.—"Prester W." will find Dunman's "Glossary" (Griffiths & Farran), a useful book for "Biological, Anatomical, and Physiological Terms."—CHAS. W. DUCKWORTH.

[300].—The question of "G. G. D.," No. 300, p. 388, is a very fair one. Assuming that the evaporation from the damp surroundings have saturated the warm air inside the room, a condensation of half its vapour would certainly take place if it were reduced from 50° to 32° without any interchange with the outer air, *i.e.*, if the room were air-tight and cooled exclusively by conduction through the walls. The damp surrounding would then simply recover all the moisture they had previously supplied to the warm air. If, on the other hand, the doors and windows were thrown open, and the room were rapidly cooled by an exchange of cold air from outside, the walls would continue for some time warmer than the incoming air, and therefore would receive no deposition of moisture from it; but on the contrary, would communicate some heat to it, and thus have a drying action. I have imagined these opposite extremes as exaggerated illustrations displaying the principles. Practically, however, a mixed action occurs. Conduction takes place through the thinnest element of partition, the window glass, and there we see an abundant formation of what I may venture to call domestic dew. Besides this, an interchange of atmosphere slowly takes place; but not more slowly than the cooling of the walls. Under these conditions, the condensation of excess of vapour is limited to the window panes, and the brick or stone walls, &c., remain dry.—W. MATTIEU WILLIAMS.

[Letter 220]—HEALTH OF NAVVIES.—Edwin Sachs (p. 325), in reply to my letter on the above (p. 254), says that the "natives" are dying by thousands in Java, &c., from *fever* "though inveterate betel chewers." It would have been more satisfactory if Edwin Sachs had informed the readers of KNOWLEDGE whether the *fever* was indigenous or not, as this would, of course, make all the difference, and would account for the, at present, remarkable fact of the "natives being more easily affected by fever than are Europeans." If Edwin Sachs will read my letter again, he will see that I was alluding to fevers, &c., incidental to marsh life. Mr. A. H. Church, in his edition of "Johnson's Chemistry of Common Life," never intended the betel nut to be used as a preventive against fevers which contact with foreigners may have introduced.—F. C. S.

Answers to Correspondents.

•• *All communications for the Editor requiring early attention should reach the Office on or before the Saturday preceding the current issue of* KNOWLEDGE, *the increasing circulation of which compels us to go to press early in the week.*

HINTS TO CORRESPONDENTS. 1. *No questions asking for scientific information can be answered through the post.* 2. *Letters sent to the Editor for correspondents cannot be forwarded; nor can the names or addresses of correspondents be given in answer to private inquiries.* 3. *No queries or replies savouring of the nature of advertisements can be inserted.* 4. *Letters, queries, and replies are inserted, unless contrary to Rule 3, free of charge.* 5. *Correspondents should write on one side only of the paper, and put drawings on a separate leaf.* 6. *Each letter, query, or reply should have a title, and in replying to letters or queries, reference should be made to the number of letter or query, the page on which it appears, and its title.*

MARK H. JUDGE. Thanks for report of Sanitary Association, but readers would not allow us to insert so much in way of report. E. LUXMOORE. Many thanks for description of compound pendulum, which shall shortly appear. The curves are very interesting. Is there no ink, either black, scarlet, or orange, which could be used? If there were, the curves could be photozincographed.—COGITO. When, in No. 11, I spoke of the ecliptic or plane of earth's path as unchanging, I meant that if an observer were supposed to watch the earth from the sun's centre, her apparent course among the stars would be unchanging. The earth's polar axis remains inclined at almost an unchanging angle to the plane in which the earth travels, just as the axis of a spinning top is inclined at almost an unchanging angle to the horizon plane, but this axis varies in direction (also as the axis of a reeling top varies) and the plane of the equator, which is always at right angles to the earth's axis, of course varies with it. Refer to the same figure p. 219, No. 11; here EE corresponds with the earth's equator. It remains always inclined at the same angle to the vertical, but reels round as the globe spins. I have not yet done with precession, but every week I get some such message as this: "We have forty-seven columns of over-matter, and two, three, or four more pages of advertisements than we can get in without displacing something. What are we to do?" and I have to reply, Keep out my article on comets, or on precession, or on the Pyramid, or Foster's articles on illusion, or articles by some one else who is willing (I know) to wait.—J. R. CAMPBELL. We should have liked to use your paper on the Slide Rule, but what can we do? A wants more about microscope; B more about chemistry, C botany, D entomology, E says turn out whist, and have in mineralogy, F says we want no chess, but would like something about pottery, G would like more biology, H palæontology, J says "All work and so little play makes KNOWLEDGE dull to day. Why cut down whist and chess to a column each?" and so on, to Z. You catch the idea?—M. HILL. Neither drawing represents the real changes of the moon's apparent position. If you attach a circular disc to a celestial globe, set to latitude of London, and carry the disc round from horizon to horizon, you will see the real changes, and also why they occur. A gas is said to be in the critical state when the pressure and temperature are such that, if the latter were in any degree lowered, the gas would liquefy.—F. W. B. BOUVERIE. Fear we could scarcely find room in KNOWLEDGE just now for papers on philological subjects.—J. H. FALLON. Know nothing about the Society for the Promotion of Scientific Industry.—W. B. Are not -3 and -2 as obviously roots of the modified equation?—S. STANIER. Power cannot be 200, if Jupiter had at the time of observation apparent diameter 45″. Three days before date of your letter, Jupiter's diameter was only 36″.—PRIVATE STUDENT. Your examples are rather too commonplace to be dealt with in Mathematical Column. (1.) The equations to lines parallel to $y=mx+b$, and at distance a from origin (rectangular co-ordinates), are $y=mx\pm a\sqrt{1+m^2}$. (2.) If AB is parallel to $x\cos. \alpha+y\sin. \alpha=p$, and $OA.OB=c^2$, equation to AB is $x\cos. \alpha+y\sin. \alpha=\dfrac{c\sin. \alpha}{\sqrt{\tan \alpha}}$. (3.) If AB be parallel to $y=mx+b$ and $OA+OB=c$ the equation to AB is $y=mx+\dfrac{cm}{1+m}$. But you really must not ask such questions as these; you mistake the purpose of KNOWLEDGE in doing so.—HISTORIAN. Your questions hardly suited to a journal of popular science. Considering how those two historians have denounced each other, it would be unwise of us to pretend to decide between them.—W. A. S. Your reply states the matter clearly, but does not explain. Mesmerism may be "nerve force producing artificial somnambulism on a susceptible subject." But why? or (if we cannot have Why) how?—J. W. WOOD. Your reply scarcely explains much.—H. W. B. Thanks; but fear the merely verbal contest you invite would be of small benefit.—GEO. ST. CLAIR. The star Alpha Centauri; moreover, this is the only first magnitude star which ever has shone in the direction of the Southern passage. The Great Bear was regarded

even in much later times than those you mention, as the polar constellation, except by the Phœnicians, who took the smaller circling of the Cynosure for their guide. The motion of the apsides in no way affects the precessional period. T. W. Hope one day to give such an article.—W. St. C. Boscawen. A paper on Chaldean Festivals only delayed. "Omittance is no quittance." Spectrum. We can, however, live in air rarefied enough to support combustion so ill that a bad fire would go out in it. C. T. B. Nay, they do not neutralise each other; the parts illuminated on the slant look less bright, emission diminishing as cosine of angle with normal to surface. Taking short for medicine must, we agree with you, be a dangerous practice, though not necessarily very dangerous. Luckily, the ... does not get all it might get, or scarcely a child would grow to maturity. —Unsatisfied. The pressure upwards is equivalent to weight of a column of air as large as mouth of glass, and 100 miles or so high (reaching, in fact, to the limits, if such there be, of the atmosphere): the downward pressure is that of the wine-glass full of water. A column of water about 30 feet high can be supported in this way, a column of mercury about 29 inches high; so that you might fill your wineglass with mercury, if suitable substance were placed over it. Jumbo. The fancy is a queer one. I can see that it might prove very useful in keeping account of numbers. Like you, I have the numbers in sets from 1 to 12, then to 20, then to 30, then to 50, but after that I do not go straight to 100 but stop at 80. They do not, however, go in a bent line, as with you, but in a set of verticals. W. A. F. Have never heard that the Jordan was for a time stopped up by masses of rock falling down, but as half Niagara was once for a while stopped (so they say), it seems conceivable that the same might happen to another river. Hope your question is not meant to infringe our rule about science and religion.—F. Hepbourn. You could only now obtain Hutton's "Recreations" at a secondhand bookshop (unless, perhaps, our exchange or sixpenny sale column might help you.—Excelsior. Oouff! here comes a long one! First, you are quite right. The objection was first advanced by Tycho Brahe: the answer is that the orbit of the earth is so utterly insignificant compared with the distance of the fixed stars, that no such effect can be recognised, except in the case of the very nearest stars, and then only with the most powerful and closely-measuring telescopes. Thus, Alpha Centauri, being only at the trifling distance of 20,000,000,000,000 miles, describes an ellipse having a major axis nearly 2 seconds of arc in length, one-930th of the moon's apparent diameter. 2. Twinkling of fixed stars due to moisture in our air. 3. Do not know why expeditions are not made now to South Pole; conditions there probably not similar to those at North Pole—more ice, Maury says more land. It is also a longer journey, except from places in southern hemisphere. Uranus is now very favourably situated for observation, and visible to naked eye. 4. The satellites of Mars are utterly beyond the range of a 3-inch achromatic. 5. Spots oftener seen than not, except at the time of minimum, when sometimes for months together none are seen. 6. Cannot show planet's paths now the whole sky shown in a single map. For reasons we are obliged to keep the block free from anything not belonging to the stellar heavens. The zodiacal map does better in every way. To see Uranus or Neptune to the least advantage, a good telescope is wanted, and if a map is trusted to show where the planet is, it must be on a much larger scale than our star map. This applies also to the nebulæ, except those visible to the naked eye. Am glad to hear you have been so thoroughly well pleased with the telescope you obtained from Mr. Bateman for the small price of £5. 15s., "being and doing all that is stated of it in the advertisement, and more too."—Anxious One. If there is the danger you fear, it is far too serious a matter to be trifled with. We should be wronging you if we inserted your letter for casual reply, and we have no right to ask medical men, especially mind doctors, to discuss the matter. Indeed, we know they would not do so (because *they* know they could not properly do so) without careful study of the case. Pray believe that if we could do anything, rightly, by which your anxiety might be relieved, we would most willingly do so. One thing we can say—the symptom you describe should either lead you to seek medical advice, or you ought not to allow it to worry you at all. A doctor would probably tell you how much you should notice it, and with what object. But merely noticing it, and being worried by it, can do nothing but harm. J. O. Lindsay. Thanks for extract. The publishers will probably issue covers for binding. A yearly volume would be too thick. Taking twenty-two pages per number, without advertisements, there would be 1,144 pages: far too many for a single volume.—James Dickinson. Thanks for the microcosm; but the article on Sound is written by one who understands not.—J. V. M. You seem to misunderstand my remark, that one of our contributors was "not writing for" a correspondent (who objected to his reasoning). No sneer as to that correspondent's capacity was intended. I trust I am incapable of such rudeness. I meant simply what I said, that Dr. Wilson's reasoning was not directed to meet the particular objection urged by that objector. You think scientific men behind each other too much. Is not that better than belittling each other? Would you have them like the Professor of Greek, who wished an opponent "'confounded' for his theory of the irregular verbs"? I cannot admit that Dr. Draper did not argue honestly. He was a hard hitter, but honesty itself. The case you cite is not very serious. Draper says Pilate was not answered, and Bacon said Pilate would not stay for an answer; do you know which opinion was right? You go on to object to stories about animals. You say, What would scientific men say to evidences of religion consisting of stories of pious men. Our stories of animals are not meant as evidences of science. Then you think our articles too short. But others want variety. Your letter *does* seem rather wandering, but doubtless "the gaps are caused by suppression before birth of much that you wished to say."—E. W. P. Both papers received. Thanks. Great pressure of matter only has prevented our using them yet.—R. W. J. Statement about tides in *Christian Globe* utterly wrong. Scientific authorities differ as to details, of course; but none support the ridiculous statement you quote about "geological time approaching the limits," &c.—M. H. Judge. Sorry; but letter reached us only when No. 18 was already in type.—Sirius. Statement in *Times* probably near the truth. From best estimates, Sirius gives out about 300 times as much light as sun (at same distance), which would give surface 300 times as great (if of same intrinsic lustre); diameter about fourteen times as great. Of course, this is but an estimate.—F. Matin. Thanks.—A. A. Ferguson. Smoke and fog best *seen* where light was, in cases cited; not attracted by light. The theory impossible.—C. A. C. Theory not reconcilable with evidence of former existence both of water and air on moon.—Constans. In such a subject, Dr. Ball was obliged to assume either that readers were acquainted with those laws, or would take them for granted. Your questions chiefly relate to the more doubtful and perplexing matters. You may see from my article in *Contemporary Review*, and hereafter more fully in *Gentleman's Magazine*, that there is room for considerable variety of opinions as to details.—G. S. Thanks; but answer about lightning too vague.—W. Crisp does not consider the evidence of Mallicolese skull so decisive against phrenology as Miss Buckland suggests (in passing) that it is; convolutions not stunted in growth, but forced in other directions. He notes that capacity of Neanderthal skull cannot be estimated, as it is a mere skull cap.—J. F. Lawrence. Should advise you not to use copper bowl for lemon-squeezer until thoroughly retinned.—F. Nimr. Hope you will occasionally send *translated* extracts from Al Muktataf. Of course, I understand Arabic perfectly, but "faites comme si je ne le savois pas," as M. Jourdain says.—An Engineer writes pleasantly-worded note explaining that "F.R.A.S.'s" remarks about ice had seemed to him insufficiently clear.—A. R. Sennett. Do not know; can*not* answer correspondents per letter.—Taranaki. We may hereafter publish some *southern star-maps*, but at present our hands are full. You see we italicise words in question, at your suggestion, and may hereafter adopt the system, if printers do not object.—F. F. Porrell. Red Sea was once thought to be higher than Mediterranean; disproved first by measurement, then emphatically by canal.—H. L. Macquard. Nos. 2 and 3 quite out of print; Nos. 1, 4, 5, 6, 7, 8, and 9 not very far from it, or might reprint 2 and 3; as it is, it would be useless.—F. C. S. Will try soon to find room for "Wood Gas," but "Beet" is waiting. In fact, we are choked with matter at present.—Alex. F. If you read Sir J. Herschel's essay on "Light" ("Familiar Science Essays)," you will see what a very difficult question yours is.—A. W. Buckland gives for benefit of "Experto Crede" address Professor Rudler, Jermyn-street Museum, for information about mineralogical objects.—A. J. D. saw cat on sill of fanlight over door, who presently lowered his hind legs, and, lifting knocker, let it fall, walking in when door was opened.—Daleth, T. A. Preston, R. Macpherson, and others. Thanks, but query already answered when your reply came.—W. Middleton Butters says in "Pig and Whistle" a pig = a cup or bowl, and whistle = wassail; "Bear and Ragged Stump," crest of Warwick, the King-maker; in "Magpie and Stump," mag = large coin, guinea; pie = pay; and stump = be off.—J. T. B. Ere long electrical matter will be dealt with as fully as possible.—Leo. H. Walker. Afraid to publish your suggestion, lest some mother should be tempted to squeeze "baby's" brain to make a chief of him; what a terrible thing if a father should find mamma or "nurse" with copy of Knowledge before her carefully trying to squeeze baby's head into semblance of young Mallicolese chieftain!—Amateur Botanist. Questions about preparing colours for magic-lantern slides and plant collection require articles to themselves.—Miss J. Young. Thanks, but we have a surplus of original stories.—Arthur E. Palmer. Thanks. I ought to have added that when I am pressed,

and have to work my very hardest, I always go without stimulants of any sort. In ordinary working time I am a very moderate drinker; in holiday time, like Mr. Foker, I "take my whack" with the rest; but then I do not believe in holiday-making; it means, with me, "getting out of working order."—F. A. B. Your first query a statement—namely, that coronal ring was perfect round moon at first quarter. Other query inserted; letter (abstract) also. Thanks.—SIR H. THOMPSON. I take some blame to myself, for I had read your book, and remembered well that there was therein no advocacy of vegetarianism.—M. S. Thanks.—E. G. D. Your "more than thirty notes, criticisms, and suggestions" came upon us all "too too" much at once, and many related to matters already, as we hoped, disposed of. Others have had more to complain of than yourself: but if what you wished done for you were done for all, we should want sixty pages weekly, and the paper alone would cost much more than our weekly price. What should we do? Advise proprietors to raise price? Thanks; we prefer not, if by any possibility we can avoid it. But, as you will have it so, we reply, "Farewell." Try to be a little more reasonable with the next periodical you take.—E. V. H. Fear you cannot get a really good account of the comet of 1843; the best that was written about it lies buried in proceedings of astronomical societies. The "meteoric theory of comets" (scarcely a theory now) has been dealt with fully by several writers, myself among others. It is to be considered shortly in these pages.—E. BURKE. The version given in the work you mention long since disposed of by Leverrier. —J. H. CORBETT. I think both Parallax and Mr. Newton Crosland would feel insulted at the suggestion that they are one and the same. If either could destroy accepted astronomy, the other would fall upon him. In the theory of Parallax (who is by no means the same as our too lively Hampden) the earth is not compared to a Stilton cheese more than to a Dutch cheese. The earth has only one side—the top; the north pole is the centre; there is no south pole, but in its stead we have the circumference. Dimensions I do not know. "Hampden" tells me one thing; "Parallax" used to assert another. If you quote either, the advocate of the other—whether "Hampden" or "Parallax"—tells you you know nothing of the Zetetic astronomy. If that advocate chances to be "Hampden," he calls you a lying coward, or a lily-livered, perjured villain, or something of that sort. It is a way he has. "Parallax" is very different. He is not only gentlemanly, but he is "like Cerberus, three gentlemen at once." At least, to my certain knowledge, "Parallax" was Mr. Rowbotham in 1864; De Morgan says of him ("Paradoxes," p. 306,) that at Trowbridge, in 1849, he was S. Goulden; and now he is Dr. Burley.—J. MURRAY. Yes; other notes and diagrams (gracious goodness!) received. Sorry "the Ptolemaic Decrescendo system will not allow any spots on the sun," only allowing them to pass in the same way as Venus. Astronomers are far kinder.—J. A. M. Solutions 1, 2, 3 received; hope with you, the pork is thoroughly cooked now.—R. P. T. Gregory's Electrical Theory plausible as you say, but, as you also say, quite irreconcilable with Dr. Ball's views; equally irreconcilable with laws of dynamics.—CARUS. The more hydrogen in a balloon—the hydrogen being enclosed in elastic case, so that it is nearly at same pressure as surrounding atmosphere—the greater the lifting power; otherwise, the reverse. If an air-tight case is so made as to be of constant dimensions, the more hydrogen you force into it the less will be the raising power. As to the other query, please specify the kind of work you require on palæontology—technical, popular, or what?—ELI WALLIS. If you want to see what stars lie towards, say, the south west, hold the map so that the words south-western horizon are vertically below the map's centre, then between the south-western boundary and that centre, which represents the point overhead, you will see in the map the stars you want.—FARMER. Letter marked for insertion.—J. A. OLLARI. Not a tenth of the space you want is available.—GERALD MASSEY. Thanks; but question of dog's descent is rather a biological than a philological one.

Letters Received.

H. Muirhead, W. H. Morgan, Aspiring Artist, J. Hartington, L. M. N., K. Mongar (?), M. Emerson, P. T. L., Audax, Peter Parley, Sucking Herschel (must not suck brains), E. F., Materfamilias, Peterkin, Excelsior, J. North, M. Weatherwit, St. Pancras, Q. E. D., F. Y., Formosa, Empty Noddle (try to fill), James Ingersoll, Amory, N. C., Philip St. John, Northern Lad, Calais-Douvres, Amplitude, N. Tressingham, Cœlebs, Shingly Beach, M. Peterson, J. Short, &c., &c.

Our Mathematical Column.

THE LAWS OF PROBABILITY.

BY THE EDITOR.

THE mathematical discussion of the laws of chance is regarded by many with suspicion, because they observe that while the matters discussed are admitted by the very inquirer to be doubtful, the conclusions arrived at are presented as matters of mathematical certainty. But in reality this arises from a misapprehension of the nature of the meaning made by mathematicians into questions relating to chance. A mathematician assigns a definite value, as if it were certain, to the chance of winning a prize in a lottery (where one prize only, let us say, can be won) under given conditions; but he does not assert that the event will confirm his opinions; on the contrary, he knows that whatever happens, the sum he names will not be gained. He says *that*, certainly, is the value of the chance, but he knows that either the prize will be won, in which case more than the sum he named will be won, or lost, in which case the drawer of the blank will win nothing. He cannot even say that in any given number of trials the average amount won will be what he has named; he can only say that the greater the number of trials, the nearer will the average amount won be to the amount he has named. On this point only he is certain, and not only can his view be shown by logical reasoning to be sound, but multiplied experience confirms it. The reasoning may not admit of being grasped very easily, or, at any rate, very quickly. In particular cases the mathematical determination of the value of a chance may be so difficult that only advanced mathematicians can master the demonstration. But even in such cases, experiments can often be made quite easily, by which, with a little patience, the mathematical solution may be shown to be correct. Take, for instance, one of the "chance methods" of squaring the circle. A straight rod of given length, and of given square section, is tossed at random on to a grating of equidistant bars, and after gyrating in the air a number of times, falls either athwart the bars or between them, according, one would say, to pure chance (or bare chance, or mere chance, as you may choose to call it). A mathematician says that the chance of the rod falling through—the spaces between the bars being, of course, wider than the rod—depends (in what seems an occult fashion) on the relation between the circumference and the diameter of a circle. The proof is not simple, and perhaps you fail to understand it. But set some one to toss the rod (from a place where he cannot see the cross-bars, and without any knowledge of their position) a few thousands, or tens of thousands of times, and note how often it falls through, and how often it fails to fall through; you then find that the ratio of the two numbers approaches very nearly, the more nearly the oftener the rod is thrown, to the ratio assigned by the mathematician. The experiment may be tried any number of times, and always the result is the same.

The science of probabilities is shown by such instances as these to be a science which can predict, even in matters of pure chance. It is not a science which authoritatively lays down certain *dicta*, but one which itself indicates ways in which it may be put to the test.

But then, say objectors, probability is dealt with by mathematicians in so artificial a manner, that these methods cannot possibly have any application to real events. At the very outset there are conventional rules, which, so far as we can judge, might just as well have been entirely different.

In reality, however, the rules by which mathematicians deal with probabilites are only conventional in the same sense that it is conventional to measure lines by inches or by feet, to measure angles by circular arcs, or to measure surfaces, solids, time intervals (what you please, in fine, that mathematics can deal with) as mathematicians do measure these quantities.

Let us see what these conventions are:—

In the first place, it is agreed that absolute certainty shall be represented by unity, absolute impossibility by 0, and therefore (necessarily) different degrees of probability by different proper fractions. We can thus never have a chance greater than 1, for nothing can be surer than the sure; nor can we have a chance less than 0, that is negative, for nothing can be more impossible than the impossible. These are pure conventions. We might have called certainty 10 or 100, or $59\frac{1}{3}$, or anything we pleased; we might equally have represented impossibility by any quantity, positive or negative, or either certainty or improbability by a letter. It is found convenient, however, to adopt the particular convention mentioned, and so long as, having once adopted it, we uniformly follow it, we shall no more be likely to go astray than when we represent the number "three" by the figure three throughout an arithmetical sum.

These conventions are in reality all which the student of probabilities has to make; all others which seem to arise, as his study of the subject proceeds, are in reality only necessary consequences of these.

Let us now consider some simple illustrations of the results of these conventions.

Suppose a bag contains ten balls, all white. Then the chance that a white ball will be drawn is unity, and the chance that a black ball will be drawn is 0.

If all the ten balls in a bag are of the same size, and the drawer can as readily reach one as another, or if the bag is well shaken, so that each ball has an equal chance of coming to the top, the chance of any given ball being drawn at a given trial must be exactly equal to the chance of any other. All the balls are equally likely to be drawn. So that if C be the chance for each, $10C$ is the chance for the ten. But one of them must be drawn. Hence $10C$ must be equal to certainty, i.e. to 1; and $C = \frac{1}{10}$.

This is obvious enough, but let us submit it to a test.

Suppose one ball is white and the remaining nine balls all black, and suppose that if the white ball is drawn, a certain prize, say £10, will be won. Now, if there are ten persons to draw, and each takes one ball, it is certain that one of them will draw the white ball. But there is nothing to prevent one and the same person buying up the chances of all the ten, or the certainty of drawing the white ball, and so winning the prize of £10. He should clearly pay the same sum to each, for each has the same chance of winning, and the total sum he should expend should be clearly £10, for he makes certain of getting that sum by buying up all their chances. Since, then, he has to pay £10 in equal sums to ten persons, he must give £1 to each. This, therefore, is the value of each person's chance of drawing the white ball, and this sum is one-tenth of the sum at stake. This is the same as saying that the chance of each is $\frac{1}{10}$.

We have here spoken of a bag of 10 balls; but it is clear the reasoning applies to any case in which there are a number of events all equally likely, and one of which must happen. If there are 13 balls in a bag, the chance of drawing any particular ball is $\frac{1}{13}$; if there are n, the chance of drawing any given ball is $\frac{1}{n}$. If a die has all its six faces numbered differently, the chance that when the die is thrown any given number will be uppermost, is $\frac{1}{6}$, if the die is not loaded,—that is, if it as likely that one face will be uppermost as another. Again, if a coin is tossed with a good high spin, and the coin is uniform, so as to spin with perfect freedom, the chance of head or tail is $\frac{1}{2}$. So the chance of drawing any particular card from a picquet pack of cards is $\frac{1}{32}$; the chance of drawing any particular card from a whist pack is $\frac{1}{52}$; and so with other such cases.

Next, suppose that a bag contains three white and seven black balls, or ten in all, and let us inquire what is the chance of drawing a white ball.

The chance of drawing any particular white ball is $\frac{1}{10}$. It might seem, then, sufficient to reason that, since there are three white balls, and the chance of drawing each is $\frac{1}{10}$, the chance of drawing any one of the three is $\frac{3}{10}$. The reasoning is, indeed, just, and the result is correct, but the student cannot be too careful in avoiding all hasty conclusions in these questions of chance. We shall see presently that a line of reasoning which seems at first sight quite as just as the above will lead to an obviously incorrect result. It will be well, then, were it merely to initiate a system of close inquiry into these matters, to discuss the above result somewhat attentively. We shall save time in the long run by getting these seemingly axiomatic matters thoroughly reasoned out.

Applying our former method of measuring chances by considering the value of the right to draw tickets in a lottery, we readily determine the chance we are seeking. We have only to suppose that there are three prizes equal in value—say each £1. Then the chance of each of the ten drawers must needs be equal. Now, anyone who bought up all these ten equal chances should clearly pay £3, since this is what he would obtain when the ten tickets were drawn. Each ticket would therefore cost six shillings—that is, the value of the chance of each drawer is $\frac{3}{10}$ of the prize gained by a successful drawing. We infer the justice of the conclusion that the chance of drawing one white ball from a bag containing seven black and three white balls is $\frac{3}{10}$.

Since the above reasoning is applicable, whatever the total number of balls, and whatever the number of white balls, we have this general result, that the chance of drawing a white ball from a bag containing n balls, of which w are white, is $\frac{w}{n}$.

Further, since the reasoning is as applicable to the black balls as to the white, it is obvious that the chance of drawing a black ball out of a bag containing seven black and three white balls is $\frac{7}{10}$. This is the chance of failing to draw a white ball. And, generally, the chance of failing to draw a white ball from a bag containing n balls, of which w are white, is $\frac{n-w}{n}$.

Nor is the reasoning affected if the balls which are not white are of more than one colour; while the same reasoning applies to the balls of divers colours. So that we clearly get this general rule, including all that we have thus far attained to. If there are n balls (all equal in size) in a bag, of which w are white, b black, r red, g green, and so on, till all the colours and balls in the bag are reckoned, then—

The chance of drawing a white ball is	$\frac{w}{n}$
,, failing to draw a white ball is	$\frac{n-w}{n}$
,, drawing a black ball is	$\frac{b}{n}$
,, failing to draw a black ball is	$\frac{n-b}{n}$
,, drawing a red ball is	$\frac{r}{n}$
,, failing to draw a red ball is ..	$\frac{n-r}{n}$

and so on through all the colours, and we may also combine any of the colours together in such statements as the following:—

The chance of drawing either a white or a black ball is	$\frac{b+w}{n}$
The chance of failing to draw either a white or a black ball	$\frac{n-(b+w)}{n}$

and so on.

This general law is applicable to any case where the chance is required that one of a certain set of events will happen out of a larger number of events whose chances are equal. Thus, the chance that either ace or deuce will be thrown with a single die is $\frac{2}{6}$. The chance that a court card will be drawn out of a picquet pack of cards is $\frac{12}{32}$, the corresponding chance in the case of a whist pack being $\frac{12}{52}$.

[Solutions of problems, by T. R. and others, in our next.—Ed.]

A Substitute for Water in Foot-Warmers.—People who travel much in winter, either in railway carriages or in any of the other modes of conveyance, are continually annoyed and inconvenienced by the fact that the hot water in their tins gets cold very soon; in fact, if the tins are to be of any comfort to the traveller, they must be changed every two hours. Who, amongst all those who have felt starved and miserable through the water in the foot-warmer having become cold, will not hail with delight the fact that the science of chemistry promises speedily to bring a fresh boon to the traveller in all parts of the world, in the form of a foot-warmer which will keep hot for a period of ten hours, at the same time giving out four times as much useful heat as water? The name of the chemist who is bringing about this great and useful change is M. Angelin, who purposes using crystallised sodium acetate instead of water. When once the tins are filled, the stoppers well soldered, and the warmer perfectly air-tight, all trouble ceases except warming them up when required for use. The tins can be used over and over again, the salt being perfectly stable. Experiments are now being tried on the London and North-Western Railway, also on various foreign railways, with a view to its adoption.—F.C.S.

Our Chess Column.

Games between Mephisto, and a strong Amateur.

Allgaier Gambit.

White. Mephisto.	Black. Mr. M.	White. Mephisto.	Black. Mr. M.
1. P to K4	P to K4	14. P takes P	Q takes P
2. P to KB4	P takes P	15. B to Q3 (ch)	K to Kt2
3. Kt to KB3	P to KKt4	16. Castles KR	R takes B(e)
4. P to KR4	P to Kt5	17. P takes R	Q to B4 (ch)
5. Kt to Kt5	P to KR3	18. R to B2(f)	Kt takes P(g)
6. Kt takes P	K takes Kt	19. K to Rsq	B to K3
7. P to Q4	P to Q4	20. R to B6	Kt takes R(h)
8. B takes P	Kt to KB3	21. P tks Kt (ch)	K takes P(i)
9. Kt to QB3	B to Kt5(a)	22. Q takes P(ch)	K to K2
10. B to K5	B tks Kt ch(b)	23. R to Ksq	Q to Q4
11. P takes B	R to Ksq	24. P to B4	Q to Q3
12. Q to Q2 (c)	K to Kt3(d)	25. B to B5	resigns
13. P to R5 (ch)	K to R2		

(a) The best defence is to employ the Bishop on K2 in combination with R to KBsq and K to Kt2.

(b) The tempting move Kt takes KP would not be good play.

(c) Or B to K2.

(d) Black might have defended with 12 Kt to QB3, 13 Q takes R P, 13 Kt takes B, 14 P takes Kt, 14 R takes P.

(e) A powerful resource, which threatens to break up White's game.

(f) A desperate move, but if K to R2 or Rsq, then by Q takes KP Black would completely domineer over White's game. Of course, White cannot afford to exchange Queens by interposing her on B2. R to B2, besides proving an effective defence, also keeps the attack in hand, for it would not be quite so good now for Black to play, Q takes KP, for White would then reply with R to Ksq, and Black could not then take the RP with a check, which would have been the case had the King gone to Rsq or R2. The winning of the time of one move won the game for White.

(g) Black dare not play P to Kt6, for then White would reply with P takes Kt ch, and dissolution would speedily follow.

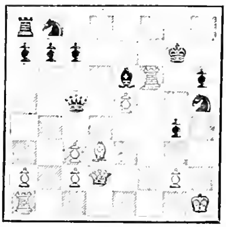
(h) White not only threatens R takes B, but also Q takes R P, Black has no alternative but to take the proffered Rook. The sacrifice is perfectly sound.

We give a diagram of the position.

Position after White's 20th move:—

Amateur.

Black.



White.

Mephisto.

(i) K to B2 instead of K takes P looks better, but it would also lose, *e.g.*:—

21.	K to B2		
22.	Q takes P or / Kt to Q2	K to Ksq	
23.	B to Kt6 (ch) / K takes P	R to Qsq or / B to B2	Kt to Q2
24.	B to B5 (ch) / K takes B (a)	B to Kt5 (ch) / P to B3	B to Kt6 (ch) / K to Qsq
25.	R to Bsq (ch) / K to K4	Q to R8 (ch) / Q to Bsq	Q to R8 (ch) / and mates in two moves
26.	R to Ksq (ch) / and wins	R to K8 (ch) / and wins	

(a) 24. K to K2 — 25. Q takes B ch / K to Bsq — 26. Q takes Kt / and wins

GAMES BY CORRESPONDENCE.—(*Continued from p. 396.*)

GAME I.

Chief Editor.	Chess Editor.
	3. P to Q4
4. P to K5	4. P to QB4
5. Kt to KB3	5. P takes P
6. Q takes P	6. Kt to QB3
7. Q to B2	7. B to QKt5 (ch)
8. P to QB3	8. B to QR4
9. B to Q3	9. KKt to K2
10. P to QKt4	

GAME II.

	3. Q to E3
4. P to Q4	4. P to Q3
5. Kt to K B3	5. Kt to QB3
6. P to Kt3	6. B to Q2
7. P to K5	7. Q to K2
8. P to QR4	8. Castles
9. B to QR3	

SOLUTION OF PROBLEM NO. 20, p. 350

By J. A. Miles.

1. B to R4 (ch)	R takes B
2. Q to QR7	K takes R (a)
3. Kt to K6 dis. ch. and mates next move.	

(a) If R takes QKt to K8, mate.

ANSWERS TO CORRESPONDENTS.

*** *Please address Chess-Editor.*

J. A. Miles.—Problems received with thanks. Your kindly-proffered assistance welcome. We can only benefit by your experience.

E. C. H.—Yes.

J. S. Flower, Ryde.—Received with thanks, but too obvious for insertion.

D.—Self mate is to compel your adversary to mate you.

H. A. L. S.—Problems 21 and 22 are intended to be easy. We should displease far more readers by giving difficult problems only than by giving occasionally easy ones. Curious to say, the easy problem of which you speak so disparagingly has proved too much for you; for if 1. R to K7 (ch), then Q takes R, and there is no mate.

H. Percival, R. S. Standen, and others.—Answered last week.

F. H. Jones.—Solution of No. 20 incorrect; if 1. R takes P, 1. R to Kt7, and there is no mate in three.

Correct solution of Problems No. 21 and 22 received from G. B. T., J. Licence, A. C. Skinner, A. J. Martin, H. Percival, Henry Bowmann, R. S. Standen, Brenton, F. H. Jones (of No. 22 only), S. H. G., J. R. W.

Problem 21.—In this problem there is a Bishop on White's King's Rook square, with a Pawn on Kt2. The object of the Bishop is to command the square on QB6; if, therefore, we replace the Bishop on KRsq. by a Queen, this object is likewise achieved, and the anomaly (if it may be so called) is obviated.

Problem 21, p. 369. From Squire.

White.	Black.
1. King's Knight's Pawn to his 4th	1. Queen takes Castle
2. Knight to King's 6th (from KB4) mate.	

Variation.

1. As before	1. If Queen takes Bishop
2. Castle to King 7th. Mate, &c.	

Problem 22, p. 369.

White.	Black.
1. Queen to Q8	1. K to KB2
2. Kt to K7, mate	
1. As before	2. Queen to Q2
2. Queen to KB6, mate, &c.	

Notice.—For "Nos. 2 and 3 are *not* out of print," in Answers to Correspondents, No. 17, read "Nos. 2 and 3 are *now* out of print." Part I. is now entirely out of print. Those who wish to complete the series would do well to get the Parts which are still in print, and to add their names to the list of applicants for Part I., so that, should any copies be returned, they may be distributed in due order. No. 5, also, is now out of print.

Our Whist Column.

By "Five of Clubs."

J. Hartington. The law against revoking, like most of those which involve penalties, is intended to prevent players, either through carelessness *or otherwise*, from doing anything by which they might obtain a wrongful advantage. Wilfully revoking would be cheating, were there no penalty; it is none the less cheating when it is done with the hope of escaping the penalty, or to hide a former honest revoke. The penalty for wilful revokes is exclusion from the society of honest card-players.

Five of Clubs.

Problem 2. An easy Double-Dummy Problem.

Colonel Drayson gives the following amusing example (which occurred to himself at double dummy) of the difference between practical and theoretical Whist. As he says, if any player had played at Whist as it is necessary to play in the following case, he would probably have been accused of trying to lose the game:—

The Hands.

A.
Hearts—10, 6, 4.
Spades—A.
Clubs—A, K, Q.
Diamonds—10, 7, 6, 5, 4, 3.

Y.
Hearts—A, K, Q, Kn.
Spades—K, 10, 7, 4.
Clubs—10, 7, 4.
Diamonds—K, 2.

	B	
Y		Dealer Z *Trump Card, Heart Nine.*
	A	

B.
Hearts—5, 3, 2.
Spades—9, 8, 5, 3, 2.
Clubs—None.
Diamonds—A, Q, Kn, 9, 8.

Z.
Hearts—9, 8, 7.
Spades—Q, Kn, 6.
Clubs—Kn, 9, 8, 6, 5, 3, 2.
Diamonds—None.

Score:—*A*, *B* 4; *Y*, *Z*, love.
Y Z to save (and win) the game.

Next week an interesting game, kindly sent us by Mr. F. H. Lewis, will appear, illustrating the use of the penultimate as a means of conveying information to partner.

Sir,—As "Five of Clubs" says that playing Knave second hand from Knave two, a small card having been led, would not be signalling for trumps, and that Clay says so, whereas I argued that, for all the third player could tell, the second player might be asking, and, as "Five of Clubs" has, I am convinced, somewhat misunderstood Clay's meaning, allow me, on a point of such frequent occurrence, and in which so many go astray, to justify my comment and elucidate the position. On referring to Clay, your readers will note that he is impressing the fact that to ask for trumps a player must throw away an unnecessarily high card, and he illustrates this by an example of his partner playing a ten second hand on a small card led, and afterwards playing a small card, whereon Clay says, "He thinks he has asked for trumps, but he has done no such thing. His ten is not, so far as I can tell, an unnecessarily high card. It is an effort to take the trick. It may be played in the ordinary way from Knave, ten, and a small card of the suit." Clay's meaning clearly is, that inasmuch as the ten may have been played from Knave, ten, and a small one, and would in that case not have been an unnecessarily high card, the play of the ten, and then a small one, is not *per se* asking for trumps. And he clearly implies that if he knew or could infer that his partner did not hold the Knave, he would have regarded it as asking for trumps; for remember that he expressly lays it down that, with Knave or ten and one small card second hand, the small card is to be played, unless to cover; so that, in his opinion, holding ten, or Knave, and a small one second hand, and playing ten or Knave on a small one, would be an unnecessarily high card, and that is the test. Cavendish's language on this point is not liable to the same misinterpretation as Clay's. He says, "It is important to distinguish between covering second hand and discarding an unnecessarily high card. For example, with Knave, ten, and a small one, it is usual to play the ten second hand on a small card. When the small card is played the second round, it is not a signal for trumps, unless your partner can infer that you do not hold the Knave." So equally and by parity of reasoning, a player holding Knave and a small one can by playing the Knave second hand, when a small one is led, ask for trumps, but it will not be a signal to his partner unless his partner himself holds the Queen, or can infer that it is in the hands of either adversary. And this, according to my experience, is the view adopted in play by Cavendish and other fine players. I therefore submit that in Problem 1 *B* (third player) could not be sure that *Y*, who played the Knave second hand and must hold the two, was not asking for trumps.

On the other matters at issue between myself and "Five of Clubs," I will only say that some expressions in his letter to you, Mr. Editor, give inaccurate impressions of the contents of my letter, for which you could not find space.

Mogul.

If our whist readers consider a few sentences preceding those quoted by "Mogul" from Clay, and a few which follow, they will see, we think, that Clay, at any rate (rightly or wrongly), thought the play of ten followed by small one (with these two cards alone in hand) an unsatisfactory way of signalling. The passage runs:—"My partner is second to play, *and holds, say, the ten and a small card of the suit*, which the adversary opens with a small card. My partner being second player, plays his ten, and the trick is taken with the King; the lead is returned, and the original leader takes with the Ace, my partner throwing his small card." He thinks, &c., as quoted by "Mogul," "He could only have given, in this way, a *legitimate* invitation for a trump, if the card originally held had been higher than the ten, which, in this case, would have been an unnecessarily high card."

It seems to me clear that Clay, with his (perhaps exaggerated) abhorrence of small cards, here teaches that if you hold ten and a small one second hand, you should not, even though you desire to signal, use these cards for the purpose, lest your partner (not holding Knave or having any means of placing it elsewhere), should infer that it remains in your hand, unguarded. Of course, every whist player knows that with Queen and one other, or Knave and one other, or ten and one other, the small card should be played. Every whist player also is familiar with Cavendish's explanation (given in almost identical terms by Pole also) that with Queen, Knave, and small one, or Knave, ten, and a small one, the highest of the sequence should be played first if you want to signal. But that I take it is a different point, not, as Mogul opines, the same. Cavendish is speaking of a case where second hand holds three, Clay of a case where he holds only two. However, Problem 1 was not mine, but taken, as stated, from the *Westminster Papers*.—Five of Clubs.

And this leads me to note that many querists seem unable to understand how *Z* could know from *Y*'s play in the ninth and tenth rounds of the game in No. 13, p. 284, that *Y* held the Knave. I was *Z*, *Y*'s partner, and I knew it; the "proof of the pudding," &c. Let me explain how I knew it—at once, without having to think over the matter. *Y* usually played strictly in accordance with the familiar rules, always "following" with lowest of a sequence, unless there was special occasion to depart from the rule. In this case he manifestly had not played the lowest, for after the ten fell the nine. I knew he would never have played ten before nine, from the sequence ten, nine only. I was absolutely certain, therefore, that he held the Knave. It was the only way in which he could have shown it. He was bound to play one of the sequence; the small card would have lost a trick and the game. If he played nine, then ten, I should have known nothing about the Knave; if he had played nine, then Knave, I should have known nothing about the ten, or rather I should have supposed ten with the adversary. If he had played ten then Knave, or Knave then ten, I should have supposed the nine with the adversary. By playing ten, then nine, he showed me the position of the third card of the sequence. My own play of the King first was decidedly wrong, though, of course, strictly in accordance with rule. I felt this the moment I had played it; who has not made such mistakes? But it seemed to me at the time that *Y* hit on a very ingenious course to show me he held the three sequence, by departing from the customary rule and playing the middle card. Of course, if he had not been a steady player, I might have supposed the play of ten followed by nine a mere piece of carelessness; but I felt certain it was *not*.

Five of Clubs.

NOTICES.

The Back Numbers of Knowledge, with the exception of No. 2 (Nov. 11, 1881), No. 3 (Nov. 18, 1881), and No. 5 (obtainable only by taking Part II.), are in print, and can be obtained from all booksellers and newsagents, or direct from the Publishers. Should any difficulty arise in obtaining the paper, an application to the Publishers is respectfully requested.

The following Monthly Parts of Knowledge are now to be had (Part I. being out of print):—

Part II.—(Dec., 1881.) Containing five numbers. Price 1s. Post-free, 1s. 2d.
Part III.—(Jan., 1882.) Containing four numbers. Price 10d. Post-free, 1s.
Part IV.—(Feb., 1882.) Containing four numbers. Price 10d. Post-free, 1s.

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KNOWLEDGE
AN ILLUSTRATED MAGAZINE OF SCIENCE
PLAINLY WORDED-EXACTLY DESCRIBED

LONDON: FRIDAY, MARCH 17, 1882.

Contents of No. 20.

** Our Exchange and Sixpenny Sale Columns appear on Page IV.—in our advertising columns this week.

ON THE CONSERVATION OF SOLAR ENERGY.

By Dr. W. B. Carpenter.

The meeting of the Royal Society on March 2nd was rendered unusually interesting, first, by the admission of H.R.H. the Prince of Wales, as a Fellow of the Society; second, by a communication given by Prof. Huxley on the fungous origin of the "Salmon Disease," which is destroying large numbers of fish in the rivers of the South of Scotland and the North of England, from the Tay to the Conway; and last, but by no means least, by the exposition given by Dr. Siemens of an "idea" regarding the mode of maintenance of the Solar energy, which he has been for some time maturing, and has at last determined to submit to the criticism of the scientific world. Of this most ingenious and suggestive speculation, the following sketch will, I hope, prove as interesting to the readers of Knowledge, as Dr. Siemens's own admirable and more detailed statement of it was to the members of the large scientific gathering to which it was addressed.

In the first place, he reminded us of the enormous amount of heat which is constantly radiating from the Sun into space; this, according to the best measurements that have been made, being such as would be maintained for only thirty-six hours by the complete combustion (as in the most perfectly-constructed furnace) of a mass of the best coal equal to the Earth in bulk. Now, if the sun were surrounded by a solid sphere of a radius equal to the mean distance of the earth, the whole of this heat would be intercepted by it; but since the diameter of the earth, as seen from the sun, is only seventeen seconds, so that its surface is only 1-2,250,000,000th part of the whole area of such a sphere, only that proportion of the entire heat radiated from the sun will fall upon the earth. Supposing the aggregate of all the Planetary bodies to intercept ten times as much as the earth, the total amount of solar heat thus utilised will be only one part in 225,000,000 of the total radiated from the sun; the other 224,999,999 parts to all appearance going to waste—in other words, doing no work.

Now the mode in which this enormous supply is kept up has been in all ages a question of great interest; but only in modern times could any scientific solution of it be even attempted. Of course, Chemical action would be the first source that would occur to almost every one—radiation of heat from a fire being the nearest thing within our experience to the heating effect of the solar beams. But, putting aside other difficulties arising out of the revelations of the spectroscope, the ordinary chemical hypothesis is met by the objection, that the accumulation of the products of combustion on the surface of the sun would in time form a barrier against further action. And, supposing this barrier disposed of, it is obvious that the maintenance of this combustion must be attended with a continual *wasting-away* of the sun, at a rate which would make itself perceptible in the disturbance of planetary equilibrium, when the loss is estimated for long periods of time.

An opposite idea was suggested some years ago by Sir William Thomson: that of a continual rain of Meteorites upon the sun—the velocity they would acquire from its attraction causing them to impinge upon its surface with such force, as to generate a large amount of heat when their motion is checked. But here we are met by two difficulties: first, that of conceiving of any supply of meteorites that would be competent thus to keep up the amount of heat which we know to be always radiating from the sun; and secondly, the progressive *increase* in the bulk of the sun that would be produced by any adequate supply, disturbing the planetary equilibrium in the contrary sense to the preceding.

It has been supposed by Helmholtz, and accepted by many physicists on his authority, that the radiant energy of the sun is the result of a progressive *shrinkage* of his bulk and condensation of his substance. But the giving-out from his surface of the heat thus generated in his interior, could only be accomplished through some medium of much greater conductivity than is possessed by any material known to us; and on this process, again, a limit is obviously imposed, since a time would come when (as seems now the case with the Moon, and nearly so with the Earth, Venus, and Mars) the limit of consolidation would be reached.

Dr. Siemens, as every one knows, is the inventor of the regenerative furnace—now coming into general use; in which a large proportion of the heat that ordinarily goes up the furnace-chimney, and runs to waste, is recovered from the products of combustion, carried back into the furnace, and made to do its proper work—thus obtaining an enormous advantage in economy of fuel. Mentally projecting this terrestrial experience into the realms of space, he was led to the conviction "that the prodigious and seemingly wanton dissipation of solar heat is unnecessary to satisfy accepted principles regarding the conservation of energy; but that it may be arrested and returned over and over again to the sun, in a manner somewhat analogous to the action of the heat-recuperator in the regenerative gas-furnace." The fundamental conditions of his hypothesis are three.

I. Everyone who has followed the recent progress of Celestial Physics, is aware of the increasing reasons which there are for regarding not only planetary, but stellar space as occupied by *matter* in a very attenuated condition; and Dr. Siemens starts with the assumption that this matter chiefly consists of hydrogen, oxygen, nitrogen, carbon, and their compounds (especially aqueous vapour and carbonic acid), besides solid material in the form of dust. The existence of oxygen, nitrogen, and carbon he considers to be indicated by the presence of those elements in our own atmosphere, to which (according to the molecular theory of gases) no such limit as was formerly assigned to it can now be admitted. We get a clue to the gaseous components

of what may be called the "atmosphere of space," from analysis of the gases locked up in freshly fallen meteorites, which sometimes "occlude" six times their own bulk. A recent analysis by Dr. Flight gave nearly 46 per cent. of the total as consisting of hydrogen, 32 per cent. of carbonic oxide, and 18 per cent. of nitrogen; and it seems clear that the hydrogen and carbonic oxide could not have been absorbed during the passage of the meteorite through our own atmosphere, but must have been brought in from the outside. Further proof that stellar space is filled with gaseous matter is furnished by spectrum analysis; and the recent investigations of Dr. Huggins and others into the composition of the last great Comet showed it to contain very much the same gases with those contained in meteorites.

II. It was long since shown by Sir William Grove that water can be decomposed—or, in modern chemical language, that oxygen and hydrogen can be "dissociated"—by heat alone; and we know that the dissociation of the oxygen and carbon in carbonic acid, is effected by light, acting through certain vegetable substances. Now, according to the law of dissociation developed by Bunsen and Saint Claire Deville, the point of dissociation of different compounds depends upon temperature on the one hand and pressure on the other; so that it is quite conceivable that when aqueous vapour is reduced to extreme tenuity, its dissociation may be effected by solar radiation at a comparatively low temperature. Some years ago Dr. Siemens tried some experiments on this point, the results of which were (so far as they went) confirmatory of this view. And his recent well-known experiments on the growth of plants under the electric light have satisfied him that, provided the source of the light give it off in sufficient *intensity*, the *quantity* required is very small. And he is thus led to suggest that all the radiant energy which is seemingly running to waste, is really doing work in dissociating the aqueous vapour and carbonic acid of the "space atmosphere," the carbon being thus made ready to unite with the nascent hydrogen into combustible hydrocarbons.

III. The third basis of Dr. Siemens's doctrine is the effect that will be produced by the rotation of the Sun around its axis, on the distribution of gases and vapours in its atmosphere. The tangential velocity of the sun at its equator being nearly four times that of our earth, an extension of the solar atmosphere must take place in the equatorial plane, to which (reviving an old hypothesis, and explaining away the objection raised to it by Laplace) Dr. Siemens attributes the "zodiacal light." Pressures being balanced all round, Dr. Siemens shows that the sun would be continually drawing hydrogen, hydrocarbons, and oxygen from the "space atmosphere" towards its polar surfaces, and be continually projecting outwards the products of their reunion, from the equatorial extension of its own atmosphere. During their gradual approach, they will pass from their condition of extreme attenuation and extreme cold to that of compression, accompanied with rise of temperature; until, on approaching the photosphere, they burst into flame, giving rise to a great development of heat, and themselves acquiring a temperature proportionate to the pressure they are sustaining. The result of their combustion will be aqueous vapour and carbonic oxide or carbonic acid, according to the sufficiency or insufficiency of the oxygen present to complete the combustion; and these products of combustion, yielding to the influence of centrifugal force, will flow towards the solar equator, and be thence projected into space.

In this manner a continual interchange of matter will be taking place between the sun and its "environment;" and as the Sun is constantly and rapidly moving through space, it will be continually traversing new portions of the "space atmosphere," which, it is conceivable, may be so differently charged with the supplies of material, as to be more or less potent in maintaining the solar energy.

Such is a general outline of Dr. Siemens's most ingenious speculation, which, whatever may be its ultimate issue, must be accounted one of the highest and most brilliant flights that the "scientific imagination" has ever made. Such as desire a more detailed exposition of it—especially as to the changes which Dr. Siemens supposes to be always taking place on the surface of the sun itself—will find it in his paper, which will speedily appear in the "Proceedings of the Royal Society." Its publication will doubtless give rise to much discussion; and, whatever may be the ultimate fate of the doctrine as a physical theory, there can be no doubt that in the new direction which it will give to investigation, its promulgation will contribute in no small measure to the advance of science.

NOTES ON ROWING.

BY AN OLD CLUB CAPTAIN.

LET us, in the first place, consider the conditions under which an ordinary lapstreaked inrigged (or half outrigged) boat should be rowed in order to get the best racing speed for a boat of that sort. I, of course, assume the rower to have mastered all the initial difficulties of his art, so as to be able to give his attention to the question of style. Well, in the first place we find that for racing purposes the great object is to adopt a style by which we may *maintain*, as far as possible, the velocity which can be readily enough communicated by a great short-lasting effort, and to do this with as little overwork as possible. Racing necessarily involves overwork, for no one who meant to row for two or three hours, or even for a single hour, would adopt a racing stroke, even for five minutes of the time. But the overwork in a race has to last over a good many minutes, and must be so distributed as to be most effective. The rower has, therefore, in racing to avoid, above all things, whatever would involve waste of power; and he very quickly finds that the most mischievous waste of power results if he suffer the extra speed communicated by his efforts to be lost more than of necessity it *must* be lost between the strokes. In other words, a given average of velocity is obtained with greater or less expenditure of force, according as the necessarily varying velocity of the boat ranges more or less above and below that average. Or we may put the matter this way (it is not without a purpose that we put it in both ways): The more uniform the velocity, the less the total expenditure of power to attain a given average rate of speed.

When we say that a rower soon finds this out, we mean that if he is attentive and apt he does so. As a matter of fact, the most successful oarsmen (in races) are those who, whether they know it or not, have practically found this out, and the rules for a good rowing style are based—as will presently appear—on this important principle. But we know that, apart from training and example, numbers of stout oarsmen would never attain a good rowing style, or at any rate a good racing style. So that we might probably have said more truly that nine-tenths of our rowing men would not of themselves discover this law, which comes out very gradually in rowing practice, even to the acuter rower, and is theoretically only to be established by somewhat difficult reasoning, based on recondite principles, partly dynamical, partly physical, and partly physiological.

But now notice how in the rules for rowing in the old fashioned racing boats this principle shows itself.

Oarsmen were told in those days, and very soundly, to row in the following way: A good reach forward was to be taken, and the water caught squarely by the oar, not by a pulling action of the arms, but by the action of the body and legs; the arms were to remain perfectly straight, acting only as "stretchers" until the body was a little past the perpendicular; then the stroke was to be finished by the combined action of the arms, body, and legs the body slanting back, the hands drawn well in to the chest. The "recovery" followed, the body being thrown rapidly forward from the hips, the arms being at the same time extended, so that, the handle of the oar being thrust forward by both motions simultaneously, the blade passed with exceeding rapidity to the proper position for beginning the next stroke.

If we consider the dynamical effects of this action, we shall see how admirably suited they were to produce a motion as uniform as possible in the racing boats of those days. (Such directions are given in "Principles of Rowing by Oarsmen," somewhere about 1840, by Bob Coombes, who became champion in 1846, and by other excellent authorities of that time.) First, the strength was applied with gradually increasing effect from the beginning to the end of the stroke, so that there was no undue strain in increasing the motion of the boat from the velocity to which it had fallen during the "recovery" to its maximum just before the "feather." Then the work was carefully distributed between arms, legs, and body, the body and legs doing the work first, then the arms joining them to give that extra lift at the finish which was meant to counteract as much as possible the tendency to lag between the strokes,—so marked a characteristic of the old-fashioned racing boat. Lastly, that this tendency might have as little chance as possible to give the oarsmen extra or waste work, there was a very rapid recovery, so that the next stroke might begin under as favourable conditions as possible.

All these rules are admirable for the heavier class of boats, or for those which in old times were called racing boats. They served to obviate what, from our present point of view, may be called the great defects of those boats, their breadth of beam, and the (relative) clumsiness of their structure.

These rules were carefully enjoined at both the Universities; but they were more perfectly carried out at Cambridge than at Oxford. The sway back of the Cambridge crews and their rapid "recovery," were things to be marvelled at in some of the great races which preceded the introduction of light, outrigged racing boats. And those who adopted this system had their reward. Of six races rowed on the Thames in the old-fashioned craft, Cambridge won five. Not only did they win as a rule, but they often won in that hollow fashion which means that superior style has won the race, and not mere superiority of strength, or even of pluck (in both which, University crews are likely to be pretty evenly matched.) Cambridge won by a full minute in 1836, by a minute and three-quarters in 1839, by more than a minute in 1841, by half a minute in 1845, the last race rowed in the old-fashioned inrigged boats.

From 1846 to 1856 the University race was rowed in boats which had a sort of intermediate position between the heavy lap-streaked inrigged boat and the present light keelless outrigged craft. We should consider the boats used during those ten years quite unsuitable for racing purposes in our time. The old style of rowing suited them well enough—perhaps as well as the modern style: a style between the two would probably have suited them better than either. In the seven races between Oxford and Cambridge rowed in these earlier specimens of the outrigged racing boat, success was pretty equally divided between Oxford and Cambridge counting one race won by Oxford on a foul as a real win (which it certainly would have been, Oxford showing the better speed), each University won three. But the Oxford wins were better, especially in the latter years. Cambridge won by two lengths in 1846, by four in 1849, by half-a-length in 1856 (when Cambridge had an exceptionally powerful crew). Oxford won by eight lengths* in 1852, by about five in 1854, and would probably have won the race of 1849 by many lengths, apart from the foul. However, six years are not enough to judge by.

So soon, however, as we turn to the races rowed since the introduction of the modern racing-boat in its present form (except as to sliding-seats), we find the University which had been almost always successful in long races with the heavy craft, and which had seemed able, very fairly, to hold its own in the keeled outriggers, beaten, not only in the great majority of races, but also by much the greater distances. Let us consider the twenty-five races which have been rowed between Oxford and Cambridge since 1857:

Of these twenty-five races, fifteen have been won by Oxford, nine by Cambridge, and one was a dead heat. If we count the race of 1859 as one which Cambridge would have won had not the Cambridge boat been half full of water at starting, we may put fourteen races to Oxford and ten to Cambridge; but then, in fairness, the dead heat of 1877 should be counted as an Oxford win.† The mishap to Thorley's outrigger in the Oxford boat in 1858 may be regarded as fairly matched by the accident to Dick's stretcher in the Cambridge boat in 1875. This disproportion is too great to be probably due to mere chance. But when we examine the circumstances under which the various races were lost and won, we find the existence of a determining cause still more clearly indicated. Take for this purpose the following table, in which, to eliminate as much as possible the effect of mere chance, all the races since the introduction of outrigged craft are considered:—

Oxford won			Cambridge won		
In 1852 by	8	lengths	In 1846 by	2	lengths
1854	5		1849	5	
1857	11		1856	½	
1861	14		1858	7	
1862	8		1860	1	
1863	13		1870	1½	
1864	6		1871	1	
1865	4		1872	2	
1866	4		1873	3	
1867	½		1874	2½	
1868	4		1876	8	
1869	3		1870	2½	
1875	10		...	...	
1878	10		...	...	
1880	3½		...	...	
1881	3		...	...	
Total	107	lengths	Total	36	lengths.
Average per race ..	$7\frac{2}{15}$	lengths.	Average per race...	3	lengths

* The number of lengths corresponding to any given number of seconds by which the race was won, may be obtained by regarding 6½ *lengths* as equivalent to as many *seconds* as the race itself lasted *minutes*.

† The reader will understand that we are only regarding either race as affecting our opinion of Oxford and Cambridge style. The race of 1859 was unquestionably an Oxford win, though every one who knows the circumstances is aware that Cambridge never had a chance from the beginning; and, in like manner, the race heat of 1877 must be regarded as a dead heat, though it is certain Oxford would have won but for an accident.

Add to this the consideration that after Oxford had won nine successive races, from 1861 to 1869 inclusive, one of the best oarsmen Oxford has ever produced, Mr. George Morrison, gave much time and care to coaching the Cambridge crew into a better style than they had before followed, so that there is good reason for believing that in some of the races which followed (from 1870 to 1873, perhaps) the influence of Oxford training was at work in the Cambridge crews.

These points considered suggest a strong probability that there has been a radical difference for many years between the Cambridge and the Oxford style, the latter being the better. As it is well known that for many years since the old-fashioned racing-boats went out of use, the old-fashioned principles of rowing have been in vogue at Cambridge, we might fairly assume, apart from all dynamical evidence, that the old-fashioned stroke does not suit racing-boats of the present fashion.

Let us see what theory suggests as likely to be the best kind of stroke (for racing purposes) in these light boats, and then let us inquire what evidence we have to show that such a stroke really is rowed by the most successful crews.

(To be continued.)

FUTURE OF THE EARTH AND MOON.

By Dr. Ball, Astronomer-Royal for Ireland.

PART III.

EVERYONE knows that the moon always turns the same face towards the earth: this has been shown to be a consequence of the tides which were anciently raised in the moon. The tides in the moon were produced by the attraction of the earth, just as the tides on the earth are produced by the attraction of the moon. There is, however, an important difference: the earth is so much heavier than the moon, that the tides which the earth raised on the moon must have been much greater than the tides which the moon can raise on the earth. It matters not that the moon now contains no liquid ocean. All that is necessary is that the moon shall once have been soft enough to admit of being distorted by tidal influence. It must be remembered that it is not the mere presence of a high tide or a low tide that does the work. It is the rising and falling of the tide which produces the currents, and it is the tidal currents which do the work. The mighty tides which once acted on the moon have long since ceased, but they have forced the moon always to turn the same face to the earth, as this is the only attitude in which tides do no work on the moon. In the distant future the small mass of the moon will achieve the same result on the earth. At the final stage the earth and moon will move as if they were fixed rigidly together by movable bars, and were revolving around their common centre of gravity in 1,400 hours.

If the earth and the moon could be isolated from all external interference, there is no reason why this state of things should not continue indefinitely, but there is another disturbing cause with which we must reckon. We have seen that it was probably the sun which originally broke off the moon as a fragment from the earth. It seems, also, that the sun is destined to derange the harmonious compact in which the earth and the moon would have otherwise agreed. Once the 1,400 hour day and the 1,400 hour month have been reached, the earth will no longer be affected by tides produced by the moon. No doubt there will be a high tide on the earth and there will be a low tide, but as the earth will then always regard the moon with the same aspect, these tides will not rise or fall, they will not ebb and flow. There can then be no lunar tidal currents, and the tides will always remain at the same height at each point on our coasts. The sun, however, will still continue to produce tides on the earth. These tides will no doubt be small, as the solar tides are small at present: they will also rise and fall with extreme slowness. At present one high tide follows another in a little over six hours. At the final stage one solar high tide will follow another solar high tide only after an interval of about five weeks. These tides are small, and the currents they produce are very weak, but by incessant perseverance even these small tides cannot fail of producing an appreciable effect. The solar tidal currents act always in one direction, they always tend to retard the earth and to make the earth revolve more slowly. Here, then, we are conducted to a very remarkable condition of things in the distant future. The month will remain at 1,400 hours, while the day is lengthened still more. We thus have for the first time in the history of our earth-moon system the day actually longer than the month. A few years ago we knew of no analogy in the solar system to the state of things here foreshadowed. But the splendid discovery of the satellites of Mars has enabled us to give an illustration. The interior satellite of Mars moves round the planet in about seven hours, while the planet itself takes more than three times as long. At the time of its discovery this seemed a most anomalous circumstance, but now the difficulty has been to a great extent removed. It seems likely that Mars himself once rotated more rapidly than at present, and that by the intervention of the solar tides the present state of things has been brought about. It must not be overlooked that Mars and his satellites are much smaller than the earth and the moon. We might therefore expect to find that the process of evolution has proceeded much further in the case of Mars than in the case of the earth. Once the solar tides have acted on the earth sufficiently to abate its velocity below that of the moon, a new reaction on the moon will be manifested. This new influence is not a little curious. As the earth ceases to turn the same face to the moon, the lunar action will again commence to develope tides on the earth. The tidal currents produced in this way will tend to drag the earth on faster, instead of to retard it as before, but the moon can exert no action on the earth without a corresponding reaction. In this case the reaction will take the form of a force tending to draw the moon in again towards the earth. The matter is, however, too complicated for us to pursue it any further with advantage.

It is, indeed, remarkable that so striking a period in the earth-moon history can be traced out merely as limiting the influence of the tide. It would seem, from one aspect of the question, that at the present moment we are near the centre of the period, inasmuch as the ratio of the month to the day has but recently passed its maximum. From another point of view, however, we seem to be vastly nearer to the first stage, ancient as that is, than we are to the last. The day has lengthened from 3 hours to 24 hours, but the lengthening has to go on until the day lasts 1,400 hours, and the rate at which the change proceeds is now extremely slow, and is getting still slower. We cannot estimate the countless myriads of years that must elapse before the moon has attained its greatest distance and the day has become 1,400 hours in length.

We have in this paper merely touched on one department of the great problem of tidal evolution. The subject is yet in its infancy, but it seems to have before it a most noble future. By a series of most splendid discoveries, Lagrange had shown that the solar system contained the elements of stability and of permanence. Lagrange showed

that the perturbation of the planets could never transcend certain narrow limits. The planes of the orbits could never be much deranged, the eccentricities of the orbits could never be much altered. The major axes of those orbits could hardly be changed at all. But all the great calculations of Lagrange involved one supposition, they all took for granted that the earth and moon, the sun and the planets, were rigid bodies. This we know is certainly not the case with many of these bodies, it is probably not the case with any one of them. Viewed with regard to the present researches, we are tempted to ask whether the absence of rigidity may affect the truth of Lagrange's great theorems. On the answer to this question depends the stability of the solar system. There can be no doubt that owing principally to their small sizes, and to the distances at which they are separated, the planets behave nearly as rigid bodies would so far as their mutual attractions are concerned. Lagrange's theorems are therefore approximately true, and they will remain substantially correct for hundreds of thousands of years, but for all that the planets are not rigid. Jupiter and Saturn do not act upon each other merely as the two heavy particles which Lagrange's theory would require. Jupiter and Saturn do, no doubt, attract each other as Lagrange supposed, but they also raise tides in each other. These tides may be despised when we are considering moderate periods of time, but the tides are there nevertheless. It may be slowly, but it is certainly surely, that these tides are doing their work, and by their agency the solar system in course of time shall become utterly transformed. Geometers know that when a curve has to be drawn, the various parts of that curve may be represented with all desirable accuracy by suitably chosen arcs of circles. As we proceed from one part of the curve to another, the radii and the centres of those circles are changed so that each arc of the circle shall be substantially coincident with the corresponding part of the curve. As the arcs of the circles coincide with the curve, so do the laws of Lagrange adapt themselves to the course of change in the universe. At any moment Lagrange's laws are practically true, but in immense periods of time the system undergoes profound modification. The laws of gravitation as ordinarily considered account for many of the *details* in the present architecture of the heavens For the profounder and grander truths of the universe it seems not improbable that we must interrogate the tides.

ERRATA.—In Part 1. of "Photography for Amateurs" (No. 19, p. 400), paragraph 4, for "improved" read "impressed." In par 5, for "photoganic" read "photogenic"; and in the last par, read "J. Nicéphore, Niepce"; and for "figures" read "pictures." In No. 17, p. 363, Letter 289, for ". . . this gas; besides which, coal-gas is harmless," read ". . . this gas, beside which coal-gas is harmless."

RIGHTS OF ANIMALS.—Of the grotesque ideas which have imposed on us in the solemn phraseology of divines and moralists, none is more absurd than the doctrine that our moral obligations stop short where the object of them does not happen to know them; and assures us that, because the brutes cannot call us to account for our transgressions, nothing that we can do will constitute a transgression. To absolve us from paying for a pair of boots because our bootmaker's ledger had unluckily been burned, would be altogether a parallel lesson in humanity. It is plain enough, indeed, that the creature who is (as we assume) without a conscience or moral arbitrament, must always be exonerated from guilt, no matter what it may do of hurt or evil; and the judicial proceedings against, and executions of, oxen and pigs in the Middle Ages for manslaughter were unspeakably absurd. But not less absurd, on the other side, is it to exonerate men, who *have* consciences and free will, when they are guilty of cruelty to brutes, on the plea—not that *they*—but the brutes, are immoral and irresponsible.—FRANCES POWER COBBE, in the *Cornhill Magazine*.

ILLUSIONS OF MOTION AND STROBIC CIRCLES.

BY THOMAS FOSTER.

IT is a long time since I last wrote about illusions. But other subjects of more immediate interest have filled all the available space.

The last illusion which I considered was a very singular one, and I have been rather surprised to notice how few among the readers of KNOWLEDGE have recognised its interest and significance. It is one which admits of various modifications, by which the real nature of the illusion may be put to the test. It will be remembered that in describing it, I noted that the paper held in the reader's hand—KNOWLEDGE—might be used to illustrate it. Rolling KNOWLEDGE into a tube an inch or so in diameter, and looking through this with one eye, while the hand not holding the tube is held touching the tube, at about the distance for distinct vision, an appearance is presented as though one were looking through the hand. There seems to be a distinctly outlined aperture through the palm of the hand, if the hand is held with the palm open, or through the fist (and this has an even stranger appearance) if the hand be clenched. Now, if the experiment be modified by using tinted, or strongly patterned paper for the tube, and by looking at variously coloured surfaces, it will be found that the strength of the illusion varies notably. (This may be tested by directing the mind's attention specially to the hand, so as gradually to overcome the absurd delusion by which one seems to look through the palm or the fist.) I leave this as an exercise to the student of illusions, noting that the result to which my own experiments have led me is this, that in ordinary vision what the left eye sees is referred by the mind (unconsciously, of course) to the right eye, and what the right eye sees is referred to the left eye. By a series of tests, such as those I have indicated, this peculiarity may be recognised. I believe the eyes might be trained to overcome illusions of this class.

And now let us turn to some illusions of apparent motion, which, like those we have already considered, can be explained if carefully studied.

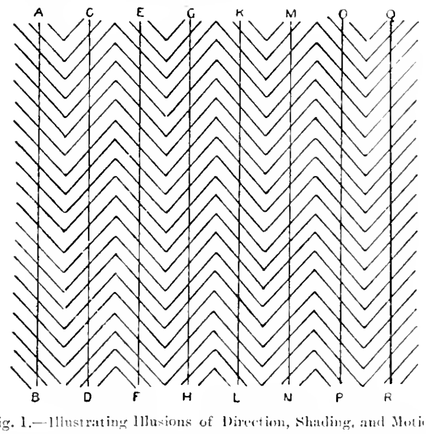


Fig. 1.—Illustrating Illusions of Direction, Shading, and Motion.

It may be remembered that in considering Fig. 1, in the first number of KNOWLEDGE, I drew attention to the fact that the lines AB, CD, EF, &c., which are in reality

Fig. 2.

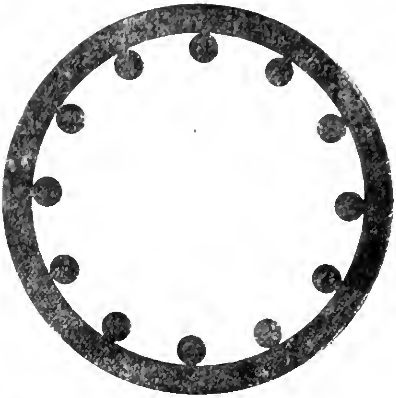


Fig. 1.

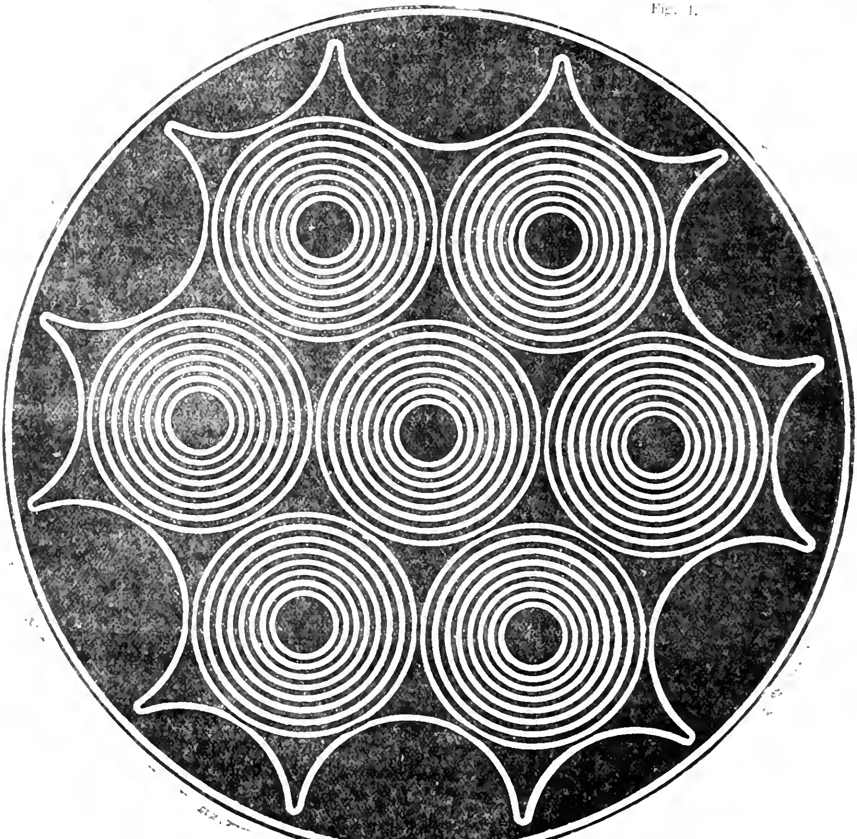


Fig. 3.

ILLUSTRATIONS OF STROBIC CIRCLES.

parallel, appear to converge towards AC, BD, CE, &c. Later (in No. 4), I called attention to the fact that this figure illustrates an illusion of motion. If the eye be run up and down the parallels, these appear to move. When the eye is at rest, they seem alternately convergent, especially if the figure is viewed a little askew, being neither held with the parallels vertical nor horizontal. But if the eye runs along two parallels which before had seemed to converge, they are found not to converge, and the effect produced is as though they had moved from each other at the end towards which they had seemed to converge. Another effect, also, is noticed. The level surface on which are the zigzag sets of lines, appears to be ridged, this being apparently due to the fact that the alternate sets of close parallels are viewed in different aspects. Thus, suppose the paper held so that B, Fig. 1, is lowest, then the parallels in sections CD, GH, MN, OR, appear farther apart than those in the other, or alternate sections. Now, if the figure is rotated in its own plane, so that first HL, then R come lowest, there is a change from the appearance just described to its reverse, the parallels in sections CD, GH, MN, OR, appearing now closer instead of farther apart than the others. Accompanying this change will be found certainly singular and rather complicated appearances of motion, the ridges sinking, then rising again, the sets of close parallels drawing apart or closer (and, if the motion is continued, closing up again, and drawing apart again respectively), and the other sets of parallels (vertical in the picture) seeming to bend and grow straight alternately.

Leaving the reader to study these changes and to note that they correspond with what we should anticipate, we proceed to more familiar instances of apparent motion, which find their explanation, I believe, in what we have learned from Fig. 1.*

Consider Figs. 2 and 3, first at rest, and then as each appears when a slow circling movement is given to it (as when a saucer is so moved as to set a small quantity of liquid in it circling round near the edge), and then as each appears when swayed through a short distance from side to side, or from and towards you, in its own plane.

When looking at either picture, held perfectly at rest, the eye, if kept still, is presently affected by appearances of quivering motion, apparently affecting the entire picture. If the eye moves backwards and forwards across the picture, or irregularly over it, the sets of concentric circles appear to undergo partial rotations,—in alternate directions in one case, irregular in the other. When the pictures are made to circle in their own plane, all the sets of concentric circles seem to turn round in the direction of the circling motion given to the picture. Lastly, when the pictures are shifted back and forward in their own plane, from side to side, each set of concentric circles is blurred at the sides, or rather in the side quadrants, distinct in the upper and lower quadrants, regarding the sets as divided into quadrants by diameters situated thus ×. When the motion is from and towards the observer, the upper and lower quadrants of the sets of circles are blurred, the side quadrants distinct.

To produce the rotatory motion, it is necessary to give a tolerably rapid circling motion to either picture. The experiment succeeds better if the picture is mounted on card, and the circling motion is communicated by a suitable crank, so as to be more uniform than any motion which can be given with the hand.

* At the meeting of the British Association in 1877 (at Plymouth), Professor Sylvanus Thompson exhibited these singular illusions.

When Fig. 4 is swayed like the others, by a circling motion in its own plane, the small black discs seem to be carried round in a direction contrary to that of the circling. It will be noticed that if the circle in which this figure is swayed is somewhat larger than is necessary to produce the deceptive appearance of motion, a regular pattern seems to be formed, by the persistence of the visual images of the small black discs in the picture.

NEWCOMB'S POPULAR ASTRONOMY.

We turn now to the less pleasant task of pointing out defects which might mislead those who rely on Professor Newcomb's well-deserved eminence as a mathematician and an astronomer.

We should have been disappointed if such subjects as the tides, the precession of the equinoxes, &c., had been simply left unexplained in a work of this character. Few subjects are less satisfactorily explained in most works on astronomy than the tides, for instance. We are supplied over and over again with the statement that the water immediately under the moon is drawn from the earth, while the earth is drawn from the water at the opposite side, a statement true enough (when properly limited) in itself, and a necessary preliminary to any explanation of the tides. But there the usual explanation comes to an end, the student being simply told that but for friction there would be high water in the region under the moon and at the antipodes of that region. Now, what would be thought of an explanation of the motion of a reeling top which only showed that an inclined top tends to tumble over? The reader would assuredly say, "What I want to know is why the top when spinning does *not* tumble over, but reels round." The common explanation of the tides is open to precisely such an objection. In the actual case of the rotating earth there would be low water instead of high in the regions under the moon, and opposite, were there no friction; and the effect of friction is not to throw back the place of high water about half a quadrant, but about three half quadrants. These are the relations which have to be explained; whereas the ordinary explanation deals with relations which have no existence, not even a theoretical existence, in nature. Now, it would have been very useful if Professor Newcomb had given an original and effective explanation of the true theory of the tides. Sir G. Airy has given one, but it is not in the books; Sir Edmund Beckett has given another, in his fine work, "Astronomy without Mathematics"; but the conditions he imposed on himself prevented him from giving the best explanation, though he has avoided mathematical reasoning. We should, then, have been disappointed if we had merely found that Professor Newcomb had left the tides entirely unexplained, or if he had simply repeated the usual incomplete explanation. Unfortunately, he has done worse than this. He has given an explanation which is entirely incorrect. He describes the earth, and truly, as circling once in a month around the common centre of gravity of the earth and moon; but he attributes to different parts of the earth as she thus moves different degrees of centrifugal force, according to the distance of each part from the centre of gravity just mentioned. If these different degrees of centrifugal force (or rather tendency) in reality existed, the tides would be far more important phenomena than they really are. A calculation which Professor Newcomb might have made on his thumb-nail as he wrote the

passage would have shown that the tidal action of the moon alone would be thirty times greater than the real action of the sun and moon together. In reality, the difference of centrifugal tendencies thus imagined has no existence—a circumstance which no one would have recognised more readily than Professor Newcomb, had he thought of examining the matter carefully. The motion of the earth around the common centre of gravity of the earth and moon may be thus illustrated: Conceive a ball 8 in. in diameter suspended by a long thread, and not rotating: then imagine the point of suspension carried steadily round in a horizontal circle 6 in. in diameter. Thus the centre of the ball will be carried round, and so will every point in the ball, in a horizontal circle of the same diameter. Neglecting the earth's rotation on her axis, which is an independent movement, her motion round the centre of gravity of her own mass and the moon's takes place in this manner, each point in the earth (whose diameter is about 7,900 miles) describing, once in a lunar month, a nearly circular orbit about 6,000 miles in diameter. Professor Newcomb's mistake consists in supposing that the motion is of a very different kind, such, for instance, as our illustrative ball would possess, if it were twirled round on a knitting-needle thrust through it at a distance of 3in. from the centre, and held in an unchanged upright position while rotated, carrying the ball with it. In this case, points near the needle would move in small circles, while points farther away would travel in large circles, those furthest off travelling in a circle 14in. in diameter. Thus there would be different centrifugal tendencies in the different parts of the ball. If the earth moved in this way round the centre of her monthly orbit, while also rotating on her axis once a day, and round the sun once a year, we should have the state of things imagined by Professor Newcomb. But our opportunities for observing the result would be precarious; for the tidal waves would be of portentous magnitude, and at least half the present land surface of the earth would be uninhabitable. It is strange that it should not have occurred to Professor Newcomb that, if his explanation of the lunar tides were correct, the solar tides explained on the same principle would be utterly insignificant compared with the lunar ones, instead of bearing to these about the proportion of 2 to 5. His mistake in this matter is a curious illustration of the errors into which even the profoundest mathematicians may fall in careless moods. It can only be compared with one which Lord Brougham is said to have made in one of the earliest publications of the Society for Diffusing Useful Knowledge. Professor Tait, in a lecture delivered before the British Association at Glasgow, stated that in such a treatise, quickly withdrawn from publication, Brougham explained that a man carries a load more readily over his shoulder than suspended from his hand, because in the former case it is further from the centre of the earth, and gravity diminishes as the square of the distance from the earth's centre increases. The story seems incredible, but it is scarcely more remarkable than that a mathematician like Newcomb should employ reasoning as unsound in reality as that of those who deny the moon's rotation. In fact, Newcomb's paradox is very similar in character to that of Messrs. Jellinger Symons and H. Perigal, though not quite so obviously erroneous.

It is in some respects even more remarkable that Professor Newcomb should have given an equally erroneous explanation of the precession of the exquinoxes, or rather of the motion to which precession is due—the reeling of the earth like a mighty top, each reel lasting for the long period of 25,890 years. The subject is, indeed, far more difficult to explain to the non-mathematical student than the tides. But for that very reason we should have expected to find our author on his guard against mistakes. The ablest mathematician may trip in explaining off hand an easy subject, precisely as the ablest gymnast may fail when lightly essaying some simple feat. But in dealing with such a subject as the precession of the equinoxes, even the ablest mathematician girds up his loins as for a task of difficulty. Yet Newcomb's explanation of the phenomenon is altogether erroneous, though his statements respecting the nature of the phenomena are, of course, entirely correct.

The explanation of the peculiarities which theory indicates as affecting the figure of the moon, though observation has not yet demonstrated their actual existence, is also erroneous. It brings our author so close to the paradox of Jellinger Symons (earlier propounded by Bentley), that one cannot but wonder how he failed to notice the mistake which underlies his reasoning.

The account of the Harton Colliery experiment for determining the mass of the earth is incorrect, and the principles on which the experiment depends are not properly stated. Professor Newcomb says that if the density of the earth increases as we approach the centre, the diminution of the force of gravity will be less rapid as we descend. But in reality the actual increase of density towards the earth's centre causes gravity to increase as the depth below the surface increases. This increase continues to a depth hundreds of times greater than can be reached by man. Our author goes on to say that "a determination of the density of the earth by the diminution of gravity in a mine was made by Professor Airy at the Harton Colliery in 1855." But in this experiment Airy found gravity greater at the bottom of the mine than at the top. Owing to this increase in the force of gravity, the pendulum at a depth of 1,260 ft. gained $2\frac{1}{4}$ seconds per day as compared with its indications at the mouth of the mine. It is, by the way, worth noticing, though so far as we know the point is not mentioned in any of our treatises on astronomy, that even if the density of the earth were uniform, gravity at the bottom of a deep opening would be greater or less than at the surface, according to the shape of the opening. It can be shown that if the earth were at uniform density, the action of gravity on a body at the bottom of an opening would be equal to the attraction which a mass equal in all respects to the removed matter would exert on that body (only this attraction must be regarded as acting towards the earth's centre), added to the attraction due to the body's distance from the centre. The latter portion is less than gravity at the surface, in just the same degree that the distance of the body from the earth's centre is less than the earth's radius: but the

* Some one in America, criticising the astronomical articles in the "American Cyclopædia" (Appleton), which were revised, and in great part re-written, by me, was kind enough to point out that my explanation of the precession of the equinoxes was not new. It was not mine, but was left by me almost untouched, being well written, and correct. I left, for a like reason, the matter relating to precession in the "Encyclopædia Britannica" almost untouched when the article on astronomy was entrusted to me for revision and re-writing. Possibly, if I had endeavoured to find an entirely new explanation, I might, like Professor Newcomb, have come upon one which, though new, was not true. At any rate, where so skilful a mathematician went astray, none need be ashamed to err. I believe that in a later edition of Professor Newcomb's book, the errors pointed out above, and in my earlier review in the *Contemporary*, have been corrected. They were, at any rate, corrected in American journals. Professor Newcomb is one of those who are strong enough to be able and willing frankly to admit and correct such mistakes as all active thinkers are bound to make from time to time.—Ed.]

other portion may more than make up for the deficiency, if the opening is wide enough. The mine method of determining the earth's mean density is, in any case, subject to great uncertainty; and few astronomers now attach much weight to the result of the Harton Colliery experiment.

There is a singular mistake in the following statement respecting Venus in transit:—"It would not be possible to see any indications of an atmosphere in such circumstances; for the reason that the light passing through its denser portions would be refracted entirely out of its course, so as not to reach an observer on the earth at all." By similar reasoning, it could be shown that we ought never to see the sun. For the rays which set out from him directly towards an observer on earth are refracted entirely out of their course, and never reach the observer at all. Precisely, however, as such an observer sees the sun by rays which but for our atmosphere could never have reached him, so solar rays pass through the atmosphere of Venus in transit to an observer on earth, which would never have reached him but for that atmosphere. In fact, the solar light seen close to the black disc of Venus in transit does not come from the part of the sun immediately beyond Venus, but from other parts, or, to speak more correctly, from every other part of the sun's disc.

Professor Newcomb disposes rather too summarily of the interesting discovery made by Kirkwood, that there are gaps in the mean distances of the small planets from the sun, none travelling at $\frac{1}{2}$, $\frac{1}{3}$, 2-5ths, 3-7ths, &c., of the mean distance of Jupiter. "Whether these gaps are really due to the relations of the periodic times, or are simply the result of chance, cannot yet be settled." He says, "the fact that quite a number of the small planets have a period very nearly three-eighths that of Jupiter may lead us to wait for further evidence before concluding that we have to deal with a real law of nature in the cases pointed out by Professor Kirkwood." The law involved is most important in its consequences, leading almost inevitably to the rejection of the simple nebular hypothesis of the solar system, and to the adoption of a theory of the development of the system in large part by meteoric accretion. We may be excused, therefore, for dwelling upon a point which, at a first view, may seem to belong rather to the smaller details of astronomical research than to the broad facts in which the general public take interest. If Professor Newcomb had constructed a graphical delineation of the distances of the smaller planets, he would have rejected at once, we believe, the idea that chance has anything to do with the relation in question. Such a delineation we have before us as we write, and we find the smaller planets most markedly divided into five principal families, the spaces separating which correspond to the following periods, or sets of periods (Jupiter's period being taken as 1):—(i.) $\frac{1}{4}$, 2-7ths, and 3-10ths; (ii.) $\frac{1}{3}$; (iii.) 2-5ths; (iv.) 3-7ths; (v.) $\frac{1}{2}$; and (vi.) 3-5ths. Now the distance corresponding to the period $\frac{3}{8}$ falls in the very heart of the richest of all these sub-families. Here we could hardly expect to find a gap, especially when we remember that the perturbing action due to the correspondence between three periods of Jupiter and eight periods of a small planet would be very small compared with the disturbance due to simpler relations—as where four, three, or two periods of a minor planet correspond with one period of Jupiter, or five periods of a minor planet with two or with three periods of Jupiter. But in reality, the richest sub-family of small planets does open out unmistakably at the distance corresponding to a period equal to 3-8ths of Jupiter's. This distance would be 2·7055, the earth's being unity. Now, there are six of the small planets whose distances lie between 2·686 and 2·701, and six whose distances lie between 2·708 and 2·721; but there is not one whose distance lies between 2·701 and 2·708. In other words, whereas the average interval between successive distances amounts only to 0·0025 in the groups on either side of the critical distance we are considering, there is a gap of 0·007, or nearly three times as great, between these groups.

We have left to ourselves no space to comment on the more speculative opinions expressed by Professor Newcomb. On the whole, he seems to us to show at once more caution and more daring than most of his contemporaries—more caution, inasmuch as he does not accept old fashioned views without carefully inquiring into them; more daring, inasmuch as he is not afraid to accept new views when he finds that there is strong evidence in their favour. Here and there, as where he ascribes the darkness of certain zones of Saturn's rings rather to the blackness of their component satellites than to sparseness of distribution, we are unable to agree with him. But it is refreshing to find an official astronomer, and especially one in Newcomb's high position, prepared to analyse and weigh evidence, instead of merely recording what has been observed. In fine, though we have thought it our duty to point out some errors which might mislead the general reader, we can cordially recommend Professor Newcomb's "Popular Astronomy" as the finest general treatise on the subject since Sir J. Herschel published his celebrated "Outlines of Astronomy."

THE CRYSTAL PALACE ELECTRICAL EXHIBITION.

Sixth Notice.

We commence this week a description of the various systems of incandescent lighting now being exhibited at the Crystal Palace. Before describing the different lamps, a word or two on their general principles may not be out of place. In the first instance, we must understand that a current of electricity, in passing through a substance, tends to heat it in exact proportion to the difficulty experienced by the current in so passing, just as a flow of water in traversing a pipe brings about a greater or less exaltation of temperature. This arises from a never-failing law that, where motive power or force is hindered or opposed, that force is converted into another force which we know as heat. Theoretically, no substance allows electricity to pass through it unimpeded, any more than water can pass through a pipe without having more or less friction to overcome. In practice, we allow a pipe large enough to carry the water, without having to resort to any undue pressure; so also in sending an electric current from place to place, we provide a good *conductor* of electricity for the purpose. Furthermore, suppose, in the case of the water-flow, that the pipe is not uniformly wide or smooth, or that here and there quantities of sand, &c., have accumulated, then at these particular places the flow will be impeded, and heat produced, while the general rate of progress can only be maintained by applying *extra* force to push the water forward. Could we measure the heat produced, we should find that it just equals the extra force necessitated by the opposition. This has also its analogy in electricity. Let our large smooth pipe be represented by the good conductor of comparatively thick copper wire, and let the obstacle in the pipe have its counterpart in the electrical circuit by inserting a very thin piece of conducting sub-

stance, such as fine platinum wire, then, on passing the current, heat is produced in the thin conductor, because of the opposition or *resistance* its thinness presents to the current. This heat accumulates, so to speak, with the current, and, ultimately, the thin conductor emits a pure white light. Our readers must please understand that light is but the manifestation of a considerable exaltation of temperature.

We have mentioned platinum as the thin conductor: the reason for this is that all metals, &c., even when of the same size, do not conduct with the same readiness or facility, copper being about the best, and platinum the poorest of metals. It then follows that a thin platinum wire (resembling the small pipe with sand in it) will offer more resistance than a copper wire of the same gauge, and, accordingly, will get hotter, and so give out more light. Even, however, if the copper were equal in its resistance, its physical features would, in this instance, preclude its use. It would, in fact, melt, or even volatilise.

Such was the first form of incandescent lighting. It did not, however, prove very satisfactory. Bodies offering more resistance than platinum were required, and it is only natural that carbon in some form or another should be resorted to. It was found, on trying it, that union was made with the oxygen of the air, converting the filament into carbonic anhydride gas. It occurred to Mr. Swan about twenty years since that, if a *perfect* vacuum could be obtained, the carbon would be made more durable, as there would be no oxygen for it to combine with. It is on this ground that Mr. Swan claims, and is almost universally considered to be, the prime inventor of the incandescent lamp. The necessary vacuum, however, could not then be obtained, but more recent inventions in pneumatics have enabled electricians to achieve their

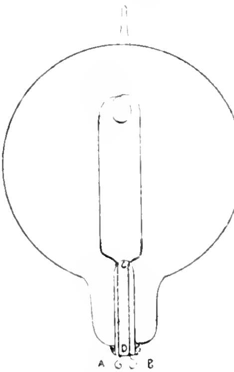


Fig. 1.

object. Even with this vacuum, it was found that the filaments were not so durable as had been hoped, and Mr. Swan must again be credited with discovering the reason. He demonstrated the cause to be the presence of particles of air, &c., located in the spaces separating the particles of the filaments, and by raising his filament to a very high temperature he expelled the air. In consequence, we secure, to all intents and purposes, a perfect vacuum. There being nothing with which the carbon may unite, the supposition forced upon us is that the filament should last for ever. Such an idea, however, is erroneous. Doubtless our readers are aware that in an arc lamp the two carbon rods are consumed, one of them twice as rapidly as the other. This phenomenon is to some extent due to the current, in passing, tearing away the particles of carbon from one rod, and depositing them on the other. This also occurs in incandescent lamps, although to a modified and very much smaller extent. The current, in passing through the filament, tears away minute particles on entering and deposits them on leaving, so that one end is gradually diminished and eventually fractured. This, however, is a process of time ranging from 600 to 1,000 hours continuous burning.

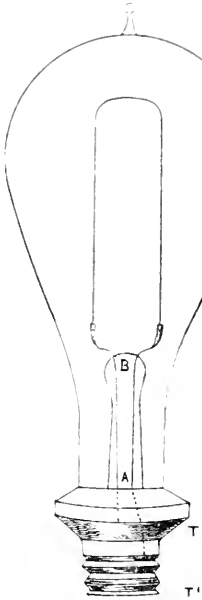


Fig. 2.

The different forms of lamp exhibited at Sydenham are the Swan, Edison, Lane-Fox, Maxim, and British.

The Swan (Fig. 1) is the invention of Mr. Swan, of Newcastle-on-Tyne. It consists of a glass globe nearly 2 in. in diameter. A glass rod, C D, with the two platinum wires, A and B, passing through it, is fused into the globe. The inside ends of the wires are attached to the ends of the carbon filament, which is made from cardboard (Bristol board), and the free or external ends are connected to wires from the machine generating the current.

Fig. 2 represents an "Edison" lamp. It consists of a glass bulb, about 5 in. long. The filament is made of bamboo fibre, one end being, by a very ingenious device, made slightly thicker than the other. Platinum wires are attached to the filament, the junctions being coated, electrically, with a little copper. The platinum wires are fused into the glass tube AB, at B, and are shown in the diagram to be continued, through a kind of cap of plaster of Paris, to a brass band, T, and brass screw-thread, T', respectively. The lamp is then screwed into a corresponding socket, so that T' is in contact with another screw-thread, having attached to it a wire from the machine. The other machine wire is connected to a brass band in the socket which comes into contact with T, and so

completes the circuit. The subject will be continued next week.

P.S.—The engraver made a little mistake in last week's Fig. 2. The fourth needle (from the left) should have been deflected to the left, so as to point to the letter "B."

BRAIN TROUBLES.

THE ECHO SIGN.

A SYMPTOM called the "*echo*" sign, which usually indicates very serious brain mischief, has, like other such signs, its analogue among the symptoms of minor mental trouble. Most of us have noticed how, when we are weary and overworked, we are apt to repeat mentally words or sounds which we have heard or had occasion to utter. Sometimes the tendency becomes exceedingly annoying—a circumstance which, though not necessarily indicating serious mischief, must be regarded as a warning not to be neglected. On one occasion, a time of great domestic trouble, the writer was haunted for two or three days in succession by these three chords, repeated in the way

here indicated. They had been first heard (or imagined, perhaps) during church-time on a calm still Sunday, when, after several days of cold and bitter winter weather, the sun shone brightly, and the air was warm and pleasant. There was illness "even unto death" in the house, and a loss such as changes the colour of life was approaching. But, probably, long and anxious night-watching had more to do with this strange affection of the mind than fear or sorrow. The haunting chords ceased only during sleep (a trained nurse had the evening before taken the writer's place); when consciousness returned after heavy, dreamless sleep, the chords were heard again, now loud and clear, anon distant and indistinct, usually in slow succession, with rather long intervals between each triplet, but at times less slowly and with scarcely the intermission of a single bar's rest. At another time, the writer would probably have been rendered exceedingly anxious by the monotonous repetition of these mental sounds, though he might have found it difficult to determine whether they indicated or were the cause of mental mischief. As it was, other thoughts engrossed his mind too much to allow of any anxiety on this account; and after a few days the chords ceased to trouble him; though to this day he is careful not to allow the mental voice to utter these sounds to the mental ear, lest again the chords should begin to be monotonously repeated. It is probable that this particular mental trouble ceased as it began, apart from any act on the writer's own part; still it may be worth mentioning that he obtained relief, and was, at the time, under the impression that he had driven away the haunting chords by adding, mentally, after each set of six chords, a series of others, as follows* :—

* It is probably not necessary for the writer to explain to musicians that he knows nothing whatever of harmony. Perhaps the above arrangements of chords is full of mistakes, so far as the laws of harmony are concerned, but it represents exactly, first, the chords which troubled the writer, and secondly, those which he added to put the former out of his head.

He noticed that the interval before the paired chords began to be mentally heard again, gradually increased, after the above plan had been followed, until the intervals of silence became so long that the mind could, as it were, forget that it was troubled by these haunting notes.

The "echo" or repetition sign, as we have said, is commonly indicative of serious cerebral mischief. Dr. Winslow was of opinion that it arose, to some extent, from that sluggish and abstracted state of thought, amounting to reverie, which is so often seen in cases of long-existing and sometimes undetected affections of the brain. "The mind seems incapable," he says, "of apprehending, under these circumstances, the most simple questions, and, parrot like, repeats them. I have noticed this symptom in other conditions of depressed vital and nervous power, but it more particularly accompanies softening of some portion of the brain." It can scarcely be doubted that the monotonous mental repetition of words or sounds is indicative of mental trouble; yet not necessarily or probably of any really serious mischief. Rest or change of occupation will in general prove a sufficient remedy. If not, it is time to seek for advice, though rather from a sensible general practitioner (preferably a family doctor) than from those who have directed special attention to cerebral diseases; for the latter are apt to alarm patients by suggesting the possibility, or even the probability, of approaching mental derangement.

As an illustration at once of the morbid phenomena of speech, and of the tendency among certain students of mental disease to exaggerate the significance of such phenomena, we may take the following passage from Dr. Forbes Winslow's book :—"It will not be out of place," he says, "to direct attention to a precursory symptom, not only of approaching paralysis, but of insanity. I allude to the practice of many patients suffering from incipient brain and mind disease, of talking aloud when alone. A distinguished physician observed this symptom to precede an attack of paralysis, in the case of a nobleman who for many years was Prime Minister of this country. In many cases of irritation of the brain, as well as of structural disease, the patient is observed to talk to himself, and the commencement of insanity is often detected by this symptom." True, Dr. Winslow goes on to say that this eccentric habit is consistent with a perfect state of health of body and mind; but these few words suggesting comfort to those who occasionally talk to themselves, are likely to be overlooked in a long passage indicating this common habit as one of the signs of approaching insanity.

SCIENCE AND RELIGION.—The corruption of philosophy, by the mixing of it up with theology, is of wide extent, and is most injurious to it, both as a whole and in parts. . . . This folly is the more to be prevented and restrained, because not only fantastical philosophy, but heretical religion spring from the absurd mixture of matters divine and human. It is wise, therefore, to render unto faith the things that are faith's.—Bacon's "Novum Organum."

EYESIGHT OF DOGS.—Kindly allow me to add my testimony to that of "G. S. S.," under above heading in your issue of March 3. Having been present at many sheep-dog trials in Merionethshire, Montgomeryshire, and Cardiganshire, I am able to assert that sheep-dogs at least have better sight than the average man. I have seen these remarkable dogs, notably at Machynlleth Park, when the signal has been given, run straight to where two sheep had been let loose about half a mile distant, up a hill, covered in places with gorse. Oftentimes the sheep have been out of sight to the spectators when they have been seen by the sheep-dogs, and brought to the bottom of the hill. My experience of dogs leads me to the belief that they are anything but near-sighted. When in the Isle of Wight, I had in my possession a large retriever bitch, that would see me coming along the Whippingham-road long before I was able to recognise the animal.—VERITAS.

EASY LESSONS IN BLOWPIPE CHEMISTRY.

By LIEUT.-COLONEL W. A. ROSS, late R.A.

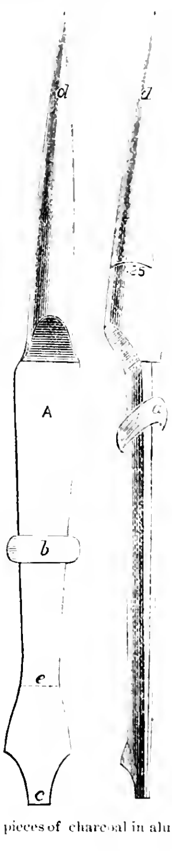
LESSON V.—GOLD, SILVER, IRON, AND MANGANESE—SILICA—TIN—MAGNETICAL PLIERS.

THE auriferous, or gold bearing bead, if now kept steadily at the extreme tip in HP, shows, through a lens, minute specks of gold, and these reactions can be repeated by a good operator over and over again. To sum up the gold reactions. We have P. acid doing BB what no other known acid in chemistry is capable of doing—dissolving pure gold. We then have this dissolved gold caused by the blowpipe to exist in three states of oxidation in its bead: (a) as a transparent, colourless, solution, after a long PP (a pyroxide which, as we shall see afterwards, can also be obtained by treatment on aluminium plate); (b) as a purple solution, after a half-inch PP; and (c) as a brownish-"muddy" solution (blue by transmitted light), after OP. Yet we find it stated in standard works on chemistry and metallurgy, that gold is incapable of pyrological oxidation! Let us now try in our bead that other desirable metal, silver. If you scrape, with a penknife, the rim of a sixpence or shilling over a sheet of paper, you get a few specks of silver, more than sufficient for our purpose; and, perhaps, her Majesty's Mint will not consider it worth their while to prosecute me for recommending such an illegal process in the pursuit, or rather assistance, of KNOWLEDGE. These specks are taken up at bottom of the hot P. acid bead, precisely as the piece of gold leaf was; but a very different result is the consequence of OP. Instead of the "muddy" bead in the latter case, the little silver ball which immediately coheres at the bottom is rapidly oxidised, and gives out a quantity of yellow, opaque matter, like cream, into the bead, flecked here and there with red spots and dashes. In fact (to compare small things with great), our P. acid bead now looks, through a lens, something like a liliputian dish of strawberries and cream, with *very* few strawberries. I will tell you all about the "lens" presently. Our bead is now "argentiferous,"* and the yellow matter (which, I suppose, is pyrophosphate of silver) is rapidly dissolved in this powerful acid, first to an appearance very like that of a thin tortoise-shell comb held against a window; then (after a half-inch PP) to a transparent, colourless bead, which you would never suppose contained silver or anything else. It looks like a drop of pellucid water on the platinum-wire ring. Now, I am going to try the mettle of my student. The following reaction, discovered by me in India in 1869, is rather difficult, but only requires practice until success attends patience and perseverance. The colourless argentiferous bead is plunged, but only for a moment, *into* the "tip" of the blue pyrocone. When drawn out it is found to present a most beautiful and perfect imitation of a *pearl*. The silver-phosphate just under the surface of the bead has been reduced to the metallic state, and shining like silver through the coating or glazing of phosphoric acid, gives the bead the exact appearance of a pearl.

The next substance I propose to try BB in P. acid is that honest prop of our native manufactures—iron. I don't think that, on the whole, rust is a difficult or expensive thing to obtain in this country, especially when one can afford to keep housemaids! However that may be, rust is an oxide of iron, containing two parts of metal to three of oxygen, and is, therefore, thus symbolised by the chemist, Fe_2O_3.† A considerable quantity of finely powdered rust is taken up on the hot P. acid bead, and dissolved by OP; for it is not a highly colouring oxide like that of cobalt or manganese. The bead is very soon topaz-yellow hot, but dissolves a quantity of rust before it retains any colour when cool. Eventually it looks exactly like a drop of watery blood; and I thought that if I now dissolved in it by OP a slight trace of MnO_2, the bead would assume the bright crimson colour of arterial blood; after HP the dark colour of venous blood. Chemists have long ago proved that animal (warm) blood contains iron and phosphoric acid, and I tried this little experiment in order to see if the brilliant crimson colour evidently produced in it by lung-oxidation, might be due to the presence of a trace of manganese in the iron of the blood; I found, however, that the Mn and Fe destroyed each other's colour. I now come to try pyrologically, a mineral which is rather too common, especially in the London streets. I refer to mud; or, as it is more politely called, moistened clay, and still more refined by the chemist into the term "Aluminium-silicate,"‡ which I shall here shorten to Al. silicate.

The treatment of Al. silicate in P. Acid PP. affords one of the most important chemical reactions of the latter, because silica is (except the rare "earth" zirconia) the only substance which is thus insoluble in it. Consequently, when we thus treat Al. silicate (or any other silicate) in it, the alumina or other substance not silica, is at once dissolved, and the silica left as a crystalline mass, more or less thick in proportion to the quantity in which it exists in the mineral. The way I proceed is this:—I first dissolve a single speck of cobalt oxide in my P. acid bead, which makes it pink. I then treat in it the finely-powdered mineral, which I suspect contains silica, and, perhaps, an alkali, as soda or potash. The silica remains undissolved; the alkali dissolving, turns the pink bead blue; alumina in considerable quantity dissolving in absence of alkali, turns the bead mauve colour. Two great Blowpipe-chemists or Pyrologists—Berzelius (a Swede) and Plattner (a German)—used the substance called "microcosmic salt," which is practically a phosphate of sodium, in this way, but the soda of this bead so rapidly dissolves silica as to make its non-solution scarcely any test at all. The whole thing depends upon the amount of heat applied; and I can dissolve to a colourless transparent bead almost any quantity of Al. silicate in Mic. salt, by simply using a powerful blast with my blowpipe.

Natural oxide of tin (*Cassiterite*—Greek, *Kassiteros*, tin) which generally contains a little iron and manganese, may be also usefully treated in P. acid, thus: tin oxide is with difficulty soluble in P. acid, and requires great heat; iron and manganese oxides, on the contrary, are extremely soluble in it by moderate heat, so that, by holding the bead having cassiterite powder taken upon it in a position where it is affected by a moderate (blowpipe) temperature only, as in a half-inch PP, you obtain the same effect as the chemist does when he uses a dilute acid—that is, you dissolve the Fe. and Mn. oxides, leaving oxide of tin, which can be dissolved out of the bead in nearly a pure state by boiling water, and filtering the solution of iron and manganese.



I shall now conclude this lesson with a description and drawing of an implement (the watch-maker's pliers) mentioned in Lesson II., which I have modified and altered so as to make it answer several pyrological purposes when required; and these uses are all very much, and often, required. I have got an ironsmith of Acton (Mr. Poore) to make me one, from which this sketch is taken; and very nicely he has made it. He has also promised to make such pliers, and other blowpipe implements, at a moderate price (I will give it in the next lesson) for poor pyrologists, if required in sufficient number; in fact, I suppose he calls himself a Poore-pyrologist already, for he has been taking measurements of my hot-gas burner, in order to make one for himself. The figure, I think, which is of the natural size, explains itself. The large round legs (apparently, perhaps, too large, but not so when practically tested) enable the operator, by holding the very tip of a straight platinum wire, held firmly between them from below at the part marked ·25, and gently turning the pliers round with his right hand towards his left, which holds the wire, to make a good round ring ·25 or quarter of an inch in diameter, such as that figured in Lesson II. The wire is straightened and cleaned (as explained in Lesson II.) by squeezing and drawing it gently through the broad, flat part of the legs at A, and this process is much more effectual than that generally recommended by chemists, to dip the red-hot wire in hydrochloric acid, which also most certainly rots the wire, and causes it to break off after a time.

The ringed wire is now fixed between the legs as far as *d* (not further) by drawing up the brass collar *a* to the position shown; by drawing it down to *b*, the legs of the pliers are left slightly open, enabling them to pick up small fragments; by drawing it to the very bottom *e*, the legs are left wide open, and useful for taking up pieces of charcoal in aluminium-plate operations, &c. The pliers,

* Latin *Argentum*, silver, and *fero*, I bear—silver-bearing.
† Fe., short for *ferrum* (Latin), iron.
‡ (Latin) *Alumen*, clay.

which are made of good steel, are now to be utilised as a horseshoe magnet in the following manner:—Left wide open, the pole of a big horse-shoe magnet (the use of which I am sure would be allowed for a few minutes by any enterprising and generous optician in London) marked "N," is to be carefully and squarely drawn down either side (say, that shown in the figure) from the part marked *c* to the tip of the leg beyond *d*: this is to be repeated, say, twelve times. Then the plies are to be turned over, so that the other side is uppermost, and it is to be, in like manner, gently but firmly rubbed by the magnet, always in the one direction, from *c* to *d*, by the other S. pole of the big horse-shoe. The Pyrologist will now possess quite a strong little magnet for testing iron, &c., in minerals, and by closing the legs with the brass collar, as at *a*, no armature is required. The end *c* acts as a charcoal borer or small screw-driver.

I am requested to mention that Messrs. Herring & Co. decline to sell P. acid retail. I get mine from Mr. Wooster, chemist, Broadway, Turnham Green.

THE USE OF THE TRICYCLE.*

By Dr. B. W. Richardson, F.R.S.

I WOULD specially recommend persons who are excessively nervous and of uncertain mind not to use the tricycle. In such people the anxiety attendant on the exercise is injurious, out of proportion to the service that is gained by it. They are ever on the strain to avoid accident and danger, and ever on the look-out for accident and danger. From these causes they fail to obtain a good command over the instrument. They are not certain what to do when other vehicles meet or pass them; they are not sure how to take a turning; they are in doubt as to the mode of going down-hill, and of resting in going uphill; altogether they are perturbed by the attempts they make beyond the value of the attempt. If, therefore, persons of this nature do not, after a few weeks of fair trial, get over these anxieties, they had better not continue to court them. I would strongly recommend all who have a sense of giddiness or of sinking and sickness, after they have made a little way on the tricycle, to give up the exercise, unless after a short training they find these sensations pass away. Or, if while climbing a hill there is felt a sensation of fulness in the head, with a want of power and precision in managing the machine, I would tender the same recommendation. Again, I would as a general rule recommend those who suffer from the affection called hernia not to become tricyclists; and if they break this rule, I would earnestly recommend them to be moderate in their exercise, and not endeavour to compete with their more favoured comrades.

Paterfamilias is often joked by his young friends that he cannot perform their feats, cannot stand on his head, or give a back, or, as the late John Leech forcibly and famously put it, leap over a walking-stick. For these stiff-jointed inactives the tricycle comes in with great force, if they use it with judgment, and do not trespass too much on reacquired skill. During the late autumn, I accompanied a fellow-rider who, though many years older than myself, could beat me in getting along, and who told me that before he began he was so rigid in muscle and joint, he could scarcely get into the machine. A few weeks' practice had set him at liberty from head to foot with such effect that in walking and riding—for he invariably walked up steep hills, pushing the machine before him—he could average his five to six miles an hour for five or six hours per day, and think nothing of the task.

It would seem, at first sight, that men who are fat and cumbrous are not quite the persons to mount the tricycle; but, if such men are in fair health, they are, after a little careful and judicious training, benefited by it more than any others. It constantly happens that men of this build, while they feel the need for exercise more than the slighter-built sort, are unable to take a proper amount of exercise, because of the great weariness which they experience soon after they have walked even a short distance. The natural result of this easy sense of fatigue is that exercise is given up almost altogether in a great number of cases, while in other cases it is a mere pretence; so that practically a habit is developed which promotes an objection to exercise, and a steady increase of all the dangers which follow upon prolonged muscular inactivity. In this state, they who are affected are apt to follow one of two courses, both of which are bad. They either settle completely down to repose, and attain a form of chronic feebleness, which requires to be provided against by avoiding every kind of lively effort; or, taking sudden alarm at some sensation they have experienced, or some observation they have listened to, they rush into forms of violent exercise, such as climbing mountains, or volunteering, or making forced walks, or such-like efforts. I believe I have seen more mischief induced in the class of persons whom I am now describing, by their attempts to get into condition through the means of excessive exercise and physical strain, than in any other class.

They who court this mode of recovery from their helplessness are of all less fitted to bear sudden strain. In them the muscles are feeble and out of play; in them the muscles, including that most important of all the muscles, the heart, are overloaded with fat; in them the blood-vessels are often weakened, and have lost their natural resilience, if they have not undergone actual change of structure; and in them the breathing organs are in such bad form for extra work, that breathlessness is produced by very little extra exertion. They are, in short, unfit for walking, and they are equally unfit for those extremer measures which are commonly designated as training, or as athletic exercises. To this class of persons, then, if they are not subject to actual disease, organic affection of the heart, the lungs, or the brain, the exercise that may be got from the tricycle is exceedingly useful.

The exercise sought in this manner should not be violent; it should not include attempts to go against time; it should not include attempts to climb steep hills or to run down steep hills at a rattling pace; but it should be taken for some time on level ground, it should be carried on to a point just short of fatigue, and it should be increased little by little each day, until the labour of working accommodates itself to easy habit.

* From "Tricycling in Relation to Health," by Dr. Richardson, in *Good Words* for March.

CHANGE OF HABIT IN ANIMALS.

AS an instance of change of habit in animals, as noticed in your issue of the 3rd inst., with regard to the carnivorous parrot, I may mention that within the last five or six years the baboons of a certain region in the colony of the Cape of Good Hope have developed a flesh-eating taste.

I do not hear that this has become general among the baboons of the colony, but I know that some farmers in the mountainous parts skirting the Karroo plains towards Graaff-Reinet have suffered severely through their depredations among their flocks. Formerly these animals were satisfied with prickly pears and other kinds of fruit, birds'-eggs, locusts, and, as a rare treat, scorpions, which I have seen them busily turning over huge stones to obtain. I have never been near enough to see the process of killing the scorpion, but my husband told me that they most cleverly divested the little reptile of its sting before trusting it between their teeth; but never till of late were they known to kill a sheep or a lamb for food. I have also been told by one farmer that the baboons on his place had killed many quite young lambs, and torn them open, merely to obtain the milk contained in the stomach. Is this preliminary to, or a farther development of, the carnivorous propensity?

As a more pleasing change of habit and adaptation to circumstances in the same part of the world is the fact that a small bird, called by the natives "Tink-tinky," on account of its notes, and by the Dutch colonists, "Kapoek (frost) vogel," from its snow-white nest, which, before the introduction of sheep, made use of the silky fibre of the wild cotton as material, now uses the wool which it gathers in sufficient quantities from the Nimosa and Euphorbia thorns, finding probably that it is more easily woven into the thick felt of which the beautiful little bottle-shaped nest is composed, merely placing a little of the silky fibre in the bottom of the nest as a lining.

M. Carey-Hobson.

THE "SOUND" OF FISHES.

I OBSERVED some few weeks back (Knowledge, p. 295) that Mr. Mattieu Williams objected to my naming the "air-bladder" of a fish the "sound," Mr. Williams's contention being that this latter name should more properly be applied to the *aorta* or chief artery of the fish. With all deference to Mr. Williams (who is not a biologist, as "Old Fossil," in your last issue, remarks), I maintain that he is confusing, not two names merely, but two distinct systems of terminology. It is perfectly immaterial to me, as a naturalist, *what* fishmongers or fishermen call the "sound." As a zoologist, I have no concern with the terminology of fisher-folk or of fish-dealers. What does concern my readers and myself, is that I should use names *which are in common use in natural history science*. The name "sound," as a popular name for the "air bladder," has been in use in natural history class-rooms for many years back, and whatever be the justification for its use, I maintain I was entitled to employ it, for the reason just mentioned. The question raised by Mr. Williams amounts to this:—Whether a scientific or a fisherman's use of a particular name is to be held as correct. Personally, I prefer to call the sac in question the "air-bladder"; but I cannot, at the same time, permit Mr. Williams to suggest that my usage of the name is an erroneous practice. "Sound," in fact, is a scientific as well as a popular term. It is not my fault if the meanings attached to it are of varied nature.

Andrew Wilson.

ELECTRO-MAGNETIC THEORY OF LIGHT.

IN a recent number of Knowledge you wrote:—"A correspondent who gives us no name, asks us to explain the electro-magnetic theory of light. We know of no such theory. The writer, who says the undulatory theory is fast being swallowed up piecemeal by the electro-magnetic, knows very little about the matter. The evidence for the undulatory theory is simply overwhelming."

Most will agree in your two last conclusions; but as there is an electro-magnetic theory of light, possibly ignorance of its existence on the part of the most omniscient man I know ["Oh Lord, sir! Why there 't serves well again."—Ed.], may justify compliance with your nameless correspondent's request for an explanation of it. It is, in fact, an undulatory theory, and only a modification in detail of the theory of Huyghens. Clerk Maxwell suggested the electro-magnetic theory as a means of determining the relation between the phenomena of electro-magnetism and those of light, based upon the assumption that each of these is due to certain modes of motion in the all-pervading "ether" of space, the phenomena of electric currents and magnets being due to streams and whorls, or other bodily movements in the substance of the ether; while light is due to vibrations to and fro in it. Dr. Thompson writes on this subject as follows:—"Here is evidence for thinking that magnetism is a phenomenon of rotation, there being a rotation of *something* around an axis lying in the direction of the magnetisation. Such a theory would explain the rotation of the plane of polarisation of a ray passing through a magnetic field. For a ray of plane-polarised light may be conceived of as consisting of a pair of (oppositely) circularly-polarised waves, in which the right-handed rotation in one ray is periodically counteracted by an equal left-handed rotation in the other ray; and if such a motion were imparted to a medium in which there were superposed a rotation (such as we conceive to take place in every magnetic field) about the same direction, one of these circularly-polarised rays would be accelerated and the other retarded, so that, when they were again compounded into a single plane-polarised ray, this plane would not coincide with the original plane of polarisation, but would be apparently turned round through an angle proportional to the superposed rotation. An electric displacement produces a magnetic force at right angles to itself: it also produces (by the peculiar action known as induction) an electric force which is propagated at right angles, both to the electric displacement and to the magnetic force. Now it is known that in the propagation of light, the actual displacements or vibrations which constitute the so-called ray of light are executed in directions at right angles to the direction of propagation. This analogy is an important point in the theory, and immediately suggests the question whether the respective rates of propagation are the same. Now, the velocity of propagation of electro-magnetic induction is that velocity "v," which represents the ratio between the electrostatic and the electro-magnetic units, and which (in air) is believed to be $2 \cdot 9857 \times 10^{10}$ centimètres per second. And the velocity of light (in air) has been repeatedly measured (by Fizeau, Cornu, Michelson, and others), giving as the approximate value $2 \cdot 9992 \times 10^{10}$ centimètres per second. The close agreement of these figures is at least remarkable. Amongst other mathematical deductions from the theory may be mentioned the following:—(I.) all true conductors of electricity must be opaque * to light; (II.) for transparent media, the specific inductive capacity ought to be equal to the square of the index of refraction. Experiments by Gordon, Boltzmann, and others, show this to be approximately true for waves of very great wave-length. The values are shown below. For gases the agreement is even closer—

	K	N^2
Flint Glass	3·162 ...	2·796
Bisulphide of Carbon .	1·812	2·606
Sulphur (mean)	1·151	1·024
Paraffin...	2·32	2·33

A. K. Rollit.

OUR ANCESTORS.

MR. GRANT ALLEN, at p. 351 of Knowledge, in describing Palæolithic men, says their flint implements are found buried under the concreted floors of caves. This no doubt is true, but he goes on to say that, "since the days of Palæolithic men, Britain has been submerged beneath several hundred feet of sea." Surely Mr. Grant Allen can hardly expect your readers to accept this statement. That Great Britain may have been submerged since the earlier Palæolithic men lived upon what is now Britain is, perhaps, possible, but that Britain has been under the sea since the Palæolithic men of the caves lived here is, I think, a stretch of the writer's imagination. Close to where I live in London, implementiferous gravels and sands are common, in one great deposit of loam and sand at Stoke Newington, Palæolithic implements are only four feet or less beneath the surface, and the surface is loam or sand full of fresh water shells, with no trace whatever of the sea in any part. These Palæolithic deposits have never been under the sea. Mr. Allen there describes the men as low-browed, fierce-jawed, crouching creatures, inferior to the existing Australians or Andamar islanders, &c., rather a complete and sweeping description, when it is remembered that no complete skeleton of any individual of Palæolithic times has yet been seen, and not enough scattered bones are known sufficient to build up a single skeleton.

* In some crystalline bodies which conduct electricity better in one direction than in another, the opacity to light differs correspondingly. Coloured crystals of Tourmaline conduct electricity better across the long axis of the crystal than along that axis. Such crystals are much more opaque to light passing along the axis than to light passing across it. And, in the case of rays traversing the crystal across the axis, the vibrations across the axis are more completely absorbed than those parallel to the axis: whence it follows that the transmitted light will be polarised."

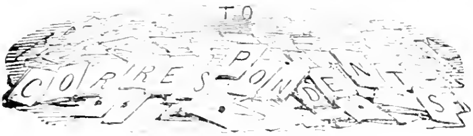
I should feel inclined to question the correctness of the greater par of the paragraph in which the above statements occur, but howeve: low-browed and fierce-jawed these "black fellows" may have been I can make one statement about the works of the Palæolithic mer who lived where north-east London now is, that few people will be inclined to deny who have ever seen the objects I refer to, and tha statement is, the flint implements the Palæolithic men have lef behind them here, are as perfect works of art in their way as any thing ever made by the best artificers of ancient or modern time: The implements found here are often models of symmetrical beaut: indicating eyes perfectly trained to the appreciation of true curve and beautiful and appropriate forms. The workmanship of thes tools shows the work of skilful and delicate fingers, workmanshi that to the present day simply defies the efforts towards imitatio of the most accomplished modern forgers.

That the time is immensely distant since the older Palæolith: implements were made, and that the older tools are generally ver rude, I quite admit, but Palæolithic times lasted for a long perio and the gravels containing the largest number of Palæolithic implements have certainly never been under the sea since the implements were made.

Worthington G. Smith.

Intelligence in Animals.—About three years ago I was livig in a street in Birmingham. At the back of the houses ran strips f garden, or half garden and half courtyard, separated from oe another at the further end by party walls about 4 ft. 6 in. hig. The grating over the drain in our court was broken, and rats one or twice paid us a visit, and once the cat vigorously pursued one il over the court. The cat was not full grown and did not manage o catch the rat, which retired out of the cat's reach beneath a watebutt raised on single bricks. Pussy watched and waited for awhi:, but obtaining no help from human friends, suddenly jumped up ad dashed over the party wall and in less than two minutes returnd full speed with another cat, eager *both* of them apparently for a renewed chase. Unfortunately the rat had meanwhile discreety retired to the drain. A cat which I had in Birmingham, and anothr which I have now, both entirely untaught, have a trick of rattlig the handles of the doors with their paws when wishing either o enter or leave the room. When *in* the room *with us* she, after pawig the handle, looks round beseechingly to us.—A. M. F.

Reasoning in Animals.—Many most interesting cases have beo given in Knowledge on this subject. Will you allow me to supplement one which, I think, surpasses all yet given. One of or seamen took a large, white-coated bitch at Petropauloski, in Kamschatka, and she became a great favourite with the crew. Subsequently the ships were ordered to China, and lay off the Englis Hongs at Canton. It was customary to allow the bitch to swi ashore for a run, and she used to swim back to the ship afterward, and be hauled on board by placing her neck and fore-legs in a bowlingknotted noose. But the tide running furiously in that part of th river, the dog had no chance of making the vessel by pushing off fro the shore ahead of the ship, but used to come down to a low wha opposite, and watch for bits of wood, &c., floating, to see which wa the tide ran, and then running up in a contrary direction, woul jump in, and come down with the tide to the ship. On one occasio she was seen to watch for some time; but, as it happened, nothin floated by to give her an indication of the direction of the tide Whereupon, she was observed to drop down on one foreleg, anc the planks of the wharf being almost level with the water, hang th free paw over into the water, by which means, apparently, sh obtained the knowledge of the tide's direction, and, running u against it, came off, as usual, quite safely. She was one of th finest water dogs I ever saw. I well remember her jumping of our gangway at Aden, 16 feet from the water line.—Percival A Fothergill, B.A., F.R.A.S.



Letters to the Editor.

[The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with their writers. All communications should be as short as possible, consistently with full and clear statements of the writer's meaning.]

All Editorial communications should be addressed to the Editor of KNOWLEDGE; *all Business communications to the Publishers, at the Office, 74, Great Queen-street, W.C.*

All Remittances, Cheques, and Post-Office Orders should be made payable to Messrs. Wyman & Sons.

⁂ *All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.*

All Letters or Queries to the Editor which require attention in the current issue of KNOWLEDGE, *should reach the Publishing Office not later than the Saturday preceding the day of publication.*

(I.) Letters to have a chance of appearing must be concise; they must be drawn up in the form adopted for letters here, so that they may go untouched to the printers; private communications, therefore, as well as queries, or replies to queries (intended to appear as such) should be written on separate leaves.

(II.) Queries and replies should be even more concise than letters; and drawn up in the form in which they are here presented, with brackets for number in case of queries, and the proper query number (bracketed) in case of replies.

(III.) Letters, queries, and replies which (either because too long, or unsuitable, or dealing with matters which others have discussed, or for any other reason) cannot find place here, will either be briefly referred to in answers to correspondents, or acknowledged in a column reserved for the purpose.

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

"God's Orthodoxy is Truth."—*Charles Kingsley.*

Our Correspondence Columns.

VEGETARIANISM. — PLANTS IN BEDROOMS. — "THE BURIAL OF MOSES."—DESTRUCTION OF SODOM AND GOMORRAH.

[326]—I must thank Sir Henry Thompson for a personal reply, which is as welcome as it was unexpected. In a discussion on vegetarianism that has been recently going on in the *Echo* evening newspaper, his name, and that of Dr. Richardson, have been definitely quoted (among those of a number of obscurities) as advocates of a purely vegetable diet. Now it can matter little to the public, and assuredly need not influence it, to learn that Mr. Smith, of Hoxton, has brought up a large family on French beans and cabbages, or that Mr. Brown, of Soho, consumes (and believes in) "The Nutriment of Longevity," which he himself sells; but the case is very different when it is definitely predicated by men of world-wise eminence, like Sir H. Thompson and Dr. Richardson, that they are, in effect, vegetarians. I may say that before penning the letter (285, p. 362) which has elicited Sir Henry Thompson's valuable rejoinder, I had learned privately that both himself and Dr. Richardson did, as a matter of fact, employ a mixed diet themselves. This being so, I thought that a proclamation of the truth would enable all interested in the food question to estimate the *bonâ* (or *malâ*) *fides* of the vegetarians who quote two distinguished physiologists as converts to, or advocates of, their system, and to appraise the exact worth of the argument which they profess to derive from such authority.

I will tell "A Fellow of the Chemical Society" (letter 315, p. 40) one way in which I believe the wretched potato diet does "influence Irish character." It may be gathered from the reports of the hideous outrages which unhappily reach us daily from Ireland, the details of which show, beyond cavil, that their perpetrators are the most arrant curs upon the face of the earth. Firing from safe concealment behind stone walls, and running like hares the moment their barrels are emptied; breaking, in overwhelming force, disguised, and with blackened faces, into lonely houses, and shooting and maiming solitary and defenceless men and women; skulking away at the mere sight and sound of resistance, and never daring to face a human being who is in a position effectually to oppose them, these people present an example of an arrant cowardice which happily disgraces but few (so-called) "civilised" races in the world. Now, enlist these men, give them their daily ration of good, wholesome meat, and what do we find? Simply, that there is no braver soldier in existence than the Irishman. Where are there finer regiments to be found than those mainly recruited from Ireland? Why, I have myself known an Irish private, totally unarmed, go in and seize a comrade half frantic with drink and armed with a bayonet with which he had previously kept several men at bay. As an example of cool courage, uninflamed by the excitement of action, this seemed very striking to me, and certainly in odd contrast to the pusillanimity of his vegetable-fed *confrère*. Finally, I would say that I am by no means convinced that the chemical hypothesis of nutrition is invariably sustained by experience. I can remember when Liebig's theory was regarded as being as unassailable in its entirety as the Apostles' Creed. Is this the case now? Growing plants, I may tell "J. C. L." (query 288, p. 410), are unhealthy in bedrooms from the fact that at night they give out carbonic acid. In sunlight, on the other hand, they exhale oxygen and keep the air of a room pure. In reply to his next query (289, same page), I fancy that he will find the poem he inquires for in "Maunder's Class-book," also in the "National Reading Book," Book V., published by the National Society, Westminster.

"A. N." (query 303, p. 410), may rest assured that no scientific proof whatever exists that Sodom and Gomorrah were destroyed on July 31, 1898 B.C., either by aerolites or in any other way.

A FELLOW OF THE ROYAL ASTRONOMICAL SOCIETY.

TELESCOPES.

[327]—While fully endorsing the opinion given by H. Sadler in KNOWLEDGE, p. 389, as to the advantage of a silvered glass reflector, I beg permission to add that by means of a tin cap, with a handle like the lid of a saucepan, fitting closely on the cell of the speculum, the film may be preserved, if not in its original beauty, yet in a perfectly serviceable condition for so long a time, that the expense of renewal will become a matter of comparatively little importance. It must be remembered, however, that this involves either an opening in the side of the tube large enough to admit the hand with the lid (which might be jointed for the purpose) on the removal of the mirror when not in constant use. Circumstances have obliged me to follow the latter plan; but I find no great inconvenience in it, and am convinced that a little ingenuity would supply a mode of effecting it readily, and at a trifling expense. With due precaution, the adjustment need not suffer. T. W. WEBB.

NIGHTS WITH A THREE-INCH TELESCOPE.

[328]—Could you at the end of these articles give the constellation and letters of the stars which will be described in the following article? I make this suggestion in order that amateurs, like myself, may afterwards compare what they have observed with the article in your succeeding number. F. C. B.

THE POTATO.

[329]—In No. 12, Jan. 20, "F. C. S." gives an article on "Something About the Potato," which contains several statements which I had hoped to see flatly contradicted by other and abler hands than mine.

He says, "the influence of the blossoms makes a great difference," and quotes a case showing an increase of about 13 per cent. where these are removed. Let "F. C. S." try a small plot during the coming season, and give us his results. With Victorias, which are about the freest bloomers known, the writer has several times tried it on plots of from one-tenth to half an acre, with no appreciable difference: in fact, if anything, the result was against the mutilation of the plants.

Further on he says, "in frozen potatoes the sugar is doubled and the starch diminished." This, I fancy, happens after or at the time of the frost going out, as I find a frozen potato, if plunged into hot wood-ashes and there cooked, is quite dry and floury, and has none of the sweetness peculiar to a frosted potato.

Again, he says "compost has no effect unless to increase the proportion of starch." It must surely have been poor compost. Please say what was its composition, and the amount of increase of starch?

Again, he says "potatoes grown on moist soils, and soils containing much organic matter, are most liable to disease;" while further on, he says "the unmanured plots are highest in disease." The one statement is a contradiction of the other, and contrary to any result I have ever heard of any one attaining, and certainly far from the results attained by myself.

Further on, he says "Good peat is found to give even better results than good stable manure." This would certainly be good peat, and a mine of wealth to its owner. Will "F. C. S." quote his authority on this matter, as I consider it downright nonsense?

Can "F. C. S." or any [illegible] reader say what is the difference of starch in a [illegible] waxy potato and a dry, floury one; and give any reason for the difference, other than sunlight and a minimum of moisture? FARMER.

HOG PUZZLE.

[330]—Lieut.-Col. W. H. Oakes' solution (see letter 288, p. 363) is the following:—We have to find four pairs of squares having a common difference. This common difference will be

Firstly.—A multiple of 21.

Secondly.—The product of four consecutive terms of an increasing arithmetical progression.

Thirdly.—The product of each of four pairs of factors.

Now $21 = 3 \times 7$. Also, 1, 3, 5, 7, is the lowest arithmetical series of four terms that includes 3 and 7. Also $1 \times 3 \times 5 \times 7$ is the product of each of four pairs of factors, namely, 105×1, 35×3, 21×5, and 15×7. The product of this series gives the lowest number of shillings that satisfies all the requirements of the problem. Therefore, each husband has spent 5 guineas more than his wife.

N.B.—Half the sum and half the difference of each pair of factors will give the number of hogs bought by each man and his wife; so we get 53 and 52, 19 and 16, 13 and 8, and 11 and 4.

[Correctly solved also by J. A. Miles, C., and J. R. Campbell.—ED.]

The original puzzle (see letter 198, p. 232) admits of the following arithmetical solution:—

The number of shillings in 3 guineas is 63. We have, therefore, to find 3 pairs of squares with a common difference of 63. Now, 63 is the product of 63×1, 21×3, and 9×7. Half the sum and half the difference of each of these pairs of factors will give the number of hogs bought by each man and his wife, namely 32 and 31, 12 and 9, 8 and 1. By question, H bought 32, and K 9; also E bought 12, and G 1. Therefore, H and A, E and K, C and G, are the respective husbands and wives.

William Emerson published the original puzzle and its algebraical solution nearly 120 years ago. I append his solution as he gives it.

Let	1	x = hogs some man bought, $x-y$ = wife's hogs.
per q^n	2	the money for the man's $= xx$.
	3	$xx - 2xy + yy$ = wife's money.
	4	$xx = xx - 2xy + yy + 63$.
4 tr.	5	$2xy = 63 + yy$.
$5 \div$	6	$x = \dfrac{63 + yy}{2y}$ = whole number.
$6 -$	7	$x - y = \dfrac{63 - yy}{2y}$ = whole number.
But	8	In this case y must be an odd number, $= 1, 3, 5, 7$, &c., but it cannot be 5.
8	9	If $y = 1$, $x - y = 31$, $x = 32$. $y = 3$, $x - y = 9$, $x = 12$. $y = 7$, $x - y = 1$, $x = 8$.
per q^n	10	A has 32 hogs, and Q 9, Also B has 12, and P 1.
	11	Whence B and Q, C and P, A and R are man and wife.

HERBERT REES PHILIPPS.

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[331]—Let x = the number of guineas each husband laid out more than his wife $= 21x$ shillings.

Call the four men A, B, C, and D, and their four wives a, b, c, and d.

Let p = the number of hogs A bought more than a.
" q = " " B " " b.
" r = " " C " " c.
" s = " " D " " d.

Now, by the conditions of the question, p, q, r, and s, are four consecutive terms of an increasing arithmetical progression. Therefore it follows that p is less than q,
q is less than r, &c.

But the number of hogs bought by each man is equal to the number of shillings given for each hog, therefore—

p hogs cost p^2 shillings,
and q hogs cost q^2 shillings,

and as by the question

$$p^2 = 21x,$$
$$\text{and } q^2 = 21x,$$
$$p^2 = q^2,$$
$$\text{and } p = q,$$

but it has been shown that p is less than q.

Therefore, as things which are equal to one another cannot be greater or less than one another, the question is an impossible one.

J. A. MILES.

THE PERFECT WAY IN DIET.

[332]—When a Fellow of the Royal Astronomical Society (285) writes of the abolitionists of the barbarism of the slaughter-house as being "as weak numerically as they are intellectually," is he really ignorant of the facts (1) that to all intents and purposes two-thirds of the human species are, and always have been (at all events, since the times when they emerged from the universal primeval barbarism), *nolentes volentes*, abstinents from fleshmeats: that it is the richer classes in all communities alone who support the slaughter-house; while the poor, because of the selfishness of the rich, are starving upon the minimum amount of non-flesh foods (upon badly-cooked potatoes and cabbages, it may be); (2) that there are such names in history as Pythagoras, Sakya-Muni (the founder of a religion the most philosophical and most humane, in its essential doctrine, that has ever been preached on the earth, and which has some 300,000,000 followers), Plutarch, Seneca, Porphyry (the most erudite philosopher of antiquity), Clemens of Alexandria, Chrysostom, Gassendi (whom Boyle characterises as "the greatest philosopher among scholars, and the greatest scholar among philosophers"), Mandeville, Evelyn (*Acetaria*), Ray, Linné, Halley, Cheyne, Voltaire, Howard, Wesley, Rousseau, Franklin, Shelley, Graham, Hufeland, Struve, Daumer, Lamartine?

Before this critic attempts again to pronounce upon the value of a creed which has engaged the earnest attention, and in very many cases the entire approbation, of the most profound thinkers of all the best times, let me exhort him to study, *with some attention*, at least, such writings, *e.g.*, as Plutarch's "Essay on Flesh-eating" (Περι της Σαρκοφαγιας), the most remarkable ethical production of antiquity; Seneca's "Letters," &c.; Gassendi; Shelley's "Essay"; Professor F. W. Newman's "Lectures"; and last, not least, Dr. Anna Kingsford's "Perfect Way in Diet." HOWARD WILLIAMS, M.A.

UNIVERSITY OF LONDON MATRIC. EXAM., JAN., 1882.

[333]—"Out of 800 candidates, less than 300 passed." Ought not this result to lead to some inquiry as to the manner in which the examiners performed their duty? From the Arithmetic and Algebra paper given by Dr. John Hopkinson and B. Williamson, Esq., M.A., I wish to place two questions before your readers for their consideration.

No. 5 question is the following:—"Six terms are in arithmetical progression, and also in geometrical progression, and their sum is 54. What are they?" The only solution appears to be that which we find in the multiplication table:—"Six times 9 are 54." No. 9 question occupies nine lines, and is as follows:—"Suppose that gold is worth 15 times as much as silver, and that silver is worth 100 times as much as copper. Find the proportions of the metals in a certain coin worth 4s., having given that a coin with double as much gold, the same quantity of silver, and 5 times (or 5 million times?) as much copper, would be worth 7s. 9d.; a coin having 5 times as much gold, the same silver, and twice as much copper (?) would be worth 19s.; and lastly, that a coin with the same gold, double the silver, and one-half the copper (a half-millionth would do equally well) would be worth 4s. 3d." If we construct the equations, and from (4) subtract (1), and next multiply (1) by (2), and from the product subtract (2), the results will be found inconsistent, unless we assume that the quantity, or rather "proportion," of copper is *nil!*

Will you, sir, or some of your readers, characterise questions such as these? If the object is to baffle and bewilder candidates, by "rejecting" 500 out of 800, the examiners are shown to have attained their end. But will the London Matric. Exam. continue to stimulate the acquisition of "knowledge," if conducted on this principle?—A TEACHER.

MATHEMATICAL PARADOX.

[334]—The following mathematical paradox is the shortest, and at the same time the most difficult, I have met with. It seems to have seriously perplexed even so accomplished a mathematician as Lacroix. I should like to hear the opinions of some correspondents before giving my own.

$$(+a)^2 = (-a)^2$$
$$\therefore 2 \log. (+a) = 2 \log. (-a)$$
$$\therefore \log. (+a) = \log. (-a)$$

a result which is not true. S. L. B.

INTELLIGENCE OF A MONKEY.

[335]—One of the tricks we were in the habit of playing him was this: he had a cord suspended from the ceiling, with a loop just large enough for him to get through; then one of us passed his chain through the loop something like a dozen times in different ways; we then watched the result. After getting into the loop and

stretching it open, he would, with his disengaged hand, take up the chain and look which way (as he thought) it passed through, and, by following the winding of the chain, would speedily untwist the whole, to his utmost delight, and to the astonishment of those who witnessed it. You can imagine the intelligence required when you think of the difficulty overcome. He had to go outside the loop every other turn.—F. SELBY.

PERSONAL ILLUSION.

[336]—To me, horizontal lines appear much plainer than perpendicular lines. Thus, I can tell the time by a clock more easily at, *e.g.*, 3.45 than at 6. So, also, the letters of printed words in small type seem to touch each other; but, if I turn the page sideways, they open out. JUMBO.

EXPLOSION OF AMMONIA.

[337]—The explosion of aqua-ammoniæ, related in KNOWLEDGE of Feb. 10, reminds me of a similar occurrence in my own experience. A bottle of ammonia, having the stopper tied down, had remained undisturbed for some time in my photographic workroom. During some hot summer weather it burst with a loud explosion, and the contents were scattered in all directions. The heat of the weather, no doubt, liberated a quantity of ammonia gas, which, being unable to escape, shattered the bottle. In the account of the accident detailed in KNOWLEDGE, the bottle of ammonia is stated to have been kept on the mantelpiece, and to have exploded in the woman's hand. A mantelpiece at this time of the year is presumably a very warm place, and if the stopper of the bottle had become fixed (as often happens), it is not difficult to understand how the heat of the woman's hand may have proved "the last straw," the bottle having probably been almost at bursting point from the pressure of ammonia gas liberated by the warmth to which it was exposed on the mantel-piece. It is impossible to suppose that an aqueous solution of ammonia could be explosive in the usual sense of the word, and I think the above must be the true explanation of the accident, and a source of danger which perhaps few are aware of. B. A. (Cantab.)

HOW TO PRESERVE FOSSILS, BONES, &c., FROM THE ACTION OF THE AIR.

[338]—1. See that the fossils are washed free of all efflorescence, dirt, &c., and well dried. 2. Take strong Scotch glue 4 oz.; dissolve in glue-kettle, with enough water to make a thick mucilage. Then add 1 oz. of boiled linseed oil, stir well; dissolve ¾ oz. of bichromate of potass in a small quantity of water, and add this to the glue, then thin down with water to the consistency (while hot) of milk. 3. Put the dried fossils or bones into an oven and make them very hot, then drop them into the boiling solution and keep them near the simmering point for half-an-hour. Wipe them with a sponge and expose them to dry in the air and sunshine. If properly managed they will now keep indefinitely.

P.S. The above prepared glue, of the proper thickness, is the best thing for repairing broken fossils, and for making any joints required to be strong and good. F.

A SERIES OF ASTONISHING COINCIDENCES.

[339]—In connection with the editor's papers on chance and coincidence, the following remarkable instance of purely fortuitous coincidence will be of interest. About a month since a friend of mine, Mr. Hunt, went to a small town, about twelve miles from Exeter, to attend a meeting, returning home the same night. While there he called upon the Rev. J. Smith, remaining about an hour. Just before leaving, the postman called at the house, and a letter was brought in addressed "Mr. J. P. Hunt, C——." The servant said to her master, "Please, sir, the postman wants to know if you know who this letter is for, as he does not know the name in the town." As it was distinctly addressed to my friend, he opened it, and found it was from a chum of his in the North of England. I may say that the letter contained nothing whatever of importance. A few days afterwards another letter arrived (at his proper address) from the same fellow, saying: "I wrote you last week, but, of course, you never got the letter, for, like an ass, I directed to C——, of all places in the world." I should say that Mr. Hunt had nothing whatever to do with C——. The postman knew nothing at all about him, and he was not expected at Mr. Smith's. So that the purely fortuitous character of the circumstances that guided the misdirected letter to his hands is complete. H. SNELL.

BRIGHTNESS OF TELESCOPIC IMAGES.

[340]—In a review of Professor Newcombe's work in KNOWLEDGE, the common opinion that large telescopes increase the brightness of objects having visible surfaces is stated to be erroneous. Is this so in all cases? Let the brilliancy of the retinal image of (say) the moon be unity. Then, with a telescope having thirty-six times the light-gathering surface, the image at the focus is thirty-six times as bright. If I magnify this image nine times superficial, the magnified image is one-ninth as bright; but the image at focus is thirty-six times as bright as the retinal image. Therefore, the magnified image is thirty-six ninths, or four times, as bright as the retinal image. If I increase the aperture and retain the same magnifying power, the image should be brighter still. No doubt I am astray somewhere, and should esteem it a favour if you would kindly point out where. Thanking you for past favours. W. KING.

[You will find that where magnifying power is less than increase of illuminating power, the eye can only receive a portion of the emergent pencils. My friend Dr. Huggins had an old telescope intended to give great increase of light (a night glass), and he found the emergent pencils were an inch in diameter—as, he said, "a telescope for a horse, not for a man."—ED.]

THE OWNER OF THE CORONA.

[341]—I was present at the birth of the nursery rhyme to which you refer in your last number, and can vouch for the original form in which it appeared. The part in question ran thus:—

[Our correspondent must excuse our omitting the first two lines and the last, which are to some degree personal. ED.]

. . . .

. . . .

He thought himself owner of half the Corona.

. . . .

The rhymes were made by Dr. Thorpe, a member of the Sicilian Eclipse Expedition of 1870. On leaving Poretta, a station between Verona and Florence, Professor Clifford commenced making nursery rhymes on the names of the stations taken from Bradshaw, and Dr. Thorpe carried on Professor Clifford's idea by rhyming the names of the members of the expedition. There is a moderation about the original recension which recommends itself to my judgment. It will be noticed that the claim set up is only to the ownership of "half of the Corona." The verses were received with great applause by the members of the Eclipse Expedition, who had been travelling with the "owner" for some days. Mr. Rand Capron's version must have been derived from an inaccurate source—perhaps the owner subsequently saw his way to improving them.

A MEMBER OF THE SICILIAN ECLIPSE EXPEDITION.

INTERIOR HEAT OF THE EARTH.

[342]—Your correspondent, "B." (No. 287), will find, if he refers to the Rev. Osmund Fisher's recently-published work on the "Physics of the Earth's Crust," that Professor Mohr's speculations have not escaped the notice of scientific men in this country. He will also find his deductions from the observations made in the bore-hole at Sperenberg very ably combated by Mr. Fisher in the above work.—I am, your obedient servant, D. C. W. HINE.

EARTH'S INTERNAL TEMPERATURE.

[343]—I have not seen Professor Mohr's book, but I recollect the inference he attempted to draw from the temperature observations at the Sperenberg borehole. It is altogether fallacious, and the error has been pointed out by Professor Everett in the "Report of the Committee of the British Association on Underground Temperatures," and by the Rev. O. Fisher in his work on the "Physics of the Earth's Crust." In almost any book on geology—say, "Coal: its History and Uses" (Macmillan), "B." will find an account of the result of the examination of coal plants by competent botanists.

A single instance of asphalte in granite is rather slender evidence for disputing the generally-received views as to the origin of that rock. It may have been a case in which granite has been formed by the intense metamorphism of a rock containing bituminous matter, and if the change was effected deep down under ground, the pressure may well have prevented volatilisation and escape.—A. H. G.

THE RAINBAND.

[344]—I observe in a recent number the discovery of this band in the spectroscope is given to Piazzi Smyth, which may be true, as there is no date affixed, but if so it must have been before April 20, 1871. On that date Professor James Cooke asked me if I had noticed

the dark line which appeared before rain, and, producing a pocket spectroscope, pointed it out. Having purchased, the year before, when at the British Association in Liverpool, from John Browning, a six-prism automatic spectroscope, Mr. Cooke came home with me to test thereon this band, which was now very obvious, and from that day to the present it has been, before rain, a familiar subject to my friends and myself.

Referring to your foot note of the nursery rhyme, which, doubtless, Mr. Lockyer enjoyed as much as anyone (?), these are the words given to me by one of the members of the 1870 Eclipse Expedition, on his return home.

* * * *

Till he of the Corona,
Says, Norman is owner,
And the rest of the sun shines for Lockyer. PRIAM.

EARTHENWARE INSULATORS.

[345] *Apropos* of the electric "Telegraph," "The wires and insulators," KNOWLEDGE, Vol. I., 374, I was, some four or five years ago, having a conversation with an electrical engineer; and he informed me that in the manufacture of earthenware insulators, a large quantity of animal charcoal was used; but if any human bone got mixed with the others and burnt, the insulators (so-called) were useless, as they did not produce insulation, and he told me that he was a loser in a large contract entered into for insulators, in consequence of some human bones having got among others which were used for the charcoal for mixing with the other materials of which the insulators were composed, and not one of which executed its appointed task. Have you ever hear of this peculiarity of human charcoal, and can you account for it? A. T. C.

SPECIAL NOTICE.

FOR some time past our Queries and Replies have involved a serious and growing difficulty, which we had proposed to remedy in some degree by having two numbers of 32 pp. per month. But the difficulty increased so much that this measure would only, we feel assured, afford temporary relief. We must, therefore, adopt a more satisfactory remedy. The difficulty is this: many questions are asked (perhaps about a tenth of those asked appear under head Queries); to some questions ten or a dozen replies are sent in, of which, perhaps, only one can be admitted, while other questions remain unanswered. Correspondents who have written out queries or replies are not well pleased if their labour is wasted, yet much labour must be wasted according to our present arrangement. If, as we proposed, we enlarge our sheet occasionally to admit more questions and answers, there would be delay in many cases, and the bulk of our readers would not care greatly to have simply eight more pages of correspondence, queries, and replies. In fact, we should have not a few letters pointing out that whereas in number, x, 12 pages out of 20, or 3-5ths, were original matter, in number $(x+1)$ there were 12 out of 28, or only 3-7ths, "and this, Mr. Editor, is grossly unjust." We shall hereafter adopt, therefore, a different system with queries. They will be classified, and sent to experts in the departments to which they respectively belong, who will reply to them at greater or less length, according to the nature of the queries, but in such a way that each reply will convey information to others besides the querist. These replies may, from time to time, furnish occasion for correspondence, corrections, suggestions, and so forth; but, for the most part, a query once asked and answered will be finally disposed of. Thus, much less space will be occupied by questions and replies, while much more satisfactory information will be given not only to each individual querist, but to our readers generally. The "Queries" and "Replies" columns will thus be practically merged in "Answers to Correspondents," classified under various headings—Astrology, Geology, Chemistry, Botany, Entomology, and so forth. It is hardly necessary to point out to our readers that this arrangement, by which the usefulness of KNOWLEDGE will be greatly increased, will *not* be altogether so inexpensive as one by which readers are left to answer each others' questions. The proprietors of KNOWLEDGE cannot, therefore, *at the same time*, enlarge the numbers. But when the growing circulation of KNOWLEDGE justifies that course *also*, readers will gain much more by it, as there will be an increase of original matter, instead of a mere growth of the Correspondence columns.

OWING to the illness of engraver, the large picture of the looped path of Mars, with reference to the earth, from 1875 to 1892, has been delayed. It will appear without fail next week.—ED.

IT should have been mentioned that the note on the appearance of Jupiter, in No. 18, was extracted from the *Kansas Science Student*. THE EDITOR.

Queries.

[311]—HEATING ROOM.—Will Mr. W. Mattieu Williams kindly say what are the objections (if any), in a sanitary point of view, to heating a room by means of an atmospheric (Bunsen) burner without a stove-pipe or vent, provided it is kept burning with a perfectly blue flame?—J. W. B.

[312]—PYROLOGICAL.—Will Colonel Ross please say:—1. If, having tried Fletcher's lamp for burning solid fats (modified form for travellers), he can recommend same? 2. Where the 10s. 6d. microscopes he mentions in "Pyrology" are to be got? 3. Where the spectrum lorgnette is to be got, and the price? 4. What fat, or fats, are best for blowpipe work?—JEMIMA.

[313]—BOTANICAL.—Would any one kindly give the name and price of a book giving the derivations and meanings of the various botanical terms?—JEMIMA.

[314]—EYESIGHT.—One of my eyes is only half the focus of the other; what is the best thing to be done?—JEMIMA.

[315]—ASTRO-PHOTOGRAPHY.—Will a reader kindly refer me to a work on this subject, or give a few elementary instructions adapted for a 3¼-inch refractor.—C. J. C.

[316]—ATLANTIC CABLE.—Where can I obtain an account of the laying of the Atlantic telegraph cable of 1865?—W. Y. N.

[317]—SEAL FISHERIES.—How are seal-skins shipped to England, &c., procured? Is it the case that the seal is flayed while alive and conscious, and, if so, how is the animal afterwards treated?—J. H. B. FLETCHER.

[318]—VEGETARIANISM.—I should be obliged if "A Fellow of the Chemical Society" would tell me where to obtain information as to the details of a proper vegetarian diet, so that I could give my household sufficient variety; and also that I might not err, as did those who fed "the prisoners" referred to.—G. A. S.

[319]—BINOCULAR MICROSCOPE.—When using the quarter-inch with binocular microscope, both tubes are not fully illuminated, but there is always a dark shadow in one or other, wherever I place the mirror. Is this unavoidable, or what will remedy the defect?—J. E. S.

[320]—BOTANY.—I am about to commence the study of botany, and should be very thankful for any information as to books, &c. Would Cassell's lessons in the "Popular Educator" be of any use to me? I cannot afford anything that is expensive, as I am only a weaver lad.—F.

[321]—BOTANY.—Providing the ovules in the ovary of a flower were unimpregnated, would they continue to develope, and ultimately to all outward appearance simulate true seeds. Of course, I know they would not germinate. This is against all laws of physiology. But everyone knows that a hen will lay eggs without the visit of the male bird. In replies, please quote references?—JUMBO.

[322]—METEROLOGICAL.—(1) What is the best self-registering rain-gauge, price, and where procurable? (2) Also the handiest cards or sheets for tabulating the readings of the barometer, wet and dry bulb thermometers, rainfall, and prevailing winds? Observations taken thrice daily.—G. B.

[323]—SHOCKING COIL.—Would any reader give full particulars for making a powerful shocking coil, with the best form of battery to use with the same in a small room, as I am desirous of making one.—IN AMBIGUO.

[324]—ELECTRIC ORGAN.—Having an intention of constructing an organ similar to the one at Messrs. Maskelyne and Cooke's, will some reader kindly explain the action, say from the keys to the pipes?—ROBERTO.

[325]—STONEHENGE.—Can you inform me whether the Druids placed the stones there, and if so, by what process?—SUBSCRIBER.

[326]—ANTARCTIC REGIONS.—Required information respecting the geology of these regions. There are volcanoes, but are there stratified rocks? I remember some years ago hearing it stated that elephant's tusks had been found, and I notice in the map an "Elephant Island." Is there any book that would throw light on the subject? I addressed a query to you, which you published, about Valley-terraces in Dorsetshire (No. 261), but no one has thought it worth while to reply.—S. H. W.

[327]—FIRST B.SC. EXAM. (University of London).—Could any reader of KNOWLEDGE tell me the best books to procure for the preparation of the various subjects required in the above?—UNDERGRADUATE.

[328]—SUB TEGMINE FAGI.—Can Mr. Grant Allen tell me why herbage does not grow as readily under beeches as under other

trees? Am I right in thinking that grass under beeches is often of a finer blade than usual? Why does Mr. Allen exclude yellow at page 21 of "Evolutionist at Large?" it surely predominates at least in spring.—M. McC.

[329]—PHOSPHORESCENCE OF FISH. What is the cause of the luminous appearance, so striking in the dark, observable on the inside of a haddock, chiefly about the bones, even after it has been cured?—LEONARD B. P.

[330]—CLIMBING PLANTS. In our hemisphere, and in our climate, of course, the sun rises on our left and sets on our right hand. Climbing plants, such as the hop, in consequence, it is believed, of the action between them and the sun, wind round their supports in the same direction. Perhaps some of your botanical readers will tell whether, in the southern hemisphere (say at the Cape or Australia), as the sun rises on our right and sets on our left, these plants wind round in a similar direction, following the sun, and thus in a totally different manner from those with us. Under the equator, might they not be sometimes puzzled as to the direction they ought to take? W. P. B.

[331]—EVOLUTION AND GEOLOGY.—Dr. Wright, F.R.S., in a lecture at Cheltenham a few days ago, declared the Lias formation to be "the greatest possible stumbling-block to the theory of evolution, for he defied anyone to find in this complete geological chapter the slightest sign of any intermediate steps of life. If palæontologists could determine such steps, the world would soon be convinced of the truth of evolution; but in endeavouring to do this, they would find the zones of life in the Lias a very hard nut to crack." What reply do evolutionists make to this objection?—EUPTERIS.

[332]—MOSSES.—Can anyone recommend me a book giving the English as well as the botanical names of British mosses? Hobkirk's "Synopsis," and Dr. Braithwaite's "Flora" give only the latter.—EUPTERIS.

[333]—SAKKARA TABLET.—Will "A Member of the Society of Biblical Archæology" kindly let me know when the Sakkara tablet, mentioned March 3 on page 379, was discovered, and by whom? What kingly dynasties are mentioned in it, and where is Maspero's correction of these dynasties to be obtained and read?—EGYPTOLOGIST.

[334]—JUPITER.—I should be glad of any information respecting cause and nature of the red spot on Jupiter, also where to find best description of same.—A. H. M.

Replies to Queries.

[31]—INTENSITY COILS.—Give the size and length of secondary wire, together with the method observed in the construction of the coils, then we may be able to assist you in determining the safe limit of battery power. If the insulation has been destroyed by "sparking," it will be necessary to unwind the wire until the faulty part has been discovered, mend the insulation with silk dipped in hot melted paraffin, and rewind the coil.—GEORGE EDWINSON.

[93]—WATER OF AYR STONE.—This stone may be cut with a disc, or a strip of sheet iron with sand and water, or it may be cut with a fine saw, as slate is cut.—GEORGE EDWINSON.

[119]—ELECTRO PLATING.—The six ounces of cyanide of copper can be worked out of the plating solution in the course of a few days by using a large anode of pure silver. Of course, the first deposits of silver will be inferior in colour, because alloyed; but I know of no other practical method. Separation by the chemical process would not pay for such a small quantity of solution.—GEORGE EDWINSON.

[152]—NICKEL PLATING.—To economise space here, I may mention that two good practical articles were given on this subject in the *Mechanical World* for Sept. 17 and Oct. 1, 1881.—GEORGE EDWINSON.

[228]—MICROPHONE.—Plates for this purpose, about 3 in. in diameter, the volume of the current increasing with size of plates. Any number until the required tension has been obtained.—GEORGE EDWINSON.

[229]—HAIR.—See, for accounts of almost instantaneous conversion of coloured hair to white, vol. i. pp. 198-99 of Hinton's "Physiology for Practical Use."—E. D. G.

[238]—ELECTRIC.—Inductive electric force is displayed in the space enclosed between the wires of a galvanic circuit, and also in the close vicinity of such wires. The "co-efficient of induction" would be the sum total displayed by two inductors, the amount of induction by one being known.—GEORGE EDWINSON.

[241]—FAURE ACCUMULATOR.—(1.) All the tongues of the lead plates destined to form the positive element of the cell must be joined together. (2.) All the tongues of the opposite plates must be joined together, and "look" the other way. (3.) In series, one after the other, or side by side, as may be required. (4.) When all the oxide of lead has been converted into the peroxide of lead by the action of the charging current, the cell is fully charged, but the "quantity of electricity it will hold" varies with the age, &c., of the cell, for it improves with age. (5.) Open, if preferred, or protected from dust by a cover of wood. (6.) Yes. A cell might be charged with this force, or even less. (7.) No. (8.) The force of the battery does not depend upon the force of the charging battery itself, but upon the amount of chemical energy developed in the cells by the charge of electricity. Perhaps the discharging current does not realise more than 80 per cent. of the force expended in charging the battery.—GEORGE EDWINSON.

[242]—PROSE COMPOSITION.—The theory of English prose composition is admirably treated in Bain's "English Composition and Rhetoric," 1 vol. (Longmans): while original and useful practical methods are suggested in W. S. Dalgleish's "Introductory Textbook of English Composition," 1s.; or bound with the "Advanced Textbook," 2s. 6d. (Oliver & Boyd).—E. D. G.

[244]—LECTURES.—The "Working Men's Educational Union," some years ago, published an *Illustrated Reporter*, containing lists of subjects for lectures, and of diagrams published in connection with those subjects. The list before me comprises sets of diagrams in astronomy, geology, volcanoes, the steam-engine, the telescope, the microscope, the mechanical powers, and twenty-nine other subjects. These diagrams are now published, I believe, by the Religious Tract Society. Those which I have used were rough, but good.—E. D. G.

[247]—WARMTH AT NIGHT.—It is certainly conducive to health to maintain the body at a comfortable temperature at night; but have a care in the choice of stoves to keep the room warm. Unless the products of combustion, produced by a gas-stove, are carried out of the apartment by a flue, they will counteract all the beneficial effects of increased warmth.—GEORGE EDWINSON.

[263]—VEGETABLE FOOD. If Provost P., or anyone else who desires any information about vegetarianism will write to Mr. Doremus, 30, Rochester-road, London, N.W., he will get information how to begin. A few stamps enclosed will further procure him some pamphlets.—T. R. ALLINSON, L.R.C.P.

[270]—BLOWPIPE CHEMISTRY.—"Amateur" seems to have puzzled himself sorely over Colonel Ross's very clear instructions for constructing his blowpipe. The piece of brass is to be soldered to the large end of the 12-in. telescopic tube, the mouthpiece being placed on the opposite end. "Amateur" will now see that there is no exit for the air blown into the tube through the mouthpiece. The jet (of which a drawing was given by Colonel Ross) is let into the telescopic tube at right-angles to it, and about half-an-inch from the stoppered end, fitting close on lines A and B of drawing. The hole in the jet, between lines A and B, now forms the exit for the air blown into the tube. The balloon is secured to the larger end of the jet, and acts as an air-chamber or reservoir, and, owing to its elasticity, a bellows. The brass nozzle is fitted to the small end of the jet, and thus forms the only outlet for the air blown into the tube. I hope this crude explanation will help "Amateur" over his difficulty. He seems to have mistaken the *jet*, of which a drawing was given, for the *tube*, of which only the dimensions were given.—PYROLOGIST.

[273]—STRENGTH OF MATERIAL.—There appear to me to be several errors in the solution given by "Anderson." First, he uses two different values for the distance between the guide-rail and the centre pivot—namely, 9·17 ft. and 8 ft.; and, again, he resolves an apparently vertical force into vertical and horizontal components. Taking 8 ft. for the distance between the guide-rail and centre pivot, and 25 ft. for the distance between the guide-rail and the direction of the weight, the solution is as follows:—

$$\frac{30 \times 25}{8} = 93\tfrac{3}{4} = \text{tension in tons on the centre pivot,}$$

$$\frac{30 \times 33}{8} = 123\tfrac{3}{4} = \text{pressure in tons on the guide rail,}$$

$$123\tfrac{3}{4} - 93\tfrac{3}{4} = 30 = \text{weight in tons on the crane.}$$—F. M.

[274]—FORAMINIFERA IN CHALK.—Brush out with a hard brush, well wash in water, and pick out with a needle, and mount in balsam.—ALBERT SMITH.

[274]—MISCELLANEOUS.—(3.) The following drawing-books may be recommended to "Eozoon":—1. "Vere Foster's," Marcus Ward & Co.; 2. Cassell's series of "Popular Drawing-Books"; and 3.

those, published by Blackie & Sons, under the direction of E. J. Poynter, R.A.—Robert Macpherson.

274.—Miscellaneous.—The treatment of chalk foraminifera differs somewhat from that of foraminiferous sand. A very satisfactory method is to boil a small piece of chalk with caustic potash in a test tube until it separates in fine powder. This is shaken up in a large bottle full of water, the foraminifera are then separated by specific gravity, the water being poured off after standing a short time, and a fresh supply added as long as it comes away of a milky tint. The deposit will consist chiefly of foraminifera, and may be mounted in balsam. I have prepared several slides of chalk from the North Downs, in Kent, by this method, which I have always found to answer perfectly well.—C. Harris.

275.—Salt.—The objection to the use of salt can only be accounted for by some peculiarity of taste. It is certainly not shared, as far as I know, by any of our "high medical authorities." By the use of salt the salivary glands, the secretion from which aids in the digestion of food, are stimulated. Its use is, of course, not absolutely necessary; but that it is the most natural of all condiments, and therefore the best, is made evident by the fact that saline matters are contained in all kinds of natural food.—Robert Macpherson.

[275.]—Salt.—Many object to salt on the idea that it is an acquired taste; they say salt is not a natural part of man's food, and so we ought not to take it. They instance children, who always splutter out salt food when given them. It creates a false appetite, and a craving for food even when the stomach is full. Also giving rise to thirst, or a desire to drink. It increases the flow of gastric juice and of saliva, for a time, the same as any other bitter substance will. Some object to it on the ground of it being a mineral, and say that our food contains all that is needed without it. I know many vegetists who never take it from year to year, who cook everything without it, and who even have it not in their houses.—T. R. Allinson, L.R.C.P.

[277]—Luminous Paint.—This consists of calcium sulphide, ground in oil. The light given by it is bluish. A 6-inch square surface will show time by watch. Of course, the paint must have been recently exposed to light.—Lewis Arundel.

277.—Luminous Paint.—This is sulphite of calcium, made by burning oyster shells in a closed vessel with sulphur.—Albert Smith.

[278.]—Smell from Burning Gas.—The gas is imperfectly consumed. The globes sometimes cause a rush of air, through the hole being too small, and then the gas is not consumed.—A. Smith.

[282.]—Smelling Salts.—The use of smelling salts is only beneficial in cases of fainting and nervous depression, and at times in neuralgic headache. Women use them more frequently than men for the same reason that men use tobacco more frequently than women, the force of custom in both cases being the ruling power.—Robert Macpherson.

[284.]—Pencil-point Protectors.—You can re-silver, using cyanide of silver; it is very poisonous.—A. Smith.

[285.]—Scientific Terms.—Collins & Sons publish an illustrated Dictionary of Scientific Terms, by W. Rossiter (price 3s. 6d.): very useful and compact. Anything not in Rossiter is easily found by taking the Roots and consulting a Latin or Greek dictionary, as the case may be.—G. B.

286.—Electricity.—Get Sprague's "Electricity," published by Spon, Charing Cross.—A. Smith.

297.—Tobacco and Science.—Tyndall has shown that the blue of the sky and the blue of the sea are caused by the breaking up of the rays of light by infinitely small particles of solid matter. The blue smoke rising from the glowing end of a cigar or pipe contains very minute particles of carbon at a high temperature. After the smoke has been drawn into the mouth and expired, two changes have taken place: the smoke is at a lower temperature and laden with moisture. It is also heavier. Perhaps there has been some chemical change in addition. At any rate, we may safely say the atoms of carbon have united into larger particles—just as coal-smoke particles form smuts—and reflect light in a different manner. The smoke issuing from the paper tube of a cigarette contains the two smokes: a small quantity of smoke at a high temperature drawn back from the glowing end, and a larger quantity of cold smoke that has been in the smoker's mouth, which has been rendered darker and heavier.—W. D. B.

Photographic Bromide Solution.—Ammonia bromide, 60 grains; liquid ammonia, 1 drachms; dis. water, 12 drachms.—A. Smith.

Letter 300.]—Duration of Life.—S. wants to know what I mean by stating that it depends on ourselves whether we die at 35 or at 70. It is thus. "All diseases are due to three conditions"—1st, my parent's fault; 2nd, my neighbour's fault; 3rd, my own fault. 1st. If my parents transmit to me a tendency to gout, consumption, or insanity, then the fault of ill-health belongs to them. If a person dies before 20 it is the fault of his parents, after that it is his own fault. 2nd. If my neighbour's drain runs over, poisons my drinking water and I get typhoid fever, then he is to blame. If another one comes to my house whilst he is suffering from small-pox and gives it to me, then surely I am not to blame. 3rd. This is the largest class, and contains most diseases, for by care the tendency to gout, &c., can be rooted out from the system. Given many persons taking rich food and much of it, then bilious attacks are to be looked for. If the rich food be continued for a long time, then expect gall stones and stones in the bladder. Give alcohol, and we get chronic indigestion, rheumatism, &c. Give rich food and alcohol, and we get gout, apoplexy, heart, kidney, and liver disease. Give tobacco, expect sore throat, palpitation, slowing of heart's action, and in some few cancer of the lip. If fresh air be neglected, then expect colds on the least exposure. If exercise be not taken, we expect constipation, piles, congestion of liver, &c. Now add up the action of lots of meat, plenty of drink and tobacco, and we shall get our people dying at 35 years. Take all in moderation, and an average constitution will last till nearly 70. But by abstaining from beer and tobacco and being spare with meat, by taking plenty of exercise and fresh air and keeping the mind calm, we may go to fourscore or more.—T. R. Allinson, L.R.C.P.

Answers to Correspondents.

*** *All communications for the Editor requiring early attention should reach the Office on or before the Saturday preceding the current issue of* Knowledge, *the increasing circulation of which compels us to go to press early in the week.*

Hints to Correspondents.—1. *No questions asking for scientific information can be answered through the post.* 2. *Letters sent to the Editor for correspondents cannot be forwarded; nor can the names or addresses of correspondents be given in answer to private inquiries.* 3. *No queries or replies savouring of the nature of advertisements can be inserted.* 4. *Letters, queries, and replies are inserted, unless contrary to Rule 3, free of charge.* 5. *Correspondents should write on one side only of the paper, and put drawings on a separate leaf.* 6. *Each letter, query, or reply should have a title, and in replying to letters or queries, reference should be made to the number of letter or query, the page on which it appears, and its title.*

A Reader and Subscriber to Knowledge. The subject is fully considered in treatises on physiology.—Mathematicus. Mathematical course at Dublin is excellent.—J. M. You are not content with 2½d. for 2d., but insist on 3d. or 2¾d. at the least. The paper is excellent.—H. A. Bulley. You evidently misapprehend what science says on the subject. You carefully make a straw giant, and as carefully upset him.—Tyro. As to your first, ask us an easier one. As to the other, to find how much a ton at earth's surface is attracted to the moon, multiply a ton by the square of 3,960, divide by square of 238,818, then by 81, and you will get the mass whose downward pressure at earth's surface is equal to attraction of a ton of matter (on earth) moonwards.—Nicholas Morgan. Pardon me, the phrenologist contends for something more than that difference of cranial form indicates a like difference of disposition, or of talent, or of form. *So much*, many admit who are not phrenologists. The Mallicolese skull compression cannot, however, afford much ground for argument either way.—Teeld, or Jecla, or Yeela (??). Have never heard of Dr. Long's "Astronomy." For a beginner, should say Guillemin's "The Heavens" (Bentley) would suit.—A. N. That frog cure is really too absurd, also too cruel.—Celt wants his theory discussed, that the builders of the Great Pyramid heaped up earth all round the Pyramid so as to form a slanting surface to the Pyramid level, as it rose layer by layer, and afterwards removed all that earth. He thinks—but does not say why—that this would explain the slant passages. He asks if any one can offer a better theory. "In those days," he says, "engineering appliances were not likely to be able otherwise to raise such great blocks of stone, and labour was practically unlimited." It must have been, if the theory is correct.—Charles Horner. It would be simply absurd to tell our readers that the Great Gallery has been measured to within a hundredth of an inch. No one who knows anything about measurement would believe it of any gallery fifty yards or so long, still less of the Pyramid Gallery. You could make the length anything between 1,870 and 1,890 in. according to the way you chose to measure, and anything between 1,800 and 1,950, according to the length you choose to assign to the pyramid inch. These coincidences are mere tricks, though honestly meant enough, I have no doubt. The length of York Minster is quite as near the 1,000 millionth of the sun's mean distance as the Pyramid's height; so is the height of Rouen Cathedral; but no one has yet started any craze about the divine

inspiration of the architects of either cathedral.—W. A. C. It is admitted, by so staunch an opponent of cruelty to animals as Mr. R. H. Hutton, that if all men became vegetarians, multitudes of animals now used for food would of necessity perish for want of food. This being so, it is not fair to tell the creophagist that though "he can get all the chemical qualities of meat without taking animal life, he takes it in order to tickle his palate." Note also that F.R.A.S. only speaks for himself; he does not attack others; you go out of your way to charge him with cruelty. Observe, I am quite with Mrs. Dr. Kingsford and other vegetarians in all that they say about the coarse and disgusting scenes now associated with the supply of flesh meat. But I think the uncompromising vegetarian does more to prevent change than to hasten it. The world at large would be easily moved, I think, to see that our meat supply was obtained in a better fashion; but you do not aim at that you try to persuade the world to admit at once that animal food should be dispensed with; and the world replies in effect, If *that* is what you want, you are not likely to get it.—Taranaki. If you read Sir E. Beckett's book more carefully, you will find that where he writes, "Chance is only the uncalculated result of some known or unknown laws of nature" (these are his words, but your quotation is near enough), he means only what he expresses thus later on, "The only meaning of the word 'chance' in the physical universe, since it began to exist, is this sort of incalculable consequence either of the known or unknown laws of Nature." In other words, he is referring to the word "chance" as applied to natural processes. What I have spoken of as pure chance he speaks of as "bare chance" at p. 19; and, of course, like me, he not only "believes," but knows "there is such a thing." To use his own illustration, "every rational man concludes" that the atoms of the universe have been, "as we say, of dice, 'loaded,' to make them behave in a particular way, not a thousand or a million times, but always," . . . "because the only possible alternative is that of bare chance; and the idea of all the atoms of the universe behaving as they do by chance, is too absurd for any man in his senses deliberately to entertain."—Tangential Tendency. No; it cannot be said that because the sun's attraction acts in same direction at one part of the earth's orbit as the tangential tendency acts a quarter of a revolution later, the sun's attraction during one quarter causes the tangential tendency in the next. The direction of the earth's motion is changed, but the earth's velocity in the tangential direction is not generated by the sun's action.—A Reader of Knowledge. The difference is due to use of mean instead of solar time. We shall treat fully of this presently; it cannot readily be explained in few words. Name a good science manual? Would you mind mentioning on what subject?—Henry Bowman. We should be glad to give more chess, if we could; but others want less, and many want more mathematics.—Edward Hargreave. We should be glad to give more whist, if we could: but others want less, and several want more chess.—H. B. R., Charles Jerrold, M. James, and others. We should be glad to give more mathematics, but others want less, and many ask for more whist.—H. W. Fawcett. Two of my essays on the Pyramid are in my "Myths and Marvels of Astronomy," two in my latest work, "Familiar Science Studies," both published by Messrs. Chatto & Windus.—W. McManus. Your query would lead to endless replies. The opponents of evolution are many, their arguments numerous. Read, as one of the best examples, Sir E. Beckett's little book on the Origin of the Laws of Nature (S.P.C.K.), also Dawson's Story of the Earth and Man (Hodder & Stoughton) — a really charming work.—Simplex. I am very much obliged to you for so carefully, in response to my wish, showing some examples of Bell's line-writing; but although, from a scientific point of view, it may (I cannot honestly say I see why) be beyond comparison with anything preceding it, it seems to me to compare unfavourably with Pitman's in brevity. The double curves for *v, f, l, r, w* (English) seem serious objections from a stenographic point of view.—Paradox. He was kind enough to send me his confounding of Darwin.—John J. Prince. Considering that the subject of the great changes of climate which different parts of the earth appear to have undergone, occupies many hundreds of pages in divers treatises on geology, you should hardly expect us to put the matter as a query, inviting readers not only to incubrate thereon, and to give reasons for their opinion, but if possible to demonstrate it mathematically. The subject is one in which we may shortly offer an article by an acknowledged master of the subject, but for correspondence and replies,—not much: (for we should get too much). As to the other question (which you ask over name Tyro), whether the earth's diameter is being increased by layers formed out of its own substance, the question seems akin to this, When Pat took a strip a foot wide from the bottom of his blanket and added it to the top, how much did he increase the length thereof?—W. C. You are angry because we will not give up the fine saying by Liebig. Well, you make a mistake in this; and, as Liebig says, there is no harm in making a mistake. But before "putting us down," do consider the harm you propose to do us.—W. H. H. Soames. Thanks for your courteous letter. May I, for a last word, say that perhaps if those who have devoted much time to the account in question were at one in their interpretation, Science might do well to consider it more attentively than at present. What say you to this from Monsignor Clifford, respecting the account?—"C'est nullement l'histoire de la création, fait en sept jours ou sept périodes de temps, mais simplement la consécration, sous forme d'hymne sacré, des sept jours de la semaine à la mémoire, au souvenir, des sept œuvres principales de la création." Whereunto my excellent friend, M. l'Abbé Moigno, replies:—"Oserai-je exprimer le regret que Monsignor Clifford ne partage pas mes convictions de la vérité de la cosmogonie de Moïse, quoique mystérieuse encore dans quelques-uns de ses détails."—Historian. Queries answered, I think, in this section; history scarcely falls into our line. C. T. B. Surely the description of tobacco smoke as "a cloud of tar in half-burnt gas" is inexact.—J. H. Ward. *Errari*; you can either correct as you suggest, or for mean distance write "perihelion." A. T. C. Newton settles it, does he not? yet he says *Hypotheses non fingo*. Doubtless he came near the truth; but it was not a matter of observation, experiment, or mathematical demonstration. Wonder who told him. Ayrshire wants book on Ventriloquism, with dialogues.—Private Student. Such questions are not suitable; we can neither insert in Mathematical Column nor answer here. We should be flooded with such questions if we did.—C. C. C. You seem to think I keep all the questions and answers in my head. I cannot tell what theory you refer to unless you give page and column of my answer.—Ignorant. Any textbook of biology will tell you how fish breathe by means of their gills; the air in the water which passes through the gills being "what they breathe."—J. P. Sandlands. We have treated you with courtesy and consideration; you repay us by accusing us of unfairness and cowardice, because we decline to break through a rule which we consider absolutely essential to the maintenance of Knowledge in its proper position. We shall say no more.—Edgar Flower. George Stephenson, the engineer, was, of course, right in saying that the uppermost point of a wheel of a carriage moves twice as fast as the carriage, while the lowermost is for the moment at rest: does this require elucidation?—T. W. Johnson. If you are "thoroughly satisfied" it is a delusion, all is well so far as you are concerned.—J. McGrigor Allan. We are quite with you, but have not room for the subject, which does not belong to our programme.—A. Daniels. We cannot find time or space to work "sums."—Jno. Trist. We regret that the necessities of space will not permit us to find room for "a few essays on the doctrine of philosophical necessity."—H. H. L. Hill notes that the collection of the late Professor Tennant are still on sale at a shop a few doors west of King's College, and will shortly be sold by auction. Suggestion noted as to centre of pressure, &c.—C. Harris. The attraction of the mouthpiece is virtually *nil*. As to the colour, there can be little doubt you are right, and that the smoke looks blue only when seen by reflected, brown only when seen by transmitted light.—Professor Buchheim. Your letter appears.—Jemima. Scarcely space at present for the refining of sugar.—E. F. Scarcely a reason,—rather How, than Why.—John Sparks writes "i" for "I," except when he forgets his part. He may note with advantage that the word "science" would not be spelled "cience" by one who wrote "i" for "I."—F. Blaker. The law of diffusion of gases would not affect, to any appreciable degree, the *stagnation* of the air in upper part of room, and would only very slowly cause the carbonic acid gas continually poured into that part of the room (by persons breathing) to be diffused; in fact, it would act much more slowly to diminish the carbonic acid gas than the breathing even of one person only would tend to increase it.—Clare. You are rather hard on Mr. Abbott; we do not think he wants to study a treatise of either class, but simply to know what is commonly understood by the expression "abstract reasoning." The reply about tobacco smoke assumes more than can very easily be proved. Iron certainly does rust faster in salt water.—W. Wilson. I agree with you that there are *cases* where ordinary modes of expression are misleading, "A tog is a tog, but the question is, is *this* tog a tog?" for which you'll overhaul your "Snarley Yow," and when found make a note of.—E. D. G. There are certainly cases in which the differential equations admit of more than one solution; but it has not been shown that such cases can occur in nature. Till this has been shown M. Paul Janet's inference is but a rather fanciful guess. The question is too difficult and artificial to much interest the bulk of our readers.—B. Riley. Question already answered satisfactorily.—L. D. S. You should get an elementary book on astronomy.—E. W. C. The one with larger capital, whether A or B, has the best chance of winning in the long run. See my essay on a Gambling Superstition.—Erin. As you think the outside car is the best ever invented, you ought

n[illegible] Moon's phase can scarcely [illegible] as they are explained in every respectable text-book on astronomy.—H. D. Barclay. A man may be said to be evolved from a baby, or an oak from an acorn, but in the science of our time the term evolution is generally applied to the development of species.—A Schoolboy. The reason is that the earth goes round the sun once a year, so that the stars on the side opposite the sun necessarily change. Imagine a long, straight line from the sun to the earth and beyond. This would point in one direction at the beginning of a month, in a changed direction (because of the earth's motion) at the end.—W. B. Grove. Pardon me, it is you who are wrong in denying that to give an equal chance of a particular event, such as all the trumps being in one hand, there must be a certain number of trials. You are right in saying one particular arrangement is as likely as any other, but it is very far, indeed, from an even chance that that particular arrangement will appear. You shall correct the mathematical discussion of this point, if you like and can, when it appears.—Archimillion. Your "cosmetical specimens of angularised forms" too much for us: we need rest after your letter.—W. W. There is nothing to make the earth assume a more upright position. The removal or burning of all the coals in the globe could not affect her axial pose, even if they were all at one spot in the northern hemisphere.—S. S. H. By triangle of forces, the actual wind FE (Fig. 2, p. 36), and the wind EC, resulting from yacht's motion in direction CE, would be balanced by a wind CF, and, therefore, their resultant is a wind FC. 2. Well, you might, for such a problem as you require, take this. Assuming frictional resistance constant, and sail GC always set to bisect the angle FCE (as this angle varies with the increasing velocity of the yacht), determine the maximum velocity which the yacht can attain on a given course and with a given wind. Do not ask me to solve this, please, for time will not permit.—A. H. Moseley. We partly think with you that the stories on "Intelligence in Animals" have now run far enough; but we have to consider, what perhaps you overlook, that many readers take much more interest in such matters than in those with which you wish to see Knowledge filled. Your friend who told you that the very amusing and well-written sketch you object to would have been sent to the W.P.B., but for the military rank of the writer, is no doubt one who understands very thoroughly his own nature, and judges others accordingly. Those who know me best tell me I err in the other direction. Need I say I do not agree with them? but at any rate, they know me; your friend very manifestly does not. You say "give us more astronomy, instead" of such articles; but for one who so addresses me there are tens, or rather fifties, who say give us more that is light and readable. Of course, if all readers wanted deep scientific matter, I should feel bound to consider them; but I expect that would mean such a limited array of readers that Knowledge would very soon come to an end. This point has to be considered, you know. Oddly enough, you are the first who has written objecting to the introduction of these stories (except one who gave a semi-religious reason). Now the round is complete, and *every* subject dealt with in Knowledge has had its turn. If all were listened to, therefore, we should appear with blank pages. Yet all who have criticised special subjects have done their best to advance our success. Many thanks for what you have done in that way.—Geo. Robson. We are thinking of having "vellum wove," with illuminated borders and headings. Your own special copy shall be printed in gold letters on purple-tinted silk, delicately scented. Yet we fear you would not even then be happy. Will it cheer you to be told that the average cost of the numbers since January 1, instead of being less, as you fondly imagine, has been half as great again as that of the first nine numbers? In this, of course, is included one very important item, which persons who, like yourselves, complain of paper, folding, and so forth, persistently overlook.—C. O. Thanks for your very pleasant letter; it does me good after G. R.'s sour one. You have not the problem at p. 380 quite right; you take a square *plus* a square, and the figure is a square *minus* a square.—Gnats. Very likely you are right in assuming that thin-skinned persons are preferred by biting insects to those with less penetrable cuticles.—Lewis Arundel. Questions already answered, or in course of being so. The mistake in letter 289 is annoying; it is one of those cases of perverted ingenuity which are apt to escape the "reader," for his attention is directed to orthography, punctuation, syntax, &c., so that the absurdity of a statement, as such, is apt to escape his notice.—Percy B. Do[illegible]. Thanks; but the problem has been already dealt with in Knowledge, No. 8, p. 166.—Edward Stubbs. I have carefully compared the weather records for the years 1768 to 1792, left by White of Selborne, and find not the slightest balance (even) of evidence in favour of the theory that the weather repeats itself after seventy-six years, in England, at any rate.—F. Cowley. Thanks. About photography, Mr. Brothers, of Manchester, has [illegible] promised to write for us. The other questions already answered.—Hallyards. I did not mean that I could just see the *lumière cendrée* in America a day before first quarter, but that it was then strikingly obvious—as obvious as in England when moon shows but a narrow crescent. When you saw objects more clearly through moist air, they must, you think, have been magnified; but were they? Surely you could tell whether objects looked larger or not. It is absolutely impossible that moisture in the air should magnify. I am quite with you about differences of eyesight. I can see details with my naked eye (literally eye, for it is only one which has the power) which some find difficult to see with an ordinary magnifying glass. But personal differences such as that have no bearing on the question whether moist air magnifies. You speak of a change of tone in my communications to the E. M. after 1872; possibly you refer to letters in which I exposed the jobbing schemers. Truly, then I did *not* mean "legitimate playfulness." I laid on the lash with intent, and with tolerable effect. Looking back now at that time, I see nothing to regret except occasional leniency. Sorry it jars on you to hear our writers speak of "Huxley" and "Darwin" and so forth. It would jar very much on me to speak of Mr. Darwin or Mr. Herbert Spencer. Will ask the publishers whether the quadrifid ornaments on either side of the heading of each page ought to hurt readers' eyes. Are you not rather sensitive? The large maps can be bound up as the double-page pictures in the *Illustrated* and *Graphic* are bound. As to the words which seem to imply that the Egyptians were acquainted with the earth's motion, surely there is some difference between them and references to the rising, setting, and motion generally of the sun and moon. These bodies seem to move—the earth does not. *Does* an open fireplace help to ventilate a sick room? I know something on that score from experience, and I venture to say that if there is a place where our open fires are greater nuisances than elsewhere, the sick room is the place.—Jno. Sutherland. We cannot undertake the office of private tutors to individual readers. Nine-tenths of the initials, &c., under head "Letters Received" belong to correspondents who ask such question as yours. If we had space, we should not have time. Again, if you must ask questions involving the use of the differential calculus, why use the fluxional notation? To use Babbage's old joke, we prefer d-ism to dot-age. Lastly, to your question: (we answer just this once) you could hardly expect to get the right differential equation if you treat Y as a constant, even though it be only once. You have $\frac{d^2y}{dt^2}=Y$, and you say $\therefore\left(\frac{dy}{dt}\right)^2=2yY+C$; but Y is not constant; indeed you write down eventually to an equation in which there is a term involving $\frac{dY}{dx}$. "That's how the error has arisen."—R. C. Fraser. We must wait till the microscope has shown the diamonds in the razor's edge before we take that for a reason.—A. Blake. Other suns and systems might, of course, have originated as our sun and his system have done. We are not nearly so clear how this has been as you seem to imagine. The "sweet influences" of the Pleiades have been explained in several ways; there can be very little doubt the reference is to the supposed influences of the Pleiades when the sun was in their neighbourhood; just as Sirius was supposed to combine his rays with the solar rays, later on in the year, to produce unpleasant effects, so in early spring (when in Job's time the sun was passing the Pleiades) the germinal influences of the season were attributed to those stars.—J. M. Fothergill. Of course, Cotton Mather was an American; I thought everyone knew that. The story is told by Wendell Holmes, in his Breakfast series. I referred only to the "irreverent." It struck me as good fun to hear that strict, solemn, and most rigid Calvinist charged with irreverence—enough to make him turn in his grave; albeit one may note a good deal that is very irreverent indeed, in my opinion, in his "Remarkable Providences."

Letters Received.

A. Hargreave, J. J. Snellgrove, M. M., Neminis Umbra (we cannot see it), Willow, M. Paterson, J. K. L., M. Petersen, Blue Peter, An Admirer, Constant and Thorough Reader, Lucasian, P. Parley, Titmouse, Aggrieved One, Patience (just what Aggrieved One wants), L. Prowse, Ternary, S. Smithson, B. J. Harvey, Amused, Vegetarian, Prospero, M. Macnulty, Jerry, F. T. Heffernan, Harpax, Morose, Mother Shipton.

Notes on Art and Science.

HAIR TURNING WHITE.—A person I know [illegible] old friend of mine) met with a railway accident [illegible] hair perfectly white. I do not know if instantaneously [illegible]. He lived to be an old man, and after death his hair [illegible] returned, to its original colour, dark brown.—JOHN A[illegible].

EXPERIMENTS have repeatedly been made with the [illegible] of producing natural imitations of the craters and [illegible] on the moon's surface, and it has been found that the figures of the lunar inequalities can be closely imitated by throwing pebbles upon the surface of some smooth plastic mass, such as mud or mortar. Mr. Meydenbauer, of Marburg, uses a basis of dextrine for the purpose, and drops small quantities of the same material from a moderate height upon that basis. A photograph of various figures which are thus produced, shows a remarkable resemblance to the various inequalities visible on the moon's surface.—*A. M.*, in the *Academy*.

SOOT *v.* POLLEN.—I have often noticed that although the hazel will grow and flower freely in the suburbs of London yet it will rarely produce fruit. To gather nuts you must go some miles into the country. A few days ago, I was examining the catkins and female flowers of the hazel (gathered about four miles from the city) under the microscope, and I was struck by the fact that the pistils were each severally coated with a deposit of soot, sufficiently thick to prevent any chance of fertilisation. The hazel being anemophilous, the absence of nuts in the neighbourhood of London (and, I presume, of other large towns) is thus, I think, sufficiently accounted for.—WILLIAM H. ALLEN.

MICROCOCCI IN MUMPS.—The *Gazette Médicale* says that MM. Cabitan and Charrin, at a recent meeting of the Biological Society of Paris, gave an account of the investigations which they have for some time been engaged in, on the presence of minute organisms in the blood of persons suffering from mumps. These are multipliable by cultivation in Liebig's broth, and are found to consist of minute *bâtonnets*, but chiefly of micrococci, all in a state of motion. These minute organisms, they consider, corroborate the clinical observations which tend to place mumps among the infectious diseases. The absolute proof that this disease is due to these minute existences, by reproducing it by inoculation of the "cultures," has not been attained by the experiments made to that end.

ENGLISH AS THE SPEECH OF THE FUTURE.—The success of the English-speaking peoples as colonists, and their superior prolificness, are not the only reasons for thinking that the English tongue is destined to dominate the world. The flexibility and terseness of the English language has made it the language of international telegraphy, and from statistics just collected it appears to be the great newspaper language. In other words, it about equally divides the newspapers of the world with all other tongues combined. The total number of newspapers and periodicals now published is given in H. P. Hubbard's forthcoming "Newspaper and Bank Directory of the World," as 34,274, with a circulation of about 116,000,000 copies, the annual aggregate circulation reaching, in round numbers, 10,000,000,000 copies. Europe leads with 19,557, and North America follows with 12,400, the two together making over nine-tenths of all the publications in existence. Asia has 775; South America, 699; Australasia, 661; and Africa, 132. Of all these, 16,500 are printed in the English language, 7,800 in German, 3,850 in French, and over 1,600 in Spanish. There are 1,020 daily newspapers, 18,274 tri-weeklies and weeklies, and 8,598 issued less frequently. It appears that while the annual aggregate circulation of publications in the United States is 2,600,000,000, that of Great Britain and Ireland is 2,260,000,000.—*Scientific American*.

THE SOUND OR SWIM BLADDER OF FISH.—Perhaps the following quotation from the "Icelandic-English Dictionary," by Cleasby and Vigfusson will be sufficient to satisfy your readers as to the derivation of the word "sound" or "sounds" as expressive of the air or swim-bladder of certain fishes. The word is spelt "sund." "Sund (*q.s.* svund), [from 'svimma,' dropping the *v* and changing *m* into *n*]: 'a swimming';" &c. Some compound words are given as "sund-fjoðr" (*f.* "a swimming feather"); "sund-fœrr" (*adj.* "a good swimmer"); "sund-hreifi" ("a swimming pair"), of a seal with several others. It is said that "swimming was a favourite sport, the antagonists trying to duck one another," and that "sund" is one of the sports in King Harold's verses. It is also added that the word "sund," as used to denote a "sound" "or straight, narrow passage," is quite a different word from the preceding, being derived from "sundr," *i.e.*, "that which sunders." So that the proposed connection of fish "sounds" with words (having a somewhat similar appearance) in the Sanskrit, Assyrian, Chinese, Egyptian languages, &c., denoting "blood," "heart," &c., has no real existence whatever. The Scandinavians were doubtless aware of the part which "the sound" plays as an aid to the fish's powers of swimming, and never attributed to it the [illegible] of [illegible]. They knew better.—W. HOUGHTON.

ANTHRAX VACCINATION.—M. Pasteur's, or the "vaccination," for "anthrax," has also been shown by repeated experiments to be absolutely protective. Professor Greenfield has vaccinated cattle from rodents (gnawing animals like rats, squirrels, &c.) with the "anthrax disease," and has found that they remain free from all disorder, local or constitutional. The same result has attended M. Toussaint's experiments with the bacilli "cultivated" in special fluids, not in the living body of any creature; sheep and dogs inoculated with this cultivated poison showing no form of the deadly "anthrax" disease. The experiment was conducted on a large scale under the auspices of the provincial agricultural societies of France. A flock of fifty sheep was placed at M. Pasteur's disposal. Of these he vaccinated twenty-five with the cultivated "anthrax" poison on May 3, 1881, repeating the operation a fortnight later. All the animals thus treated passed through a slight illness, but at the end of the month were as well as their fellows, the twenty-five which had not been vaccinated. On May 31, all the fifty were inoculated with the strongest anthrax poison. "M. Pasteur predicted that on the following day the twenty-five which were inoculated for the first time would all be dead, whilst those protected by previous 'vaccination' with the mild virus would be perfectly free from even mild indisposition. A large assemblage of agricultural authorities, cavalry officers, and veterinary surgeons met on the field the next afternoon to learn the result. At two o'clock twenty-three of the unprotected sheep were dead; the twenty-fourth died an hour later, and the twenty-five at four. But the twenty-five 'vaccinated' sheep were all in perfectly good condition; one of them, which had been designedly inoculated with an extra dose of the poison, having been slightly indisposed for a few hours, but having then recovered."—R. A. P., in the CORNHILL MAGAZINE.

MR. RUSKIN ON EDUCATION.—Mr. E. J. Baillie, of the Ruskin Society, is contributing a series of articles on Mr Ruskin and his Teachings to House and Home. In the article on "Education," Mr. Baillie says:—"In one of his books Mr Ruskin has spoken pointedly upon the prominence and precedence almost invariably given to what may be termed caste, or class distinction. He has told us that there is a widely expressed desire for 'an education which shall keep a good coat on the back; which shall enable a son to ring with confidence the visitors' bell at double-belled doors; which shall result ultimately in the establishment of a double-belled door to his own house; in a word, which shall lead to advancement in life; *this* we pray for on bent knees; this is all we pray for. . . . It never seems to occur to the parents that there may be an education which in itself is advancement in life. That any other than that may perhaps be advancement in death. . . . To many "advancement in life" means . . . in a word, the gratification of our thirst for applause. That thirst, if the last infirmity of noble minds, is also the first infirmity of weak ones, and, on the whole, the strongest impulsive influence of average humanity; the greatest efforts of the race have always been traceable to the love of praise, as its greatest catastrophes to the love of pleasure.' In the scheme of national education, Mr. Ruskin realises the necessity for national Government schools. He maintains 'there should be training-schools for youth established, at Government cost, and under Government discipline, over the whole country; that every child born in the country should, at the parents' wish, be permitted (and, in certain cases, be under penalty required) to pass through them; and that, in these schools, the child should (with other minor pieces of knowledge hereafter to be considered) imperatively be taught, with the best skill of teaching that the country could produce, the following three things:—(*a*) The laws of health, and the exercises enjoined by them; (*b*) Habits of gentleness and justice; and (*c*) The calling by which he is to live.'"

Our Mathematical Column.

THE LAWS OF PROBABILITY.

BY THE EDITOR.

THE general law enunciated in our last number may be regarded as the fundamental law of probabilities. Nearly all problems in probabilities, direct or inverse, depend on this law, to which the more complex cases are reduced by various devices of greater or less simplicity according to the nature of the problem. And again, the value of any chance not relating to tickets in a lottery, or balls in a bag, may readily be

referred to these easily-understood illustrations. Thus (to take in passing a very familiar case), suppose we are told that the betting is four to one against a certain horse in a race, what are we to infer as to the value of the chance which betting experts assign to him. The odds of four to one mean, of course, that there are four chances against the horse for one in his favour, or that out of five equal chances he has one. His chance of winning is, therefore, so far as *the betting shows*, equal to that of drawing one particular ticket from a bag containing five, all equally likely, antecedently, to be drawn. I need hardly say that the real chance of the horse may be very different. The betting experts may not know nearly so much as they suppose, and the horse may have a much better or much worse chance than they imagine. Or again, they may know (some of them) much more than they pretend to know. But so far as the betting shows, that is the horse's chance. We shall see further on how this simplification of the problem enables us to determine from the known betting about two horses singly, the proper betting about the pair, and similarly for three or more horses.

Again, take cases which at first sight do not seem to resemble the problem we have just considered—as, for instance, the tossing of a coin or the casting of a die more than once—and let us see how these can be reduced to the simpler case. Here is a simple problem of this kind:

What is the chance that, when a coin is tossed twice, the tossings will be unlike—that is, that both will not be heads nor both tails? Simple as this problem is, by the way, the great mathematician d'Alembert went astray in dealing with it (at a time when the mathematics of probabilities were not very well understood). He reduced it to our general law in summary but inexact fashion. Thus he said, There are three possible events: either both tossings will be heads, or both tails, or they will be unlike: therefore the chance that they will be unlike is one in three, or one-third. This result d'Alembert maintained with a degree of confidence which seems singular when the simple nature of his mistake is considered. To solve the problem correctly, we must proceed, as he did, to consider the various possible events; but not err as he did, by failing to notice that he counted two of them as one. The possible events are four, viz., head, head; tail, tail; head, tail; and tail, head. There are manifestly no others, and as manifestly any one of these is as likely as any other. Now of these four cases, two give unlike tossings, viz., the two last. The real chance, therefore, is not one in three, but two in four, or one half.

If d'Alembert had been a betting man, and had backed his opinion by wagering two to one against the occurrence of unlike headings, during a great number of trials, he would have lost heavily—the real wager being even. In the long run, half the trials would be of the kind against which he had wagered; and losing thus as often as he won, while paying twice as much when he lost as he received when he won, he would manifestly lose just as much as his opponent had wagered. At least, the result would approach this (and his loss, therefore, be the greater) the more numerous the trials.

We note, then, in passing, that men who are unwise enough to gamble ought to have a clear idea of the laws of chance; for in the long run these are as inexorable as the laws of the Medes and Persians. A man may buy a single ticket in a lottery, and he may chance to win, or he may try his luck pretty often at *roulette* or *rouge et noir*, and still be a winner; but if he keeps on long enough, he will inevitably lose in proportion to the calculated chances against him. (He may lose at the outset, and probably, on the whole, it is the better for him that he should.)

There are, indeed, ways in which men suppose that with patience they can always win, though slowly. The following is a familiar illustration, which we leave as an exercise for the reader:—

Two gamblers, A and B, toss a coin on the following terms. A wagers against B evenly £1; if B wins A wagers £2 even with him; if B wins again A wagers £4 even with him; and so on, doubling each time, till A wins the toss. When this happens, whatever the number of tossings before the event, A wins £1. They begin again. A wagering £1 even, as before, and doubling till he wins, when he pockets another £1. Every wager is strictly fair, yet every trial (as A must, at last, win) ends by A winning £1. The system seems altogether unfair in its results, though perfectly fair in details. Is it so or not? Assume A and B to have equal capitals, say £1,000, and estimate their chances of success or failure. At first sight it seems as though A must gradually win every pound B has. In reality it is not so, as we shall show later.

"All Roots." (See "Answers to Correspondents," p. 391, line 17.) In reply to "All Roots'" query, I give an easy method of extracting the fifth root of 5153632, and will endeavour to state the rule as briefly as possible. Make as many columns as the number of the root to be extracted; then, having found the first quotient figure, put it in the first column; add the same number to it; then multiply the sum obtained, placing the product in the second column. Continue this course, viz.: add and multiply, add and multiply, decreasing one column each time, till you have only one to make in the first column, which will be the same number of additions as the root to be extracted. Then add 0 to the first column, 00 to the second, 000 to the third, 0000 to the fourth, and bring down the next period in the fifth. Then find the probable number of times that the fourth column will be contained in the fifth; put this in the quotient, and also under the first column, and add and multiply as before. This method was discovered by the late Mr. Horner, M.P., and is applicable to all roots. Your correspondent will find all particulars in "Kavanagh's Arithmetic."

Romeo.

Find the fifth root of 5153632.

				5153632 (22
2	4	8	16	32
2	8	24	64	1953632
4 × 2	12 × 2	32 × 2	80000	
2	12	48	176816	
6 × 2	24 × 2	80000	976816 × 2	1953632
2	16	8408		
8 × 2	1000	88408		
2	204			
100	1204			
2				
102 × 2				

Notice.—The necessities of space oblige us to defer solutions of problems till next week.—Ed.

Our Whist Column.

By "Five of Clubs."

THE PENULTIMATE.

Sir,—I send you a game for young players, illustrative of the above heading. Playing the penultimate is, leading the lowest but one in suits of more than four cards, which are not headed by the Ace, or do not contain two commanding honours or strong sequences, &c. This mode of play is, practically, the invention of Cavendish, and is the logical extension of the lead of the lowest but one, in suits containing intermediate sequences. I look upon the invention as little inferior, in its means of conveying information (and its consequent influence upon the game), to the convention which requires a player to return his lowest in suits of four cards, or his highest in a suit of three. In point of fact, in some cases it conveys more rapid information, for the lead from five cards may be, and often is, declared in the second round of the suit. And where the dealer, being happily possessed of five trumps, is forced before trumps are led, and trumps with the penultimate, the information of a minimum of five trumps is conveyed at once. I ought, perhaps, to state that, unless there are special reasons to the contrary, the lowest but one should still be led in the trump suit, notwithstanding that it might be headed by Ace, King, or Queen, with small cards. It is true that, occasionally, a weak lead simulates a lead from five or more; for a player, unknown to his partner, may have his strong suit attacked by the adversaries. In such cases, where the lead is not an original lead, a partner must draw his inferences with more caution.

Frederick H. Lewis.

The Hands.

A.
Clubs—4, 3, 2.
Hearts—K, Kn, 4, 3, 2.
Spades—Kn, 8, 3.
Diamonds—8, 6.

Y.
Clubs—7, 6, 5.
Hearts—10, 7.
Spades—10, 7, 6.
Diamonds—A, Q, Kn, 5, 3.

B

Y Z

Dealer.

A

Trump Card, Club King.

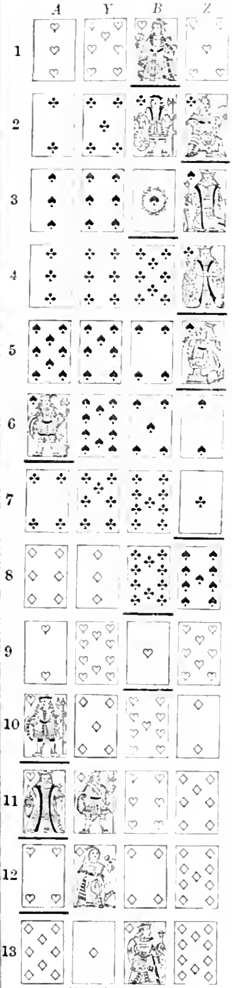
B.
Clubs—Kn, 10, 9, 8.
Hearts—A, Q, 9, 6.
Spades—A, 5, 4.
Diamonds—K, 4.

Z.
Clubs—A, K, Q.
Hearts—8, 5.
Spades—K, Q, 9, 2.
Diamonds—10, 9, 7, 2.

Score:—Three all.

THE GAME.

Note.—The underlined card wins trick, and card below it leads next.



REMARKS AND INFERENCES.

1. —*A* leads the lowest but one. *B* wins with the Queen and observes that the two does not fall in the trick. Either one of the players is calling for trumps, or *A* is leading from five or more. If from more than five, the suit will be trumped next round. Having a sequence, he leads the head of it.

2.—*B* knows now that the adversaries are two by honours. *Z*, having turned up the King, properly heads the trick with the Queen.

3.—*Z* plays his best suit. *B* winning the trick continues the trump lead. He knows that he must make two by cards to win the game. He properly leads the eight, to show his partner the strength of his sequence.

4.—*Z* continues his suit.

5.—*Z* plays another Spade in the hope of finding the Knave with *Y*.

6.—*A* knows now that the Ace will fall, and that *B* will, probably, remain with the last trump. Of course, there is a chance of *Z* having the seven.

7.—*Z* is now in a difficulty. He knows he can force the remaining trump, but he knows also that *B* has led trump upon the Heart suit. *Y* may, however, have an honour in Hearts, and it is better in any event, as far as the Diamonds are concerned, that *Y* should be fourth player. *Y* discards a Diamond to the 13th Spade, though not of much use now, as the only trump is forced from *B*. The rest of the game plays itself, but if the young player will play the game over, and suppose *B* to have led three rounds of Hearts before leading trump, he will find that *Y* and *Z* will make six tricks and win by their honours. If the two had been led instead of the three, *B*, although he might nevertheless have led trumps, would have had no means of judging that the entire suit was between him and *A*.

Errata.—It is, perhaps, hardly necessary to explain that in the very easy Double Dummy problem given last week, the partners A and B, not Z and Y, are to save and win the game. The correction was made in the proof sheets; but, in making a more important change ("Mogul's letter had been put last, instead of in its proper position), the compositor overlooked this one. A similar remark applies to the word "small," p. 416, 2nd col., l. 22. Clay abhorred "false" cards, not small ones. Whist players will recall Cavour's *mot;* when some one complained, as bad players always do, of constant ill-luck at whist, he replied, "C'est, mon ami, que vous n'avez pas assez de respect pour les petites cartes."—Five of Clubs.

All the Trumps in One Hand.—In *All the Year Round* for October, 1876, page 77, two instances are given of a player being dealt the entire trumps ["having dealt himself," it should be]. Very recently, I myself held all the trumps but two (five and six) in a hand at whist.—R. E. P.

A correspondent asks how to score at Short Whist, not finding Clay's account clear. If he would say what seems to him obscure, we will endeavour to explain. Five of Clubs.

J. Mackenzie regrets to see Knowledge *wasted*, advising how best to play Whist, "a game depending so much on mere chance." *Does* it depend on mere chance? Cavendish has settled that question pretty decisively. The element of chance is eliminated in the long run, and good play tells as certainly at Whist as it does at Chess. The way chance works is to make Whist a capital moral as well as mental exercise. The player who, having a bad hand, steadily does all he can while yet a chance remains, and perhaps retrieves a game which another might have thrown up as hopeless, has had a useful exercise and set a good example.—Five of Clubs.

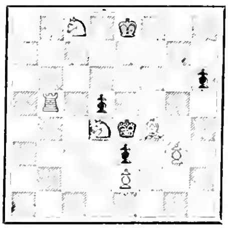
G. Thompson's second letter, relating to B.'s lead in problem I., somehow escaped the editor's notice until now, and has only just reached me. He points out that in a somewhat similar case, in Cavendish's fifth Hand, leader's partner prefers returning his partner's lead, to leading from a hand headed by a tenace (A, Q, and two small ones.) The play in these hands is not intended to guide Whist players, though Cavendish does not call attention to every error in the play. In Hand V, Cavendish calls attention to the importance of returning the best of *two* cards, without saying whether B would not have done better to show his best suit. There is, however, an important difference between the two cases. In Problem I, B. has length in trumps; in Cavendish's Hand V, B. has not. Again, in hand V, B. can return a good strengthening card. The ten of Clubs, in Problem I., played with the certainty that Y (fourth player) holds the Queen, and that Z (second player) is weak in Clubs, could scarcely be considered a strengthening card at all. The words to which I referred (which Mr. Thompson could not identify, having a different edition) are these:—"Many players will not lead from a strong suit if headed by a tenace; preferring, for instance, to lead from ten, nine, three, to Ace, Queen, four, two. They argue that by holding up the Ace, Queen suit, they stand a better chance of catching the King. So far they are right: but they purchase this advantage too dearly: for the probable loss from leading the weak suit may be taken as greater than the probable gain from holding up the tenace." Mr. Thompson argues, however, that the case comes under Cavendish's advice about returning your partner's suit rather than your own, with weak or only moderately strong suits, which you open to a disadvantage. Does Mr. Thompson regard Ace, Queen, to four, as only a moderately strong suit? I should call this great strength. Cavendish refers to such a suit as Queen to four, in some cases King to four, or Queen, Knave, two small ones. Even in such cases as these, if you have strong cards in the adversaries' suits, it is better to show your own. Five of Clubs.

Our Chess Column.

PROBLEM No 25.

By Leonard P. Rees.

BLACK



WHITE

White to play, and mate in three moves.

THE GIUOCO PIANO.

This form of the Knight's opening, called by the Italians the slow game, arises if, instead of moving his Kt. to B.3 on his third move, Black should play B. to B.4., *i.e.*,

1. P. to K 4. / P. to K.4.	2. Kt. to K.B.3. / Kt. to Q.B.3.	3. B. to B.4. / B. to B.4.

White has now four moves at his disposal, viz., 4. P. to B 3

4. P. to Q.3. 4. Kt. to B.3. 4. Castles. All these moves are safe, but slow in their effect. Modern players incline towards a more incisive line of play.

If White plays 4. P. to B.3., with the intention of playing P. to Q.4., when he would obtain a strong centre, the following play would result:—

4. P. to B.3. Kt. to B.3.	5. P. to Q.4. P. takes P.	6. P. takes P. B. to Kt. 5 (ch.)
7. B. to Q.2. B. takes B. (ch.)	8. Q.Kt. takes B. P. to Q.4. or (A.)	9. P. takes P. K.Kt. takes P.
10. Q. to Kt.3. Q.Kt. to K.2.	11. Castles. Castles.	12. Q.R. to K.sq. P. to Q.B.3.
13. Kt. to K.5. Q. to Kt.3.	even game.	

(A.) 8. — Kt. takes K.P.	9. Kt. takes Kt. P. to Q.4.	10. B. takes P. or (A1.) Q. takes B.
11. Castles. B. to Kt.5.	12. Kt. to B.3. B. takes Kt.	Black having a slight superiority.

(A1.) Players of an attacking style might play 10. K.Kt. to Kt.5., then 10. — / P. takes B. 11. Q. to R.5. / P. to K.Kt.3. 12. Q. to R.6. / Q. takes P. 13. Q.R. to Q sq., followed by 14. Castles with a strong attack.

Instead of 9. Kt. takes Kt., White may also play 9. P. to Q 5., a move adopted by Mr. Steel, of Calcutta. This move cramps Black's game very much, and unless he carefully opposes it, he will get a bad game. Black might continue, 9. Kt. takes Kt.

10. Q. takes Kt. Kt. to K.2.	11. P. to Q.6. P. takes P.	12. Q. takes P. Kt. to B.4.
13. Q. to Q.5. Kt. to R.3.	Castles Q.R.	

It would obviously be bad to play 9. Kt. to R.4., as Black would then most likely lose this piece by P. to Q.Kt.4. being eventually played. Checking with the Queen on Black's tenth move would also be bad, as White would play K. to B.sq. On the whole, we think the move 9. P. to Q 5. is sound, and ought to obtain at least a drawn game.

After 1. P. to B.3. / Kt. to Kt.3. White might also continue with 5. P. to Q.3., a move favoured and played often by Mr. Blackburne, who then proceeded to bring his Q.Kt. over to his King's side. Should Black Castle too hastily on his King's side, White would at once proceed with an attack, by advancing P. to K.R.4., *i.e.*,

5. P. to Q.3. P. to Q.3.	6. B. to K.3. B. to Kt.3.	7. Q.Kt. to Q.2. Kt. to K.2.
8. Kt. to B.sq. P. to B.3.	9. Kt. to Kt.3. P. to K.R.3.	10. Q. to K.2. P. to Kt.4.
11. Castles Q R. Kt. to Kt.3.	12. P. to Q.4.* Q. to K.2.	

If, on his sixth move, White proceeds with P. to K.5., Black ought to obtain the better game, *i.e.*,

1. P. to K.4. P. to K.4.	2. Kt. to K.B.3. Kt. to Q.B.3.	3. B. to B.4. B. to B.4.
4. P. to B.3. Kt. to B.3.	5. P. to Q.4. P. takes P.	6. P. to K.5. —

Black's best reply to this is 6. P. to Q.4. If, now, White replies with 7. P. takes Kt., a likely-looking move, Black will have a very good and valid defence, *i.e.*,

7. P. takes Kt. P. takes B.	8. P. takes Kt.P. R. to Kt.sq.	9. B. to Kt.5. P. to B.3.
10. Q. to K.2.(ch.) Q. to K.2.	11. B. takes P. Q. takes Q.(ch.)	12. K. takes Q. P. to Q 6.(ch.),

with the advantage.

Better than 7. P. takes Kt., is B. to Q.Kt.5., but even then Black would get the better game, *i.e.*,

7. B. to Q.Kt.5. Kt. to K.5.	8. P. takes P. B. to Kt.3.	9. B. takes Kt.(ch.) P. takes B.
10. Kt. to B.3. Castles.	11. Castles. B. to Kt.5.	12. B. to K.3. Kt. takes Kt.
13. P. takes Kt. P. to K.B.3.	14. P. to Q.R.4. P. takes P.	15. P. to R.5. P. to K.5.,

and Black has the better game.

(To be continued.)

* As played in the match by Blackburne against Zukertort.

GAMES BY CORRESPONDENCE. *(Continued from p. 415.)*

GAME I.

CHIEF EDITOR.	CHESS EDITOR.
	10. B to QKt3
11. Q to K2	11. Castles
12. P to KR4	12. P to QR3
13. P to KKt8	13. Kt to KB4
14. R to KR3	14. P to Q5
15. Kt to Kt5	15. P takes P
16. B takes Kt	16. R takes B
17. P to KKt4	

GAME II.

	9. Kt to KB3
10. Kt to Q2	10. Kt to KB4
11. P to QB3	11. Kt to Kt sq
12. Kt to QB4	12. B to 33
13. K to B2	13. P to KR3
14. B to Q3	14. Q to B2
15. Kt to QR5	15. P takes P

ANSWERS TO CORRESPONDENTS.

**** Please address Chess-Editor.*

J. F. B.—You have taken the correct view of the subject in your last letter to us.

Geo. Bell.—Only Pawns "can take Pawns" in passing. For instance, place White Pawn on K5, Black Pawn on Q2; if now Black moves P to Q4, White's Pawn can take the Black Pawn as if it had only moved to Q3.

Clare.—Cook's "Synopsis," 3rd edition, or Gossip's "Theory of the Openings."

J. Park.—Game well known; also published in Howard Taylor's "Chess Brilliants." 2. From about 1840 to 1850.

J. Hall.—Thanks.

D. See.—We should be going beyond the scope of our Chess Column.

John Griffith.—In Castling the King only must not pass a square commanded by a hostile piece. 1. Wormald has about 74 or 76. 2. It is quite a different thing. 3. No. 4. 3s. 6d.

Letters received from J. Licence, J. Watson.

Contents of KNOWLEDGE *No.* 19.

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KNOWLEDGE
AN ILLUSTRATED MAGAZINE OF SCIENCE
PLAINLY WORDED—EXACTLY DESCRIBED

LONDON: FRIDAY, MARCH 24, 1882.

CONTENTS OF No. 21.

THE FIRST DAFFODIL.

AFTER watching it closely for four or five days, I have just found the first daffodil of the season wide open this morning, with a big humble-bee buried deep in its capacious throat, already rifling its little store of nectar, and dusting his body and legs with pollen which he will promptly carry away to fertilise one of its pretty sisters in some neighbouring garden. Though I have watched it like a child, I could not resist the childish temptation of picking it, and I have got it here before me now for dissection, poor thing, with my little pocket-knife, though it does not need much of a magnifying power to see all that need be seen of its structural arrangements. It is only a common wild English daffodil: the "daffy-down-dilly who came up to town in a yellow petticoat and a green gown," as the old nursery rhyme has it; and it has been simply transplanted hither from the meadow beyond the bourne; but it is as gay and bright a blossom as one could wish to see, for all that, besides being full of genuine scientific interest for those who care to look at it aright. Let me cut it straight down through the middle, so, and then you will understand better what it is driving at.

You see, the flower consists of a single amalgamated tube, with six lobes or points, and in between them, projecting from its centre, is a large circular crown, broadly tubular in shape, and brightly yellow, like the rest of the blossom, in colour. It is well to begin at the beginning; and so we may first ask why it is six-lobed? The answer is, because it is one of the monocotyledonous plants. That is a very long and technical word—I am half afraid our English-speaking editor will cut it ruthlessly out—and, indeed, I wish it were shorter and simpler; but at present, unhappily, I know of no other that will efficiently supply its place. Let me try to explain it. Many years ago, when flowering plants first appeared upon the earth, they began to diverge into two principal divisions, from one or other of which all our existing flowering kinds (except only the cone-bearing pine family) are ultimately descended. One of these primitive groups had two seed-leaves in each seed, the other had one. There are a great many other differences between the two tribes, but these are the most constant; and it is to the last tribe that the daffodil belongs. Now, so far as the flower is concerned (and it is with that part of the plant alone that I am going to deal to-day), the widest original difference between the two great divisions was this: the plants with two seed leaves had their parts arranged in whorls of five, while the plants with one seed-leaf had them arranged in whorls of three. Thus the typical flower of the first class has five sepals, five petals, five stamens, and so forth; while the typical flower of the second class has only three of each. In the course of time, however, this original difference has become greatly masked; for many flowers of the first kind have lost one or two of their petals or stamens, by coalescence or otherwise; while many flowers of the second class have doubled their numbers in one part or another. Nevertheless, in most cases, we can even now trace, in some way or other, the steps which connect the existing form with its primitive ancestor; and it is still true that the two types are broadly marked off from one another, as the five rayed and the three rayed forms respectively.

Now, the daffodil is a very advanced and highly modified development of the three-rayed type. The artificial family to which it belongs in the present somewhat irrational arrangement of flowers is that of the amaryllis kind; but we shall understand it better if we look first at its near neighbours of another family, the iris and crocus group. These plants in some of their modifications, such as the common yellow flag, are very simply three fold in their ground-plan. There are three seed-cells to the pistil in the centre; then there are three stamens outside them; next, there are three petals; and, last of all, there are three large spreading sepals in the outermost whorl. But in the crocus, the three petals and three sepals are indistinguishable, and have coalesced into a single tube, so that the flower seems to have a united corolla of six lobes. Now, in the amaryllis family, to which the daffodil belongs, we get the same sort of tendency carried a little further. Instead of having only one row of three stamens, the members of this group have two rows, thus making a total of six—for, though no mathematician, I will fearlessly venture upon so much arithmetic as that. In the simpler amaryllids, such as the snowdrop, the confusion goes no further than this single step; and we get, first, a three-celled pistil in the centre; next, six stamens in two rows outside it; then three small green-veined petals; and last of all, three large pure white sepals. Here the original three-fold symmetry is hardly at all masked by the occurrence of a double set of stamens; while the petals and sepals are quite separate down to their very base, without any sign of union or coalescence. I don't say they never have been united; indeed, I have certain grave doubts of my own upon that head, connected with what botanists call their inferior ovary; but I'm not going to mention that point to-day, lest I should tell you too much about them all at once, and so spin out my paper to an unconscionable length. For the present, it must suffice to notice that we still possess amaryllid flowers in which the primitive arrangement by threes is even now distinctly visible.

The daffodil, however, has got beyond this early stage, and has undergone so very much modification that its primitive aspect is almost entirely masked by its acquired traits. When I slice across its ovary, or embryo fruit, it is true, I can see that it still consists of three cells, produced by the union of the three originally separate pieces; but with this exception, all its parts now appear to be in sixes rather than in threes. There are six pollen-bearing stamens, produced by doubling the original three; and there are six

ube to the corolla, produced by the coalescence of the three petals with the three sepals, so as to form a single united tube. The object of this coalescence is easy enough to understand. As in the harebell, the daisy, and so many other flowers, it has been effected by the selective agency of humble-bees and other insects, like the one whom I found buried so deeply in its throat this morning. The tubular form, with its stamens hanging out from the side, ensures the fertilisation of the flower much better than the system of open petals; and so it has been brought about by the fact that any variation in that direction was unconsciously favoured by the insects, while variations the other way were universally neglected. But while many other plants have hit upon this same device of coalescence, few have carried it so far as the daffodil. In the first place, the tube in the five-rayed flowers is formed out of the petals alone; but in the three-rayed flowers, the petals are too few in number to make a sufficiently wide tube, and so the sepals or calyx pieces are joined with them in producing the desired result. Thus we can trace a gradual progress from flowers like the iris and snowdrop, where the sepals are distinctly different from the petals, through flowers like the wild hyacinth, lilies, snowflake, and flowering rush, where all six pieces are equal and similar, to flowers like the crocus, meadow saffron, and daffodil, where the six pieces are united together into a long tube. But, furthermore, and in the second place, the daffodil and the others of the narcissus kind have done more than the mere ordinary tubular blossoms, inasmuch as they have produced a singular outgrowth in the shape of the crown or cup, which forms, as it were, a vestibule to the tube, and thus still better ensures the proper fertilisation of the flower. In some of the pink tribe (amongst the five-rayed flowers) we get a scale or parapet on each petal in somewhat the same way; but in the daffodil and its allies the crown is united and circular, like the tube, though one can still trace six wavy lobes or sinuosities on its edge. In some exotic members of the narcissus group the crown is very small and rudimentary, and is brilliantly coloured with red or orange, so that it seems rather to act as a honey-guide for the bees than as an additional aid to fertilisation; but in the wild English daffodil it has reached a very high state of development, and occupies at least half the entire length of the blossom.

One word more as to its colour. The daffodil is a pale yellow, and it apparently depends mainly for impregnation upon the visits of diurnal insects. Hence it is quite scentless, for its large size and brilliant colour suffice to attract quite enough visitors, without any necessity for the extra allurement of sweet perfume. But many of the southern species, like the jonquils of our flower gardens, have pure white petals, and possess a very powerful jasmine odour. Such white, strong-scented flowers always depend, in part at least, upon night-flying moths, which are largely attracted by perfume; and, of course, no colour can be so well perceived in the dusk of evening as a pure glossy white. Hence the difference in hue between the two kinds. At the same time, the southern varieties are also fertilised by day-flying bees, and for these the frill of the crown is prettily fringed with brilliant orange. Each insect selects the plant that suits it best, and their joint selection has thus produced the snowy petals and exquisitely-coloured cup of the garden jonquil.

A STUDY OF MINUTE LIFE.

By HENRY J. SLACK, F.G.S., F.R.M.S.

No. II.

THE editor will, it is hoped, allow the writer to explain that through the accident of his not receiving a proof for correction, many typographical errors appear in the last paper. The principal are, "cilian" for *cilium*, "injurious" for *in infusions*, in the sixth line from the bottom of the first column; "divided" instead of *directed*, eleventh line from the bottom of second column; and "analysis" for *analogy*, eighth line, p. 372.

If we were engaged in tracing life from its simplest modifications upwards, we should now speak of those objects, such as amœbæ, which are composed entirely of small masses of protoplasm, not built up into any positive structure. Some notice of these is reserved for a future paper, but it is best for the student to begin with objects that can be obtained for certain, without difficulty, and which are easy to observe. It may, however, be mentioned now that amœboid creatures, in their ordinary and simplest state, have no permanent distinction of parts. They put forth prolongations and draw them in again; portions that were outside get inside, as they move on in a slobbery way, and they swallow their food, not through any special aperture, but anywhere, by flowing all round it.

It is not uncommon to hear people talk about "homogeneous protoplasm," but as there is, and can be, no such thing, it is not correct to describe any amœboid object as composed of it. Life is only manifested by the co-operation of divers matters, highly complex in chemical constitution, and able to perform different functions. The ciliated infusoria, of which the Paramecium, spoken of in the former paper, is a good example, is a little bag of skin sufficiently firm to support the cilia, and full of the protoplasmic material. When we examine this material, wherever it is found in a living state, we see a vast number of particles in a viscid fluid. Their optical aspect suggests that they are not all alike in molecular structure; and if their extreme minuteness did not render it impossible to separate them for analysis, we should find they varied in composition. Lumping altogether, particles and viscous fluid, the constituents of protoplasm are found to resemble those of the white of an egg. It belongs to a group of substances found in all living things, and which carry on the chief vital work. Carbon, hydrogen, nitrogen, oxygen, sulphur, and phosphorus all combine to build up the molecules of this material, and the compound is in a state of such delicate equilibrium that it is easily modified or decomposed.

The Paramecium, and similar creatures, take their food in by a mouth, and in the early stages of knowledge concerning them, the great German naturalist and microscopist, Ehrenberg, thought they possessed a multiplicity of stomachs, because the food particles were dispersed in many little spaces. Their processes of digestion and assimilation are probably carried on without the help of special organs, though, no doubt, the particles seen in the protoplasm have the power of performing different kinds of work. The infusoria will not swallow *everything*, but their selective faculty is very small, and they readily take in particles of indigo or carmine, floating in the water about them, although they are of no use to them as food. Microscopists have long been fond of feeding them in this way, as the colouring matters can be traced inside the little animals. The cilia near the mouth bring all sorts of objects floating in the water towards it, and other cilia make an inward current to suck in what the creatures want. The appearances often seem to support the many-stomached, or polygastric, theory, but the vacuoles in

which the food particles assemble are not constant in their position, and certainly do not possess any firm walls. Although the vacuole has not the structure of a vessel, when food particles are assembled in its little open space, it acts just like a real stomach, and we must conclude that fluids specially adapted for the work of digestion are prepared by some of the visible granules and poured into it. The nutritive products of the digestion diffuse themselves through the soft moist mass of the creature, and the useless and used-up matters are excreted, in many species through a definite anal aperture.

The organs of these transparent and minute creatures, when they have such, are often impossible to trace. Paramecia, and many others, for example, have a contractile vesicle which expands as if filling itself, and then contracts rhythmically, and it is supposed from the analogies offered by larger creatures, that these vesicles, whose walls are invisible, are connected with a series of tubes through which fluids are impelled for both respiratory and excretory purposes.

However small may be the creatures and their particles of food, the composition of the latter, no doubt, resembles that of higher animals, and has to be digested and assimilated by a succession of similar processes. The microscopic plants supply complex nitrogenous as well as starchy materials, and the sarcode of the microscopic animal resembles in ultimate composition the flesh of higher creatures.

The hay infusion is sure to supply some animalcules, showing an advance of structure upon that of the Paramecium. A little oval creature, called Stylonichia, scarcely so long as the largest Paramecium, is common, and has, besides swimming cilia, some stiff bristles at each end, and with these it can walk briskly over any substance. There are many other animalcules similarly provided. The skin, or integument, in some cases acts like the crab's shell, or the insect's external skeleton of the horny substances called chitin.

Let us for a moment consider the indications of nerve power, without nerve structure, afforded by the creatures we have noticed. First comes the perpetual movers. The action of their surroundings upon their bodies causes the molecules that do rudimentary nerve work to respond to certain impressions by stimulating the cilia to their rapid motion. The creatures that can be quiet when in full vigour, and vary their movements with apparent purpose, prefigure the voluntary actions of higher beings. The little animals that can run, or swim, exercise something that prefigures choice as to which set of locomotive organs they employ. The reader may ask, Have they any sense of pleasure in their busy and merry-looking existence? To this very natural query it is difficult to give even a conjectural reply. Consciousness of existence could not be conceived of them, but a feeling of pleasure may long precede any kind of knowing, and we may gratify our own sympathies, and not be far wrong if we deem them happy in their little way.

NIGHTS WITH A THREE-INCH TELESCOPE.

By "A Fellow of the Royal Astronomical Society."

PENDING the appearance in the eastern and south-eastern sky of the spring and summer constellations, we shall devote our present night to an examination of the more strictly circumpolar ones. Our original intention was to have gone over Virgo and the neighbouring region of the heavens. This, however, had, we now think, better be deferred until that constellation approaches nearer to the meridian during the working hours of the ordinary amateur observer. Moreover, more than one of the constellations we propose to investigate is now in a very favourable position. First, then, let us turn to, perhaps, the best known of them all Ursa Major (Map, p. 381), now high in the north-east. We will begin by turning our telescope, armed with a power of 120, upon ζ (Mizar). Sharp sighted people will detect with the naked eye a small star (Alcor) in the immediate neighbourhood of Mizar. In the telescope with the power specified, Mizar itself will be seen to be double, and forming with Alcor the pretty triple system shown in Fig. 23.

The pale-green of the small star of the pair will be noted. ξ Ursæ Majoris, examined with the very highest power at the disposal of the observer will furnish an absolutely crucial test of the excellence at once of his eye and telescope. 23 Ursæ Majoris is rather a wide pair, but interesting from the different tints of its components. 57 is a pretty pair for a similar reason, but very much closer than the last; it is unnumbered in the map. 65, a fine triple, is also unnumbered, but may be recognised to the south of χ on the boundary of Canes Venatici. γ Ursæ Majoris lies in a fine field of stars. This constellation, we may remark, swarms with double and triple stars, but as in a large proportion of cases they are of less than the 6th magnitude, the map takes no account of them, and it would be useless to give their co-ordinates, unless the observer's instrument were equatorially mounted. Several interesting nebulæ are to be found in Ursa Major, but in the case of the student for whom these papers are written, it can only be by fishing. If he will conceive an equilateral triangle, to be described with α and 23 Ursæ Majoris at the extremities of its base; then, by sweeping about to the right of its apex with the very lowest power he possesses, he may hit upon the two nebulæ 81 and 82 Messier, $\frac{1}{2}$° apart. About 2° (four diameters of the moon) south-east of β is another nebula, 97 Messier, a pale circular object, looking like the ghost of a planet. An imaginary line drawn diagonally from α through γ Ursæ, and continued nearly as far again, will strike upon I V. 13, an oval nebula. Half-way, too, between β and 97 Messier lies I V. 46. This will require some gazing at with so small an aperture.

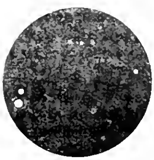


Fig. 23.
ζ Ursæ Majoris.

Fig. 24.
The Pole Star.

Fig. 25.
η Cassiopeiæ.

And now we will direct our telescope, armed with a power of 160, to the Pole Star, which will be seen as depicted in Fig. 24.

This is sometimes alleged to be a test for a 3-inch telescope, but it is not so. Dawes has seen the companion with a 1·3-inch object glass, and the eagle-eyed Ward, of Belfast, with only 1·25-inch aperture! North-west of ζ, Ursæ Minoris will be found π^1, a wide and easy object.

Cassiopeia is one of the constellations through which the Milky Way passes, and hence it affords innumerable rich fields and clusters to repay the observer who sweeps and fishes over it; γ, to begin with, lies in a fine field of small stars. η Cassiopeia, shown in Fig. 25, as viewed with a power of 160, is a beautiful object, the

colours being so well contrasted. Ψ is a triple star, but, with our optical means, will only be seen as a rather wide double. σ Cassiopeiæ, to the south of β, is a beautiful, delicate, and by no means easy, double star; a sort of miniature of ε Bootis, which we shall describe in a future "Night." About ι, between γ and κ, between π and ο, &c., lie some of the beautiful fields of stars to which reference has been made above.

Camelopardus contains several more or less striking pairs; but as none of them are marked in our map of reference, we pass on to Lynx, where we find 38 (Map, p. 298) a very close, delicate, and rather difficult pair. 19 Lyncis is a pretty triple, but it does not appear on the map. As both Cepheus and Draco are below the Pole, we must defer our description of the principal objects of interest they contain until some future occasion.

Errata. In "Nights with a Three-inch Telescope," on p. 376, paragraph 2, line 4, "lower circle" should be hour circle; and at the beginning of paragraph 3, "Cancri" appears instead of Cancer.

NOTES ON ROWING.

By an Old Club Captain.

Consider now in what respects the racing-boat of our time differs from the racing-boat of 1840. It is much lighter, it is much narrower and sharper, and has a perfectly smooth keel, so that it encounters a much smaller resistance, the leverage of the oar is greater, and the oar is longer. Taking the last point first, we see that the oarsman *must* pull the oar more sharply to give even the same velocity of propulsion as in the old boats, for the simple reason that he works at the end of a longer arm, while the increased length of the other part of the lever (the oar from blade to rowlock) only makes up for this increase in the length of the part which lies between the rowlock and the handle. It can readily be shown that, apart from the acquired motion of the boat, the driving distance for one full stroke of the oar would differ very little with the longer oars but increased leverage of our time from that obtained with the old style of oars, if the angle through which the oar is swept were the same as of yore. But to obtain this angular sweep the handle of the oar of our time must be carried through a distance, greater in just the same degree that the distance from handle to rowlock has been increased. The hands must, therefore, move more quickly to give the same rate of propulsion as to the older boats. But these boats will take and retain between the strokes a greater rate. Consequently the oar must be urged more sharply still, if it is to be effective in giving to them the greatest speed they can attain. The long, steady pull proper in the old racing-boats would give, no doubt, to these much lighter boats the same rate of speed that it gave to the heavier boats, and with much less effort to the oarsman. But the racing-boat would not then travel at the best pace that can be given to it.

I would call special attention here to the circumstance that it is not a mere matter of opinion, but of absolute certainty, that the same stroke which was good for the old fashioned racing boat must be ineffective for the modern, outrigged, smooth bottomed, light racing craft. It can even be shown that the actual stroke rowed by Selwyn and his contemporaries, would not do more than simply follow the motion of a racing craft at full speed, instead of adding to its velocity.

Let us run through a little calculation, the elements of which, be it noticed, are not open to doubt or question:—

The University boats go over the 4¼ miles course on a good tide in about 21 minutes. We shall not be far wrong in saying that a very good racing boat would cover 4 miles *on still water* in about 20 minutes, or would move at the rate of 1 mile in 5 minutes (note that whether a boat is travelling with or against the stream the rower works as if in still water, for the boat shares the motion of the stream). Certainly this speed is attained in spurts, and a still higher speed in sharp bursts over a short course. A mile in 5 minutes, means 352 yards per minute, or 5·87 yards (or 17·6 feet) per second. This speed is not absolutely constant even in the lightest and best of our racing boats; but as every one knows who has watched the progress of a bumping race when the pursuing boat has its nose very near to or overlapping the stern of the pursued, falls off perceptibly between the strokes. Still the falling-off is very much less than in the best boats of half-a-century ago. We may fairly take 19 feet per second as the maximum velocity attained just at the end of stroke, and 16 feet per second as the minimum velocity just before the beginning of the next stroke.

Now, 40 strokes to the minute is pretty nearly the maximum attained even in spurts, the tendency being (as I shall presently explain) to diminish rather than to increase the number of strokes per minute. At any rate, 40 strokes per minute is very quick rowing indeed. If, then, the boat travels 17·6 feet per minute, she covers about half as much again per stroke, or 26·5 feet. In other words, a boat travels very nearly nine yards at each stroke, in the case of an absolutely first-class and perfectly trained "eight" in a good craft, rowing at top speed.

Now, it requires, with an oar suited for an outrigged craft, a very good reach forward and a good pull home, to give the blade a sweep of six yards in the water; and, taking account of slip through the water and of the arc nature of the blade's motion, we may consider five yards an unusually good *effective* sweep. Now, with the old-fashioned stroke, the oar was in the water at least twice as long as in the air, during each complete stroke (from feather to feather). If, then, this stroke were rowed now, the oar would be two-thirds of the fortieth part of a minute in the water, during which time the boat, with the motion already considered (whether supposed to be communicated by previous effective strokes or by the rest of the crew rowing properly), would travel six yards; so that rowing the old fashioned stroke in the old-fashioned way, an oarsman would not be driving the boat at all, but simply following with the blade of his oar the (relative) motion of the past rushing water.

It is obvious, then, that the stroke which was so effective in 1840 will not do now. It is equally certain that the requisite rapidity of propulsion cannot be attained by rowing the same sort of stroke, but more to the minute; for experience shows that no crew can keep up so quick a stroke as would be required—rowing full length, be it noticed, for else the quickening would do no good. Nor can men take a much longer stroke (in the same time), even with the modern sliding seats. Leverage is lost with increase of length; and though up to a certain point this consideration must be overlooked, it tells very much when the question is of adding a foot or so of forward reach to the reach which had already brought the oar to an inclination of some 40 degrees to the mid position when the leverage is greatest. The sliding seats add something to the old length of stroke,

and doubtless with advantage, but we were considering their use in the above calculation. More could certainly not be added, without bringing the oar to a position in which a large part of the oarsman's strength would be wasted in pushing the water from the boat instead of parallel to the boat's length.

It is, then, a simple matter of demonstration that the stroke must be changed, in the modern racing craft, in respect of the time during which the oar is in the water. If a greater rapidity of propulsion is required, as we have proved, and neither the number of strokes per minute, nor the length of the stroke, can be increased beyond a certain point, which does not suffice to give the necessary rapidity of propulsion, it follows of necessity that the oar must be a shorter time in the water and a longer time out of the water.

This is commonly misunderstood, especially by persons who have never rowed in light racing craft. They say, the stroke must be kept "long in the water," and in one sense they are perfectly right: the stroke of the oar in the water must be as long as possible in distance, but *not in time*. But then "comes answer like an Absey book," You advocate a quick stroke, and more than so many—say from forty to forty-four strokes per minute—should not be taken. This again is true, the oar must be dashed through the water quickly (or rather, for in good rowing there is very little slip, must be dashed down sharply against the water and the boat lifted along by sharp strong pressure against the water), but not too often to the minute. But then, again, comes still the objection, That means a slow feather, for if each stroke from feather to feather occupies a certain time, shortening the stroke means lengthening the feather; and every moment that the oar is out of the water the boat is losing speed. Again we reply, the objection is valid; but it is a necessity of the case that to give the swift, sharp impulse to the long, but quickly drawn, stroke, the oarsman must take a longer time in the feather. Of course, the best thing of all would be to have as many strokes as possible per minute, the longest possible stroke, taken in the shortest possible time, with the longest possible oars, and in the lightest possible boats. But the rowers being limited in their powers, the choice must be made between long dragging strokes with lightning feather, and long but swift strokes with less rapid recovery; and as the long dragging strokes would simply not propel a boat at all at the swift pace of a modern racing craft, the long, swift stroke must be taken. (Here Pendragon, of the *Referee*, who sat heavily upon the Editor, three years ago, for asserting the necessity of these long, but swiftly taken strokes, may come in if he please, and say we advocate short, swift strokes, which every great race of the last ten years has shown to be ineffective.)

To sum up,—a modern racing eight, whatever may have been the tub practice of the crew, must be driven by sledge-hammer strokes, long and sweeping, but sharply taken, and resulting from the concentrated exertion of all the energies of the body, followed by a moderately quick recovery, during which the oarsman gathers himself together as it were for the next great effort, a momentary pause (which in old times would have been called a hang on the feather, and deservedly criticised as a defect) preceding the simultaneous plunge of the eight oars to grip the beginning of the stroke. In other words, a stroke must be taken which, with the old fashioned boats, even when they had attained their best speed, would have meant breaking the oars, if the men were only strong enough to give it. How this impulsive stroke is to be given we shall consider in our next.

THE CRYSTAL PALACE ELECTRICAL EXHIBITION.

SEVENTH NOTICE.

THE Lane-Fox (Fig. 3) differs somewhat from the two lamps described last week, the chief difference being in the connection between the filament and the wires from the generating machine. The glass bulb is about 3 in. long and $2\frac{1}{2}$ in. in diameter. The filament is made from bass-broom, and is connected to short pieces of platinum wire, which are fused into the glass tubes A, B. The lower ends of the wires are immersed in small quantities of

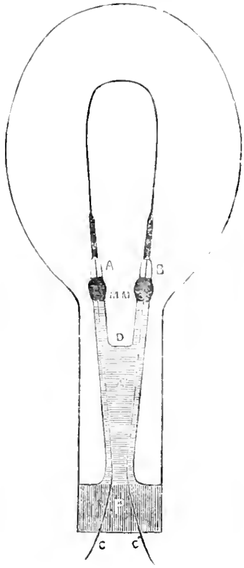


Fig. 3.

mercury, M, M'. Two copper wires, C, C', pass from the mercury through the lower portions of the tubes, A, B, (which merge into one tube at D), and thence outside the lamp. The wires are then soldered to the portions of the socket connected to the machine. The tubes, A, B, and their continuation, D, are filled with plaster of Paris, P, and wool, W, keeping the copper wires and mercury in position.

The Maxim lamp (Fig. 4) exhibits another form of contact or connection. The globe is about $2\frac{3}{4}$ inches in diameter, the neck, A, B, being turned inwards until the aperture at C, is reduced from about an inch to a quarter of an inch. A glass rod, CD, is then fused on at C, and has two platinum wires passing through it. Externally the wires are continued to E and F (outside the neck), so that in fitting the lamp into its socket, the wires come into contact with two insulated springs, each of which is connected to one of the machine wires. Inside the lamp, the platinum wires are flattened and bent into a kind of hook (H). The filament, which is flat and in the shape of a gridiron, or letter M (for Maxim), is made from paper, so that in cutting it out, there is little or no difficulty in making the extremities considerably broader than the other

portion. A small circular platinum washer is placed on one face of each end, the hooked end of the platinum wire on the other, and a small bolt (?) passes through the whole. Small nuts screwed on to the bolt clamp the three parts, and so ensure the connection.

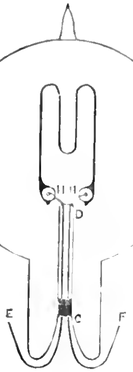


Fig. 4.

The British Lamp (Fig. 5) is perhaps the most recently offered to the public. It consists of a glass globe, $2\frac{1}{2}$ inches in diameter, continued by a comparatively long glass tube. The filament is made from cocoa nut fibre, and is attached at A and B by means of small carbon

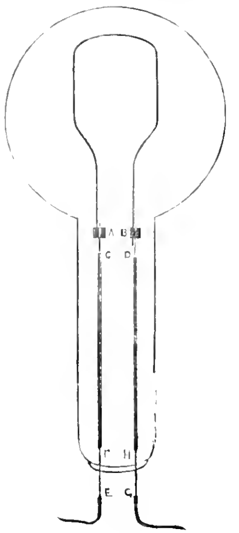


Fig. 5.

tubes to two short platinum wires, AC and BD. Two other platinum wires, EF and GH, are fused into the lower part of the glass tube, FC and GH being lengths of copper wire connecting the platinums. Externally, copper wires connect the platinum to the wires from the machine.

It will doubtless be noticed that the similarity between the various lamps is, at least in regard to the principles involved, very great. The slight variations or differences may be classed under two heads viz. (1), the substances from which the filaments are made, and (2) the methods of connecting the filaments to the external circuit. Platinum, so far as is at present known, is the only substance available for passing *through* the glass, being the only metal which will fuze into that substance—that is to say, it is the only metal which expands and contracts at the same rate as glass on an increase or decrease of temperature. Its use is, therefore, inevitable. Swan, Edison, and Maxim take their platinums from the filament to the outside of the bulb. Lane-Fox, however, connects the interior to the exterior by means of mercury—a device which appears useless, for equally good contact could be made by attaching the copper to the platinum. The copper wires inside the British lamp are evidently used on the score of economy, platinum costing about 25s. to 30s. per ounce. Whether it is true economy or not, remains to be proved. We, however, fail to see why this form of lamp should be valued at 10s., while the Swan can be bought for 5s.

Great efforts have been made by the different exhibitors to secure the public favour, and in some cases these efforts have been successful; but there are times when we detect traces of a want either of ordinary energy or of practical knowledge, and it is to the absence of such shortcomings that we must look for an explanation of much of the favour with which Mr. Edison's exhibits are received. Although he has a large staff in London, most of them are Europeans, and are, therefore, not open to the charge some people seem disposed to hurl at them, as accounting for their diligence and enthusiastic loyalty to their employer, viz., that of being Americans. It would be somewhat invidious to draw any distinction, or to refer to any particular instance of inexperience, but we may mention one which came under our notice some years since. Shortly after the transfer of the telegraphs to the Government, a number of men were sent to construct a line of telegraph in a woody district not a thousand miles from London. They did their work during the winter months, and followed out to the letter instructions to keep the line as clear of the road as possible. They did their work *too well*, for when the spring and summer came, and covered the trees with shoots and leaves, the wires were enveloped and lost sight of, and as a consequence the slightest shower of rain rendered them useless. The result of this want of experience was that the line had to be taken down and reconstructed. Electric lighting, however, is a young industry, and some time must elapse before all its votaries acquire their necessary information

There are other points of great interest in connection with incandescent lighting, which, however, we cannot refer to now, but will avail ourselves of the opportunity in our next notice.

THE GREAT PYRAMID.

By the Editor.

THUS far all has been tolerably plain sailing. Of the astronomical use and purpose (not quite the same thing, be it noticed) of the Great Gallery, there can be small room for doubt, when we find (1) every feature in all the passages and in the Great Gallery correspond with the requirements of the theory, and (2) many features explicable in no other way.

But here our difficulties begin. Astronomy no longer lends its aid when we ask why the builder of the Great Pyramid wanted to have an astronomical observatory as well as a tomb. To begin with, I suppose Egyptologists are quite clear that a main purpose of each pyramid was that it should serve for a tomb. And I suppose, further, that this being so, it was essential that each pyramid, including that one which we have been regarding hitherto only in its astronomical aspect, should be as nearly as possible completed before the death of its future occupant. There may be, for aught I know, some reason to believe that in the days of the pyramids an Egyptian king might be able in some way to assure himself of the *bonâ fides* of his successors, and that they would continue the work which he had begun and more than half completed. But it is very difficult to imagine that this really was the case. Human nature must in those days have resembled pretty closely human nature in our own time; and it seems as unlikely that a king could trust in his successors so far as to believe they would expend large sums of money and a great amount of labour, in completing a work in which they had no direct or actual interest, as that, supposing he trusted them to this degree, their conduct after his death would have justified his confidence. Thus, when we find that the Great Pyramid was actually completed in the most careful and perfect manner, we have very strong reason for believing it to have been all but completed during the lifetime of the king, its builder if it was indeed intended for his tomb. I must confess that the exclusively tombic theory of the Great Pyramid (at least) had always seemed to me utterly incredible, even before I advanced what seems to me the only reasonable interpretation of its erection. One may admit that the singular taste of the Egyptian kings for monstrous tombs was carried to a preposterous extent, but not to an extent quite so preposterous as the exclusively tombic theory would require. Of course, when we see that the details of the great edifice indicate unmistakably an astronomical object, which was regarded as of such importance as to justify the extremest care, our opinion is strengthened that the pyramid was not solely meant for a tomb. For this would bring in another absurdity, scarcely less than that involved in the exclusively tombic theory of structures so vast, if even they were non-astronomical,—this, namely, that the Egyptian kings thought the celestial bodies and their movements so especially related to *them*, that their long home must be astronomically posited with a degree of care far surpassing that which has *ever** been given to an astronomical observatory. Common sense compels us to believe that whether the Great Pyramid was meant for a tomb or not, its astronomical character was given to it for some purpose relating to the living king who had it built. (I suppose Egyptologists are absolutely certain that the Great Pyramid *was* built by one king, and, therefore, within a few decades of years.)

Now, it is not reasonable to suppose King Cheops' purpose was simply scientific. We may fairly take it for granted that the king who expended such vast sums and sacrificed so many lives to build for himself a tomb, was not a man taking a disinterested interest in science, or even ready to help the priests of his day to regulate religious ceremonials by astronomical observations conducted with reference only to general religious relations. To put the matter plainly, the builder of the Great Pyramid must have thought of himself first; next, of his dynasty; then, perhaps, of the priesthood (though always with reference to the bearing of religious ceremonies on the welfare of himself and his dynasty); lastly, of his people, as part of his wealth and power. For abstract science he cared not, we may be well assured, a single jot. I do not wish to suggest that Cheops was wickedly selfish. I have no doubt he was thoroughly persuaded that he was carrying out the purpose of his existence in expending much treasure and many lives on his own well-being (both before and after death). But there can be no doubt this *was* the real object of his expenditure of time, and wealth, and human life on the great structure which bears his name.

Now, our thoughts are at once turned by these considerations to that one sole line along which astronomy ever has been followed with the hope of material profit: and we are led to remember that if there is one idea which has more strongly taken possession of the human race than any other, or one which more than any other is associated with the astronomy of ancient Egypt, it is the idea that the stars in their courses rule the fate of men and nations. When we remember that even now, when science has shown the utter incorrectness of the ideas that underlie the ancient system of astrology, this system has its influence over millions. Even now the terms belonging to the system remain part of our language. Our very religion has all its times and seasons regulated in ways derived from the astrological system of old Egypt. Our Sunday is the old Chaldæan and Egyptian quarter-month rest day, and the Jewish Sabbath is this quarter-month rest day associated with the belief in the malefic influence of the planet (Saturn), which formerly ruled the last day of the week (still called Saturday or Saturn's day). The morning and evening sacrifices of the Jews and their new moon festivals were manifestly astronomical in origin—in other words astrological (for astronomy was nothing except as astrology to the old Chaldæans and Egyptians). The Feast of the Passover, however later associated with other events, was derived from the old astrological observance of the passage of the sun (the Passing over of the Sun-God) across the equator, ascendingly; while the Feast of Tabernacles was in like manner ruled by the passage of the sun over the equator descendingly. Our calendar rules for Easter and other festivals would never, we may be well assured, have been made to depend on the moon, but for their original derivation from astronomical (that is astrological) ceremonial.*

When we remember that the astronomy of the time of Cheops was essentially astrology, and astrology a most important part of religion, we begin to see how the erection of the mighty mass of masonry for astronomical purposes may be explained,—or, rather, we see how, being certainly astronomical, it *must* be explained. Inasmuch as it is an astronomical building, erected in a time when astronomy was astrology, it was erected for astrological purposes. It was in this sense a sort of temple, erected, indeed, for the peculiar benefit of one man or of a single dynasty; but as he was a king in a time when being a king meant a great deal, what benefited him he doubtless regarded as a benefit also to his people: in whatever sense

* Even in our own time, though we get greater accuracy in our observations than Cheops obtained in his pyramid, we have not to give anything like the same degree of care to the work.

* The Jewish people, when they left Egypt after their long sojourn there, had doubtless become thoroughly accustomed to the religious observances of the Egyptians (at any rate there is not the slightest reference even to the Sabbath before the sojourn in Egypt), and were disposed not only to retain these observances, but to associate with them the Egyptian superstitions. We know this, in fact, from the Bible record. Moses could not—no man ever could—turn a nation from observances once become part of their very life, but he could, and did, deprive them of their superstitious character.

the Great Pyramid had a religious significance with regard to him, it had also a national religious significance.

It would have been worth Cheops' while to have this great astrological observatory erected, even if by means of it he could learn only what was to happen, the times and seasons which were likely to be fortunate or unfortunate for him or his race, and so forth. But in his day, as in ours, astrology claimed not only to read but also to rule the stars. Astrologers did not pretend that they could actually regulate the movements of the heavenly bodies, but they claimed that by careful observation and study they could show how the best advantage could be taken of the good dispositions of the stars, and their malefic influences best avoided. They not only claimed this, but doubtless many of them believed it; and it is quite certain that those who were not astronomers (*i.e.*, astrologers) were fully persuaded of the truth of the system which, even when the discovery of the true nature of the planets has entirely disproved it, retains still its hold upon the minds of the multitude.

There is, so far as I can see, no other theory of the Great Pyramid which even comes near to giving a commonsense interpretation of the combined astronomical and sepulchral character of this wonderful structure. If it is certain on the one hand that the building was built astronomically, and was meant for astronomical observation, it is equally certain that it was meant for a tomb, that it was closed in very soon after the king died for whom it was built, that, in fine, its astronomical value related to himself alone. As an astrological edifice, a gigantic horoscope for him and for him only, we can understand its purport, much though we may marvel at the vast expenditure of care, labour, and treasure at which it was erected. Granted full faith in astrology (and we know there was such faith), it was worth while to build even such a structure as the Great Pyramid; just as, granted the ideas of Egyptians about burial, we can understand the erection of so mighty a mass, and all save its special astronomical character. Of no other theory, I venture to say, than that which combines these two strange but most marked characteristics of the Egyptian mind, can this be said.

THE "SATURDAY REVIEW'S" COMET.

CASSIO.—Why, *this* is a more exquisite song than the other.

THE effect of [Mr. Proctor's] note of alarm was promptly seen in the queer homily in the *Spectator*, of which we have already taken notice, having for its text this prophecy of Mr. Proctor, as one "of whose astronomical authority and ability nobody doubts." Upon this undoubted fact were founded some characteristic speculations as to the moral attitude with which the inevitable doom is likely to be met as the day draws near, which day Mr. Proctor would doubtless be able to fix with still greater precision. The Menacing Comet has, it appears, since been "scratched" in the pages of KNOWLEDGE. But the title of the essay, and what is drawn out as the line of proof, epitomised by us [query, *as* epitomised by the *Saturday Review*—ED.] shows to our mind an unmistakable desire, however it may be sought now to turn it off as a playful freak of science, to make the reader's flesh creep. *How far*, indeed, so many of us as are not dead with fright *at the indications vouchsafed them of the coming catastrophe does not yet appear.* Nor does it very much matter. All must soon be over. "Mr. Proctor's name will have one instant of lurid fame, in which that and everything else connected with our corporeal life will expire." In a few weeks—possibly in a few days, or even hours, in the words of Mr. Proctor—the sun, excited for a while to intense heat and splendour, will resume his usual temperature, his usual lustre; but there will be no one to bask in his genial beams, no popular science teacher to tell of all he has done or is henceforth to do. [And no *Saturday Review* to give treatises on science for review to "society writers," reduced to the abject necessity of "stealing their thunder" from the authors they criticise.—ED.] It may yet be that some spirits of a bolder and more hopeful turn less trustful of popular science prophets, may bethink them that out of the myriads, not to say millions, of these menacing bodies that are said to circulate round the sun with the same chance of precipitation into his mass, *it would be odd if in the whole range of historical, or, let us say, geological time, such a catastrophe had never taken place before.* Yet, whether any such dreaded crash has come off or not, here at least we are. There has been, indeed, a strong impression that *the unwonted disturbance of the sun's surface, noted at once by Mr. Carrington and Mr. Hodgson, on Sept. 1, 1859, was due to the inrush of a comet* ["two meteors" it should be. ED.] *into the sun*; yet nothing came of it beyond a slight extra tremor of the galvanometer needles at Kew and elsewhere. Comparing the stupendous mass of the sun with the utmost material volume that can be assigned *to these filmy volatile portents of the sky, what great accession can we suppose any one among them to be singly capable of bringing to the vast cosmical centre of light and heat?* Any particularly nervous person may as well fling a pinch of snuff into the fire, and see how it affects a thermometer upon the opposite wall. After all our advance in scientific observation and theory for the last fifty years, are we really nearer to any definite knowledge of the material constituents or the physical conditions of these mysterious wanderers of our system? "What are comets made of?" asked a French lady of the most distinguished savant of the time. "Madame, I do not know." "Then what is the use of being an Academician?" "Madame, that I may be able to say I do not know." *It would never do for an oracle of popular science to have it thought there was anything he did not know.*—*Saturday Review* for March 18, 1882.

[The reader may find it interesting to compare the following five extracts, respectively, with the five italicised passages above.—ED.]

Supposing there really is a possibility that our sun may one day, through the arrival of some very large comet travelling directly towards him, share the fate of the suns whose outbursts I have described above, *we might be destroyed unawares, or we might be aware for several weeks of the approach of the destroying comet.*—From "Myths and Marvels of Astronomy," 1877, by the Editor.

If among the comets travelling in regular attendance upon the sun there be one whose orbit intersects the sun's globe, then that comet must several times ere this have struck the sun, raising him temporarily to a destructive heat. Such a comet must have a period of enormous length, for *the races of animals now existing upon the earth must all have been formed since that comet's last visit,* on the assumption, be it remembered, that the fall of a large comet upon the sun—or, rather, the direct passage of the sun through the meteoric nucleus of a large comet—would excite the sun to destructive heat. We may fairly believe that all comets of the destructive sort have been eliminated.—From "Myths and Marvels," 1877, by the Editor.

I am not sure but that *we may regard the meteors which seem to have fallen on the sun on Sept. 1, 1859, as bodies travelling in the track of the comet of* 1843, just as the November meteors, seen in 1867-8-9, &c., until 1872, were bodies certainly following in the track of the telescopic comet of 1866.—From "Myths and Marvels," 1877.

The dread of the possible evils which might accrue if the earth encountered a comet will possibly be diminished by the consideration of the *extreme tenuity of these objects.*—The Editor, in KNOWLEDGE for *November* 11, 1881.

To the astronomer, the appearance of so many comets—some of them large ones—has been full of interest, because he hopes by the application of the new methods of research discovered within the last quarter of a century to solve some of the *mysteries with which the whole subject is still fraught,* despite a number of interesting discoveries which have recently been made.—The Editor, in KNOWLEDGE for *November* 4, 1881.

TRICYCLES IN 1882.

BY JOHN BROWNING.

SEVERAL correspondents have requested me to give my reasons for preferring the machines I have named as in the foremost rank in the previous article, and as I cannot reply to them individually, I will endeavour briefly to furnish such information here.

First, then, as regards the Rucker Tricycle. This is the only open-fronted, rear-steering machine in which the pedals are so placed that if the rider stands on them he adds to the stability of the machine by keeping the hind steering-wheel more firmly on the ground, while the pedals, being well under the rider, give the rider more power, particularly in hill-riding, with less fatigue. Another

great advantage the machine possesses is that it can be steered by either or both hands, and having a chain to each wheel, it is a *true double-driver*. In most of the open-fronted, rear-steering machines there is a tendency, when travelling down-hill, from the weight of the rider being in front, for the hind wheel to leave the ground. When this is the case, the power of steering is lost just when it is most wanted.

The Monarch I have selected for its originality, good workmanship, portability, and lightness. These good qualities are obtained by dispensing with all levers, chains, cog-wheels, or other gearing. The pedals are in the form of a stirrup, and are hung on the cranks.

There are no more bearings in the Monarch than there are in a bicycle, and all bearings are ball bearings. The brake acts on both wheels, and it is a true double-driver. It would be better if the brake acted on the hubs of the wheels, or on drums, instead of on the tires, and some persons would prefer the machine if the wheels could be made larger than forty inches, but small wheels are stronger, safer, and lighter than large ones, and a machine with small wheels can be driven much easier than one with large ones against that bugbear of tricyclists, a strong head wind.

The National Tricycle I mentioned for its lightness and excellent workmanship, but the lightness seems to have been exaggerated, for a machine which was supposed to weigh 65 lbs. proves to weigh 80 lbs.

The Improved Omnicycle solves in the best manner yet contrived the application of speed-changing gear to a tricycle. It gives the rider a command of three different speeds, which can be changed without stopping the machine. The gearing is strong, and is not likely to get out of order. It seems to me probable that the power of applying changeable gearing to a tricycle, so as to adapt it to the varying inclinations of the road or their condition, will, when the machine has been perfected, cause it to be generally preferred for road-riding, or at least for touring, to the bicycle.

In my former paper I have described the Humber tricycle, and spoken of it as the fastest machine yet made. I have, therefore, been asked if I strongly recommend this machine. I reply that I do for speed, but I think this has been gained at a sacrifice of comfort, and, to a certain extent, of safety. The Humber has no foot rests, and this greatly detracts from the comfort and safety of the rider when running downhill.

The new Rotary Coventry has the chain in the centre. It is one of the lightest machines made. The throw of the cranks can be altered at pleasure. It makes only two tracks, is an open-fronted machine, has ball-bearings to all parts, and is the most perfect of all machines in steering. This is due to the fact that it is steered by means of two wheels, which are moved by one rod in opposite directions.

The machine can be turned round in a circle one foot less than its own length. None of our fastest riders have ridden the Coventry Rotary in races. If they would do so, I think the machine would prove among the fastest machines made, probably coming very close to, and possibly equalling the Humber. The Coventry Rotary is an excellent luggage carrier, one of the most important advantages that can be possessed by a tricycle.

Scarcely a day passes without my being asked: "Which is the best tricycle?" It would be about as easy to answer another question. "What is the best thing to have for dinner?" So much in this case would depend upon the eater, and in the first case on the rider. Still a few hints may guide intending riders in the choice of a machine.

For a man about 9 stone I would recommend a special Salvo, with 46-in. driving-wheels, geared down to 40, if the country is hilly; or, perhaps better still, a Monarch, with 36-in. driving-wheels. A rider who wishes to obtain speed apart from other considerations, should select from the Humber, the Premier, or the Rucker, the last being probably the best of the three, because it is a true double-driver, and has a double break. For hill riding the Humber, the geared-down Devon, the improved Omnicycle, or the Rucker are all good. For a very heavy man the Salvo, with 48-in. wheels and 1-in. tires, is excellent, and so would be many other machines, if the makers were asked to make them equally strong for a specially heavy rider.

THE DUCHESS OF CONNAUGHT'S ILLNESS.

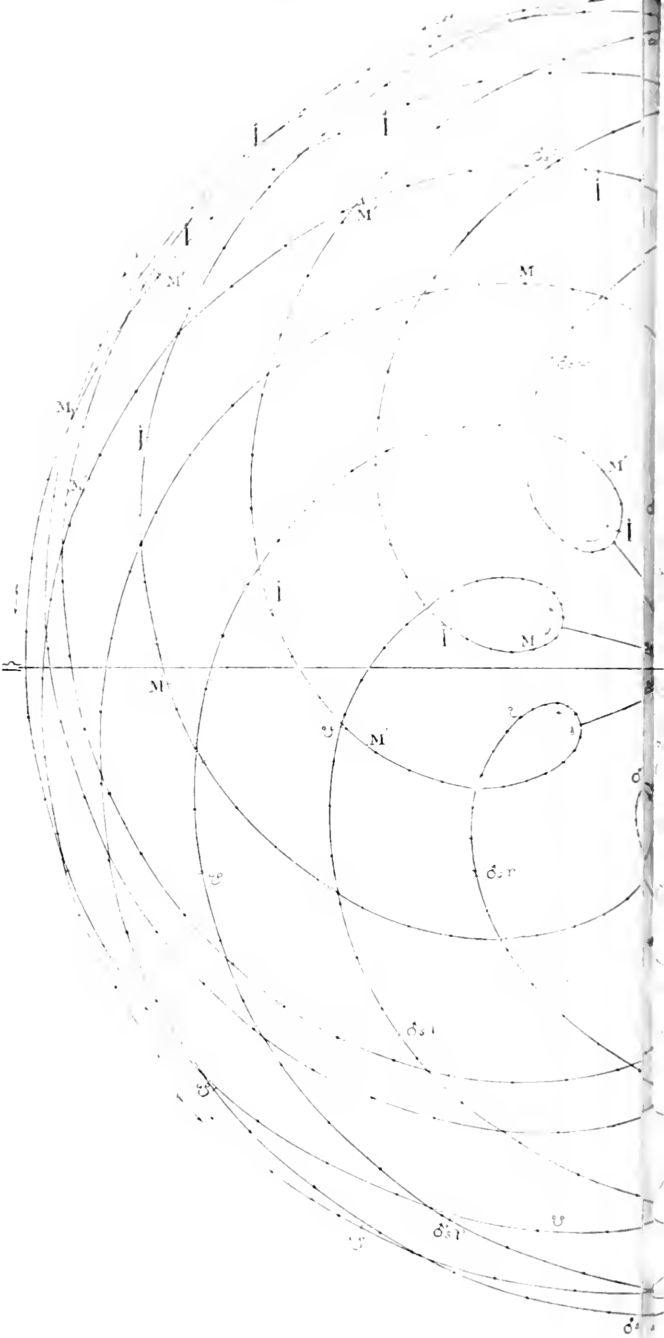
THE *Lancet* publishes the result of an inquiry into the sanitary condition of the Duke of Connaught's house at Bagshot-park. Dr. W. S. Playfair states that he writes this notice at the suggestion of the Duke of Connaught, as a matter of general interest to the medical profession, and in the hope that this instance of the danger which may arise from faulty construction and workmanship in the system of drainage may direct attention to the paramount importance of questions of this kind. These defects are believed to have led directly to the Duchess of Connaught's late serious illness, from which she is now happily entirely convalescent. It appears that the present house at Bagshot-park is not that long occupied by the late Sir James Clark, but an entirely new building, recently erected at a cost of from £30,000 to £40,000. Considerable pains were taken in the arrangement and ventilation of the drains, but not only was the system adopted in itself defective; the work was in many instances so carelessly carried out that it is surprising that in this new and costly mansion graver results did not follow. As a matter of fact, offensive smells had long been perceived about the house, but no one suspected their origin, or realised the danger they were likely to cause. Many of the inmates, however, had suffered from various forms of indisposition, such as sore throat, diarrhœa, and a general sense of heaviness and *malaise*, and these generally affected new-comers. About a fortnight after the accouchement of her Royal Highness the Duchess of Connaught, symptoms ominous of blood-poisoning presented themselves. Happily these were promptly recognised by Dr. Playfair, who has bestowed especial attention on the treatment of the puerperal state, and the only efficient means of cure was adopted, namely, instant removal. It is, however, with the cause of the perilous occurrence of incipient blood-poisoning that Dr. Playfair's statement is chiefly concerned. It may at first sight seem incredible, but it is the fact that the elaborately constructed system of baths, drains, and waste-pipes communicated directly with the soil drains of the building, and by a Machiavellian policy, which would appear to be the pastime of modern builders, pipes which ought to have been stopped were left open, and the poisonous gas which rises from fœcal accumulations was actually conducted, in sundry ingenious and wonderful ways, into the very apartments it was particularly desired to preserve from possible infection. The professedly "sanitary" constructors are, if possible, the least to be trusted. It would seem to be the common practice of these remarkable persons to ventilate the house-drains, and therefore, of course, the sewers generally, into bedrooms. We are repeatedly hearing of this piece of wantonness. Probably, in six cases out of ten the waste-pipes of baths and cisterns, which are never cleared by a current of water, except at the rare moment when a bath or cistern "runs over," communicate directly with a soil-pipe. It is well that the opportunity offered by this important instance of a wide-spread peril has been so ably utilised. The state of affairs at Bagshot, which Dr. Playfair has been not merely permitted, but commissioned, to disclose, may be taken as typical of that which prevails probably throughout the class of modern and what are misleadingly called sanitary houses. Dr. Playfair says he has long been satisfied that sanitary defects have often much to do with grave forms of illness after child-birth, the origin of which illness cannot otherwise be traced. He mentions two or three cases in which exposure to sewer-gas, as he believes, caused puerperal disease; they were cured by removal. Fortunately, in the case of the Duchess of Connaught, her symptoms did not commence for more than a fortnight after her confinement; and her removal, too, was immediately followed by a most remarkable and instructive change for the better.

VIVISECTION.—It is the notion of such absolute despotism as shall justify, not merely taking life, but converting the entire existence of the animal into a misfortune, which we denounce as a brutal misconception of the relations between the higher and the lower creatures, and an utter anachronism in the present stage of human moral feeling. A hundred years ago, had physiologists frankly avowed that they recognised no claims on the part of the brutes which should stop them from torturing them, they would have been only on the level of their contemporaries. But to-day they are behind the age; ay, sixty years behind the legislature and the poor Irish gentleman who "ruled the houseless wilds of Connemara, and had the glory of giving his name to Martin's Act. How then claim for a "free vivisecting table" may be looked back upon a century to come we may perhaps foretell with no great chance of error. In his last book, published ten years ago, Sir Arthur Helps wrote these memorable words: "It appears to me that the advancement of the world is to be measured by the increase of humanity and the decrease of cruelty. . . . . I am convinced that if an historian were to sum the gains and losses of the world at the close of each recorded century, there might be much which was retrograde in other aspects of human life and conduct, but nothing could show a backward course in humanity" (pp. 195, 196). As I have said ere now, the battle of Mercy, like that of Freedom,

once begun,
Though often lost, is always won.

—Miss F. P. Cobbe, in the *Cornhill Magazine*.

THE PATH OF MARS



LOOPED PATH OF A PLANET.

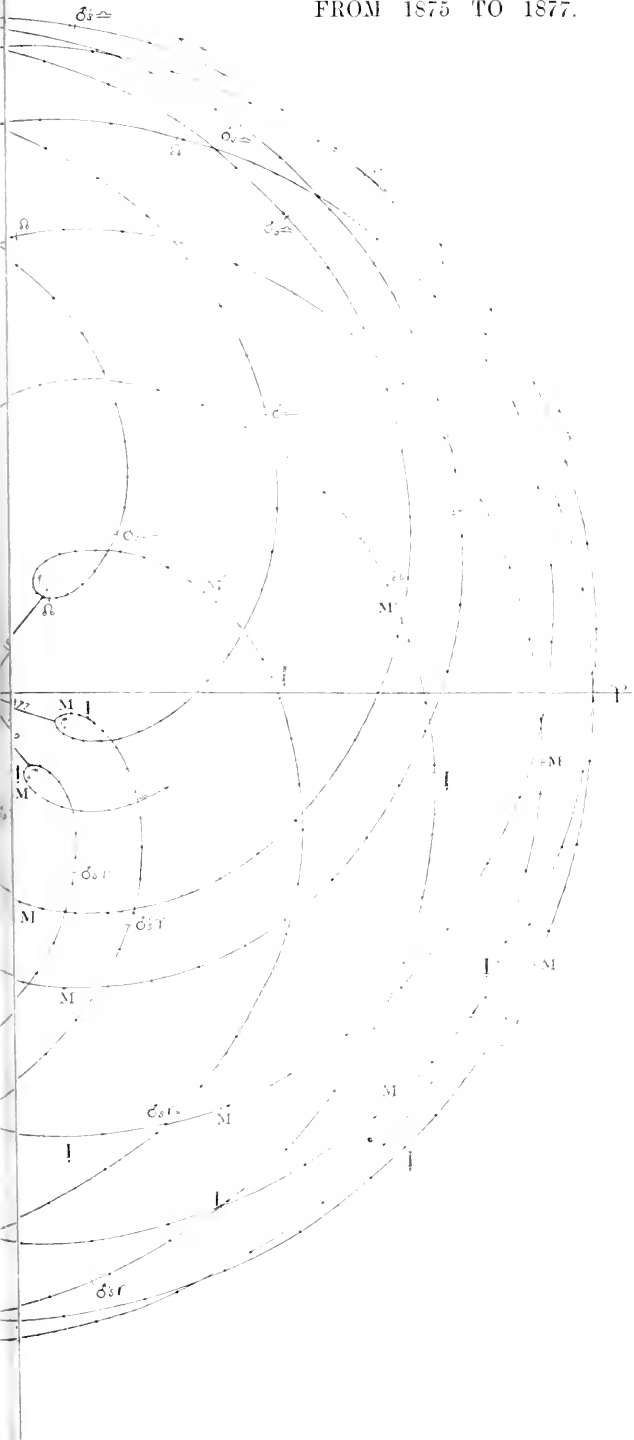
By the Editor.

MANY even of those who have read the usual descriptions of planetary motions, in our text books of astronomy, are perplexed by the way in which the planets pursue

> Their wandering course, now high, now low, then hid,
> Progressive, retrograde, and standing still.

Mars, Jupiter, and Saturn, during the last few months have given striking illustrations in the skies (as indicated in our maps) of their strange, and at first view, fantastic and irregular motions. Mars, in particular, traverses a singularly devious course upon the background of the starlit heavens. It has seemed to me that it would be interesting to exhibit the real course of this planet, the one of all the sun's family whose path, with reference to the earth, has the most complicated form. Of course, in reality this planet travels around the sun in an ellipse which is almost circular in form, though considerably eccentric in position. The earth also pursues an elliptic path, smaller in size, still more nearly circular in form, and much less eccentric. But viewed from the earth, the planet Mars, in consequence of the combination of these two circling (but not strictly circular motions), travels on such a looped path as is shown in the accompanying map. Here the planet's position, as viewed from the earth (his geocentric position, as it is called), at the successive oppositions (or times of nearest approach to the earth), is shown by the small dot at the end of the dated radial line. Then, at successive intervals of ten days, measured forward and backward from the time of opposition, Mars has the positions indicated by the successive dots. (Of course, there is a place in the outermost

FROM 1875 TO 1877.



part of each whorl where these ten-day dots meet without an exact ten-day interval: this, however, is unimportant, as in these parts of his geocentric path Mars is invisible. At the proper places along the planet's looped geocentric path are shown the places where Mars is in perihelion (M), aphelion M′, at a rising node (or crossing the plane of the earth's orbit from north to south), (☊), at a descending node (or crossing the plane of the earth's path from south to north), (☋), the place where he attains his greatest distance north ($\overset{\wedge}{\text{I}}$) and south ($\underset{\vee}{\text{I}}$) of the plane of his orbit: the place where Mars is at the point of his orbit corresponding to the vernal equinox (beginning of spring) of his northern hemisphere, marked ♂'s ♈, and the corresponding point for the autumn of Mars, marked ♂'s ♎.

The scale of the drawing is the same as that of my picture of the orbits of the terrestrial family of planets (Mars, Earth, Venus, and Mercury), in the "Encyclopædia Britannica," viz., fifty million miles to the inch, and on this scale the lines I, I, &c., indicate the greatest distance attained by Mars north and south of the plane of the ecliptic. The northerly displacement, it will be seen, is the greater.

The path of Mars must be regarded as passing above the plane of the paper, at a point marked ☊, gradually attaining its greatest height (indicated by the length of the "I") above that plane at the point marked $\overset{\wedge}{\text{I}}$; gradually returning towards the plane of the paper, which it crosses again at a point marked ☋: then attaining its greatest distance below the plane of the paper at the next point marked $\underset{\vee}{\text{I}}$; whence it returns gradually to the plane of the paper at a point marked ☊; and so on continually.

CHALCEDONY CONTAINING LIQUID WITH A MOVABLE BUBBLE.

By the Rev. Henry H. Higgins.

Two specimens were brought from Monte Video by Mr. Philip Rathbone. The larger piece contains not less than an ounce of liquid, with a large bubble: the smaller seems to have more liquid in proportion. A third specimen is broken, showing the walls of the chamber to be, in the thinnest part, not more than $\frac{1}{10}$ of an inch in thickness. The mineral seems to have been recently discovered, no mention of it being made in Dana's text book, 1880. F. W. Rudler, Professor of Mineralogy in the Royal School of Mines, informs me that he has not seen any published description of Chalcedony enclosing water with a movable bubble. I am not a mineralogist, and make the following conjectures with much diffidence. In some cavity deep in the earth, and probably under great pressure, occurred a hollow containing a small quantity of water, above the boiling point, but kept liquid by pressure, and supersaturated with silica. On any diminution of the temperature, which might occur extremely slowly, the fall of one degree occupying perhaps long ages, crystallisation would set in on all the sides of the hollow holding the liquid, and at the same time would be formed on the surface of the water, a crystalline pellicle, from which crystals would shoot downwards and inwards. Thus would be formed a cavity enclosed on all sides and filled with liquid. Layers of opalised quartz, chalcedony, might now be deposited externally upon the roots of the crystals, forming a cell with walls impervious to water or gas. Further diminution of temperature would enable minute quantities of gas contained in the water to assume a gaseous form and unite in a bubble. In the broken specimen, the walls distinctly show the radial disposition of the crystals forming the lining, and the stalagmitic character of the outer rind in which the chalcedony is deposited in layers like the laminæ of an onyx or an agate. In the large specimen, the outer surface is very interesting. It is covered with low tubercles arranged in circles $\frac{3}{4}$ in. in diameter, each circle having within it several similar concentric circles. The cavity is not strong enough to withstand any considerable bursting pressure from within. It is possible, however, that the shrinkage of the contents of the cavity from cold may have maintained the equilibrium between outside and inside pressures. Although the tank-forming chalcedonies do not appear to have been described, quartz crystals with minute drops of movable bubbles enclosed have long been common in all collections of minerals.

John W. Judd, F.R.S., in his admirable work on volcanoes, gives the best, almost the only description I have seen of them. The liquid may be water, a hydrocarbon, or even carbon dioxide. That it may be the *last* has been proved by spectrum analysis, and by the test that when the crystal is heated to 86° or 90° Fahrenheit, the bubble disappears; that temperature being for carbonic dioxide, the critical point above which no pressure can keep it in a state of liquidity. Still more remarkable is Mr. Judd's account of microscopic cavities containing liquid in which the bubble is in constant motion, pursuing a spiral track from end to end of the cavity. For the only attempt at explanation of this motion yet given, Mr. Judd's work may be consulted, and no one will regret having read the book from the first to the last page. All the cavities of which Mr. Judd speaks are very minute, and it is evident that the tank-forming chalcedonies are constructed in some manner to which the quartz crystals containing drops can afford no sufficient clue, though as in my own case, Mr. Judd's beautiful researches may lead to more or less reasonable guesses. Through the kindness of Mr. Rathbone, the smaller specimen and the equally interesting broken one have been placed in the Liverpool museum.

MODERN DRESS.

Eccentricities of costume have in all ages formed a prominent subject on which satirists have expended the magazine of their wit; and in the present day no inconsiderable attention is given to it by those who make it a business to caricature the follies of their fellow beings. It is, however, an all but recent development that is being witnessed now in connection with the efforts made by the medical profession to awaken the public to a sense of the self-inflicted evils suffered from absurd compliance with the demands of "fashion." Nor can we feel anything but sincere satisfaction that this question has so far become a "burning" one; and that men of the highest eminence deem it part of their duty to society to issue warnings against the miserable consequences of sacrifices offered to the shrine of appearance. The physical evils of inappropriate dress are so manifest to the physiologist, the constitutional damage they entail so apparent to the physician, the miseries they create so evident to the social economist, that it is rather a matter of wonder they have so long been permitted to continue unchecked and unreformed. Now and again, indeed, individuals in the past have raised a warning voice against indulgence in the fashionable caprices of their time; but never before has a determined attempt to improve public taste and educate public ignorance in the matter of dress been made that characterises the action of the National Health Society in this respect. Recently, however, Mr. Frederick Treves, F.R.C.S., of the London Hospital, delivered a lecture before a crowded audience in the Kensington Hall, on behalf of the National Health Society, the subject selected being, "The Dress of the Period." It is gratifying to hear that unusual interest was excited by the preliminary announcements; we shall perhaps be indulging in unlicensed hopes, however, if we permit this result to create in our minds a belief to the effect that the public are at last growing alive to the harmfulness of following the dictates of fashion with the unreasoning submission exhibited in the adoption of its most outrageous demands. It is true the audience which greeted Mr. Treves's demonstrations of modern fashionable follies with applause, indicative of its approval of his denunciations, was chiefly made up of ladies whose claim to be regarded more or less as victims to the Moloch under censure was indisputable; but notwithstanding, it would be the refinement of rashness to expect from them an immediate renunciation of tight waists, cramped feet, and swathed limbs. The hold of these on the lives of those who form "society" is too secure and too steadfastly maintained to permit its being easily removed. We can trust for this happy result to nothing but a general and an intelligent apprehension of the mischief attendant on continuance of the evils which carry such disasters in their train.

The ill-consequences set up by improper dress are most familiar to medical men; and medical men necessarily, therefore, are those most competent to advocate reform in customs, to the injurious effects of which they are daily witnesses. In the particular folly of tight lacing, for instance, there is probably no practitioner who is not constantly called on to remedy the evils it produces. In young girls, who, least of all, are calculated to support the strain to which their internal organs are submitted under the cruel pressure of the corset, we have often to deal with piteous examples of the sacrifices required in order to ensure a small waist. Nor is it that they suffer only while young. During their whole after-life, symptoms referable to visceral displacement and disorganisation are of frequent occurrence; no woman, probably, who has at any time conformed to this fashion of abdomen-strapping being free from some form of gastric or liver trouble. To what extent, moreover, the craze for "an elegant figure" may carry its victim is scarcely credible, except for occasional proofs afforded at inquests and post-mortem examinations. While it is not unusual to find the liver deeply indented by pressure of the adjacent ribs and displaced deep into the pelvis, it has more than once been found that long-continued constriction of the body has resulted in hour-glass deformity of the stomach. Nor need we long hesitate to decide on the influence this vicious form of fashionable sacrifice exerts on the duration of female life, when we reflect on the prevalence among the middle and upper classes of the very diseases which would be induced by persistence in such habits. Gastric ulcer is at least three times as frequent in women as it is in men. Syncope is a common form of weakness exhibited by young women who subject their viscera to the vice-like compression of a corset; and the intestinal troubles set up by interference with the functions of the liver, together with the distributed pressure on the intestines themselves, are among the most productive sources of feminine illness. Indeed, the subject of tight lacing might with advantage be taken as the sole topic for a considerable number of lectures; and the more tellingly the evils it ensures are put before the public, the more convincingly they can be taught to perceive the fatal injury the practice is doing to the race, the more speedily and surely will it cease to be commonly indulged in. We would urge this point—that of the injury suffered through it by the whole race—with especial force. By as much as any woman undermines her own health—it matters not in what manner the mischief is done—to such an extent is she also injuring the physique that will be inherited by her children. Perhaps, by exciting the maternal instinct, more benefit will be derived than by any other means at present devised. At any rate, the proposal deserves consideration.

Apart from tight lacing, there are other evils associated with modern clothing that deserve equal attention. Space forbids more than a mention of them now, but we may instance the inadequacy of modern dress to secure either of the two important desiderata—appropriate protection and equable temperature. As Mr. Treves pointed out to his audience, a fashionably-dressed woman of to-day is all but nude about the chest and back, at most but a thin, single or double layer of material protecting these delicate and susceptible regions, while a huge mass of useless clothing is swathed about the hips and legs, and trails in abundant prodigality to clothe the

floor around. Again, the disastrous senselessness of "Parisian" shoes, high-heeled and taper-toed, needs no enforcing; but the fashion which insists on them will need a very strenuous and determined opposition ere it is finally conquered.

We are glad to be able to offer Mr. Treves our hearty congratulations, both on the admirable lecture he delivered, and on the unmistakable success with which it has been attended.—*Medical Press and Circular.*

COMPOUND PENDULUM.

A QUESTION was asked lately in KNOWLEDGE about the curves drawn by the Compound Pendulum. I have made for myself one of these instruments, by which were drawn the curves which I enclose. The machine was made as follows:—

I procured two hollow brass rods about 3 ft. long. At 6 in. from the top of each a steel pin 2 in. long is driven through, and filed on the under side to a knife edge, to form the fulcrum for the pendulum. A plug of ivory is fixed in the top of each rod, having a small conical cavity drilled in it. On each rod are two cylindrical leaden weights, which slide upon it, and can be fixed at any height by a collar and screw. Each weight is divided, because it is better sometimes to use half the weight on eah rod. (See Fig. 1.) Fig. 2 shows the top of the table. A and B are two pieces of mahogany, 1 ft. by 2 in., and 1 in. thick, morticed together at right angles, and screwed to the top of a tripod stand, C. Into the end of each are fixed two brass screws, having a circular groove turned round their necks, forming the notch in which the knife edges of the Pendulums swing, and by turning the screws, the distance from the centre can be adjusted. On the top of this is fixed a small box, 9 in. by 5 in., and $3\frac{1}{2}$ in. high, so that its centre is just over the point where the arms join. This box forms the table on which the paper is laid, held down by springs. The pen is carried by two arms of thin mahogany, jointed by a metal tube fixed tightly in the end of one, the other working round it. The pen is fixed in this tube. I use a goose quill. I have tried glass tubes, but they are difficult to make and soon get clogged. Enough ink is held in the pen by a tongue of quill inside it, almost touching the point. In each arm, at the same distance, are the Pendulums, and from the centre of the table is a small screw filed to a sharp point, which works in the conical hole in the top of each pendulum rod. Thus there is very little friction, except that of the pen on the paper, which can be regulated by balance-weights. The table must be levelled by a spirit-level, and the arms which carry the pen horizontal. The Pendulums are held up by strings towards a point under the centre, and started by a trigger, by which they can be released, either simultaneously, which gives a pointed curve, or one Pendulum at any fraction of a swing before the other, by which a continuous or looped curve is produced. This would be better done by an electro-magnet, which I purpose to try. I send a set of the curves representing the intervals of the musical scale, which are as follows:—

Do.	Re.	Mi.	Fa.	Sol.	La.	Si.	Do.
1	9	5	4	3	5	15	2
1	8	4	3	2	3	8	1
Unison.	Second.	Major Third.	Fourth.	Fifth.	Sixth.	Seventh.	Octave.

That is, if one Pendulum vibrates 9 times while the other vibrates 8, they will draw a curve similar to that which can be produced optically by the vibration of two tuning-forks, whose vibrations have the same ratio, $\frac{9}{8}$, which in music is called a Second. Equal vibrations give Unison, a curve varying from a straight line to an ellipse and a circle. If one vibrates twice as fast as the other, for which it must be $\frac{1}{4}$ of its length, an octave is drawn, and so for the other curves, according to the above ratios.

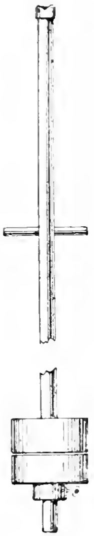


Fig. 1.—Pendulum.

($\frac{1}{4}$ of real Size.)

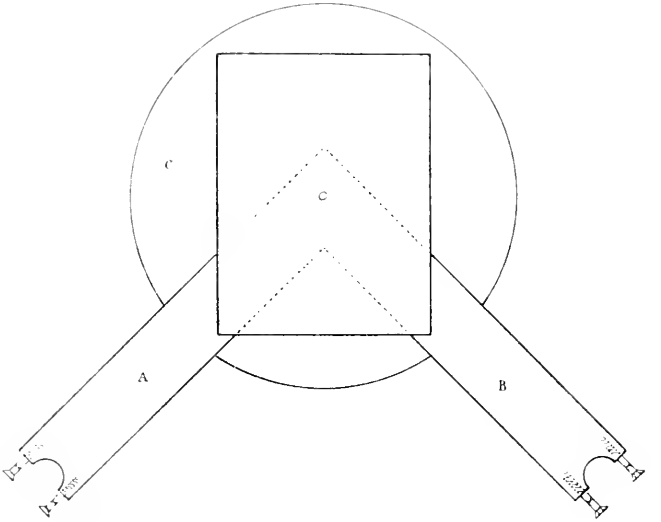
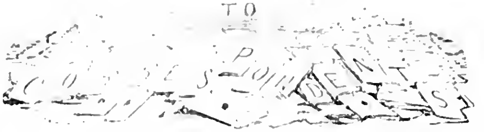


Fig. 2.—Top of Table.

There are other intermediate intervals, the curves of some of which I have obtained. One of the rods must be jointed, so that the lower half can be removed, in order to produce the higher ratios of vibration. I shall be glad to give an account of all the different curves, and the way of drawing the various forms of each, if you think it would interest any of your readers. A description of the curves, and the mathematical and musical principles involved, is given in Deschanel's "Natural Philosophy," Part 1, page 848, in Jamin's "Cours de Physique," vol. ii., p. 608, and in Ganot's "Physics," p. 207, with illustrations of Lissajou's experiment for showing the curves optically by tuning-forks furnished with small mirrors and vibrating at right-angles; but I have not found any account of the method of drawing the curves by the Compound Pendulum.

E. LUXMOORE.

HEALTH OF NAVVIES.—An experience of twenty years as a medical man in India, enables me to inform "F. C. S." that betel chewing certainly does not prevent the natives from suffering severely from malarious intermittent and remittent fevers. It is an abominable habit—the lime used destroying the gums and teeth, though it, of course, supplies some want to the system, as tobacco-smoking does.—B. M., F.R.C.S.



Letters to the Editor.

The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with their writers. All communications should be as short as possible, consistently with full and clear statements of the writer's meaning.]

All Editorial communications should be addressed to the Editor of KNOWLEDGE; *all Business communications to the Publishers, at the Office, 74, Great Queen-street, W.C.*

All Remittances, Cheques, and Post-Office Orders should be made payable to Messrs. Wyman & Sons.

∴ All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.

All Letters or Queries to the Editor which require attention in the current issue of KNOWLEDGE, *should reach the Publishing Office not later than the Saturday preceding the day of publication.*

(I.) Letters to have a chance of appearing must be concise; they must be drawn up in the form adopted for letters here, so that they may go untouched to the printers; private communications, therefore, as well as queries, or replies to queries (intended to appear as such) should be written on separate leaves.

(II.) Queries and replies should be even more concise than letters; and drawn up in the form in which they are here presented, with brackets for number in case of queries, and the proper query number (bracketed) in case of replies.

(III.) Letters, queries, and replies which (either because too long, or unsuitable, or dealing with matters which others have discussed, or for any other reason) cannot find place here, will either be briefly referred to in answers to correspondents, or acknowledged in a column reserved for the purpose.

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

"God's Orthodoxy is Truth."—*Charles Kingsley.*

Our Correspondence Columns.

SEEKING AFTER A SIGN. ERRATA.—REFLECTING TELESCOPES.

[316]—Equations, in some fashion mentally associated by me with curves, are, I am amused to see by your Answers to Correspondents (p. 365), now applied to tavern-signs. As a humble contributor towards this fresh application of them, I would say that the Pig and Whistle = (a) the Pige Washael (the Maiden's Greeting; *i.e.*, the Salutation of the Virgin) of the Danish; or (b), the Peg in Wassail. The Bear and Ragged Staff = the heraldic cognizance of the famous Earl of Warwick ("The King-maker"), who inherited it through the Beauchamps. I regret that I am not up in the Magpie, and am—in cricketing parlance—"stumped" by his adjunct.

Albeit, the heading, "Diameter of the Moon's image in the focus of a 42-inch object-glass," appears to my letter (293) on p. 387, the paragraph to which it refers seems to have dropped out. [The correction made elsewhere; heading should also have been omitted. —ED.] The compositor has not been very kind to me in the letter referred to. For example, in my first paragraph, where "Sheppey flints" should be "Sheppey fruits;" and "sulpherised iron," "sulphuret of iron." By-the-bye, in the new nomenclature this is called Ferric sulphide.

Mr. Jones (Query 281, p. 390) had very much better get a silvered glass reflector, than a metal one.

A FELLOW OF THE ROYAL ASTRONOMICAL SOCIETY.

THE CAT'S-EYE TIMEPIECE OF THE CHINESE.

[317] The paragraph on this subject (p. 313) reminds me of what Herodotus says (Book 1., 10) about the cat among the Egyptians: "They say that the male cat changes the shape of the pupils of his eyes according to the courses of the sun; for, in the morning, at the rising of the god, they are dilated; in the middle of the day they become round; and about sunset become less brilliant. Hence, also, the statue of the god in the City of the Sun is of the form of a cat." In the Egyptian Ritual (ch. 17), one of the transformations of the solar god is into a cat. As such, he "makes the likeness of Seb," or Time. In Egyptian, the cat is Ma, Man, or Mai (an inner African name for the animal, by-the-bye, as Mai in Undaza, with Imrinuts in other dialects) and the same word signifies the eye, sight, and to see; Chinese Meih, to see, to seek with claws! Mā (*l. c.*) the name of the cat, also means truth, that which is true, Ma being the Goddess of Truth and Justice. Now time is that which is true, nor shall we get a more appropriate origin for the word time, or tempus, than Ma the true—she who, as *Tema*, supplied the Greek Themis. Tema is *the* Mā, the true, the manifestor of truth; but it is equally *the* cat, whose eye was held to tell true in the matter of time, and whose type was assumed by the God Ra as the likeness of Time, or Seb-Krouns. Lastly, "Adene is the name of the Mother-goddess of Time" (Ritual, ch. 165).

GERALD MASSEY.

TELESCOPES, &c., ON THE THREE YEARS' SYSTEM.

[348]—It has been suggested in these pages that it would be a boon to many an honest science lover if such goods as microscopes, telescopes, &c. (good ones of which are far too expensive for a great many who would much like to become possessed of them, to purchase), if opticians could make it profitable to themselves to sell their instruments on a plan after the three years' system. I know an excellent firm, who are willing to adopt the system, if they can devise a plan whereby the risk of loss and nonpayment from dishonest persons may be reduced to a minimum. Since scientific apparatus is so much more portable than pianos and the like, which are now sold on the three years' system (and apparently with profit), the risk is certainly greater; but surely there is a plan, if only hit upon, whereby honest persons may be benefited by the three years' system, and the attempts of dishonest persons to ill-use it thwarted. It is for this reason I write to ask the help of the readers of KNOWLEDGE to devise a plan whereby it may be safe for opticians who are willing to adopt such a system of trading. Will readers, with the Editor's permission, kindly think and suggest something satisfactory?

F. C.

NEOLITHIC MAN.

[349] I have read with interest Mr. Grant Allen's article on Neolithic man in Britain.

Why does he use the form Euskar*ian*? If he does not say *Euskal*, why not follow the Basques themselves and say *Euskarran*? —*arra* being the termination which means people, as *Espanarra* = *Español*.

Is there good ground for stating that the Euskarran skull is long and narrow?

I have no acquaintance with the stripped skull, but, as clothed with flesh, &c., it gives both to the eye and to the hand the impression of great roundness, and it is certainly neither so long nor so narrow as my own head-piece, though I am descended in large part from black Celts of Southern Ireland, and possess, along with other physical characteristics of the race, a complexion some shades darker than that of nine Basques out of ten.

M. S.

ARE WOMEN INFERIOR TO MEN?

[350]—A friend has just sent me your very interesting journal for December, and I have read the letters in reply to M. Delaunay's somewhat ungallant pamphlet on womanhood. In looking back at the past, it is easy to see that man alone possessed any real incentives for the exercise of the intellectual faculties. It was his to fight, to exercise his utmost ingenuity, to hunt, to outwit his foes, to protect his family, to win a mate for himself; and in all this long struggle into the very moderate degrees of civilisation attained by the great nations of the world, still always engaged in warfare, woman had naturally to fill a very secondary place.

But as a higher and more refined view of life, of the purposes of man's existence in the world, of the relations between the sexes, is recognised, so will the survivals of these conditions of the past which meet us in the form of legal and social disabilities, tend to become unsatisfactory, and finally obsolete. As soon as men and women both recognise that they exist as such by the reign of a great and most useful law—one, indeed, without which progress would have been impossible—and that woman was not created specially for man, they will realise that freedom for woman is the best and safest guide for the future. Men and women represent different principles, meant for complete development; and in a very high state of civilisation, the qualities both possess are required in nearly all things in active co-operation. Man is the tree, woman the flower, and, when fully understood, they are too closely united to admit the question of the latter's "inferiority."

If, in educational systems, boys and girls more frequently studied together in classes, it would do much to refine the ideas of men with regard to women in early life, and introduce healthier feelings of sympathy and friendship between them. They have at present far too few interests in common. I need hardly say I am wholly

opposed to the methods of education prescribed by "Susan G." (in KNOWLEDGE of Dec. 2), who has spoiled an otherwise good argument, so far as it goes, by her singular advocacy of "physical force." Physical force is *ceasing* to do the world's work; thought is becoming everywhere more and more the mighty motive-power. When the great underlying principles of nature are fully realised, and the possibilities which reside in the being of man, as nature's great representative, the freedom and equality of the sexes will no longer even be questioned. By forcing his way into the realm of thought, man has really opened the door for the attainment of perfected womanhood. SUSAN E. GAY.

VENTILATION BY OPEN FIREPLACES.

[351]—I maintain, and can prove, that the "fire-hole" which Mr. Mattieu Williams wants to stop up, admits of a room being made uniformly warm and well ventilated at a small cost, better than any other means known at present. But it is essential that the room have a direct air-supply. If this be delivered into the room by a tube or tubes of not less than 18 square inches sectional area, at, or near to, the mantel, and directed towards the ceiling, the "fire-hole" will draw from the upper part of the room, the occupants will have a continuous supply of fresh air, and the floor will be warm. There should be the means of warming the direct air-supply; several forms of open grate are made which do this. In the winter there is a clamour for warmth, but during the greater part of the year it is ventilation that is wanted. F. LLOYD.

[So far as my experience goes, we suffer much more from bad ventilation in winter than when the weather is tolerably warm. ED.]

THE PHYSICAL APPEARANCE OF SATURN.

[352]—In your "Other Worlds than Ours" you show, by the violent changes in Jupiter's cloud-belts, that that planet is most probably a glowing mass, bubbling and seething with the intensity of the primeval fires. You state in the same work that the belts of Saturn resemble those of Jupiter in general shape and in colour, and also that his belts change in aspect much as Jupiter's have been observed to do. The great diversity in the appearance of Jupiter's belts in numerous drawings seems to quite bear out your conclusions as to that planet. But I am anxious for further information before I can hold your views respecting Saturn with the same confidence. Almost every picture of the latter planet (including the exquisite painting in your "Other Worlds") represents it with belts almost as even in outline as if they had been turned in a lathe—an appearance not at first sight, at all events, indicative of violent atmospherical disturbances. In "Guillemin's "Heavens," however, there is a drawing of Saturn with irregular belts, as seen by Bond in 1848, and if the majority of the pictures of the planet possessed this characteristic, as those of Jupiter do, I should feel no difficulty in adopting your conclusions about Saturn's condition. If you would kindly clear up this matter for me, I am sure many of your readers would be greatly interested. MORE LIGHT.

[Remembering that Saturn is so much farther away than Jupiter, and so much more faintly illuminated, it is not surprising that the same telescope which shows irregularities in Jupiter's belts will reveal none in Saturn's. Speaking roughly, we receive from a square mile near centre of Saturn's visible surface only about a sixteenth of the light we receive from a similar portion of Jupiter's. But with high powers, not only are irregularities seen, but rapid changes have been witnessed, in the Saturnian belts.—ED.

A NEW FORM OF ELECTRICAL ACCUMULATOR.

[353]—Seeing an account of Faure's Accumulator in this journal (No. 8, page 158), perhaps the following will not be uninteresting to the readers of KNOWLEDGE:—

Mr. Henry Sutton, of Ballarat, Victoria, has invented a new form of electrical accumulator, consisting of a copper cell containing an acid solution of sulphate of copper (blue stone), in which is immersed a plate of amalgamated lead, but not in contact with the copper. On connecting the accumulator with a battery or dynamo-electric machine, the copper solution is decomposed, metallic copper being deposited on the copper cell, and the lead plate is coated with peroxide of lead. When the solution becomes colourless, the cell is ready for use.

This cell is much smaller than a Faure or Planté cell of the same power, is very constant, and the inventor has generously placed it at the disposal of the scientific world free from all patent rights.

A cell, 6 inches square and 2 inches wide, was exhibited by Mr. E. Davis, at the Liverpool Chemists' Association, Feb. 2, which heated to whiteness and finally fused a thin platinum wire, and also worked a small Ruhmkorff's coil. (*Vide Pharmaceutical Journal.*) H. P. COOPER.

THE RADIOMETER.

[354]—Your correspondent, Mr Gladstone (298), is, apparently, not aware that the motion of the radiometer is due to rays of heat, not light. This was, I believe, first noticed by Mr. Preece, and may be proved by the following experiments

1. Hold a sheet of thin white note-paper between the light and radiometer; the paper, being transparent, allows light to pass but cuts off the rays of heat; no movement ensues.

2. Take a sheet of very thin ebonite, which will stop every ray of light, but, being diathermanous, allows the heat to pass, and the radiometer moves round merrily.

Other similar experiments may be made by interposing a glass trough filled with sodium dissolved in bisulphide of carbon, and another with alum in water.

Wimbledon. WILLIAM IRVING PAGE.

SOCIAL INFLUENCE OF WOMAN (273).

[355]—"E. Burke" says, "Name a nation where women are debarred from social influence, and you have named one which is proportionately backward in liberty and knowledge." I take up the challenge, thus: I suppose that "E. Burke" will allow that, in respect alike of "liberty" and of "knowledge," the Athenians of old excelled their rivals, the Spartans. Yet Professor Mahaffy, in his "Primer of Greek Antiquities," writes as follows (p. 15):—"We do not find that any Greek valued her high qualities for these important duties rightly, except the Spartans. For among them alone we find the mistress of the house a person of real importance, appearing when she chooses in public, and even offering an opinion which is respected on public affairs. In cultivated Athens, on the contrary, she was only taught spinning and cooking, and what rude medicine might be wanting for the treatment of her household in trifling illness. . . . Thus the liberty of women varied from a freedom as great as need be in Sparta, to a life of seclusion and neglect at Athens." E. D. GIRDLESTONE.

THE LANK YANK.

[356]—Mr. Mattieu Williams, in his paper on "The Air of Stove-heated Rooms," in your issue of the 3rd instant, gives it as his opinion that "the lank and shrivelled appearance of the typical Yankee" is due to the dryness of his native climate, and to the further dessication (or rather increase of capacity for the absorption of water-vapour) of the air caused by stove-heating. I should like to know how he reconciles with this theory the generally well-favoured and rosy appearance of the Canadians. Surely their climate cannot be considered so much more humid than that of the Yankee, with his great extent of seaboard, as to account for their marked difference in looks. He seems, too, to have overlooked the fact that in American and Canadian houses heated by stoves there is almost universally a steam-generator of some sort (usually a pan of water on the stove), which counteracts to a great extent the avidity of the air for water. Out of doors, too, the temperature being so low (often below zero), the capacity of the air for water must be very small, and the dessicating effect scarcely perceptible.

Would it not be much more natural to account for "the lean and shrivelled aspect" of the Yankee by reference to his habit of life and, specially, of feeding, so very different from the roast beef and plum pudding of the Britisher; while, on the other hand, the Canadians, who still keep to the English way of living, have not lost the jolly and comfortable appearance of their forefathers.

I might add, too, that the Swiss and Germans, whose houses are nearly all stove-heated, and without the advantage of steam (for I never remember seeing water-pans on their stoves), are by no means a meagre or lanky race. CANADENSIS.

THE CALIGRAPH.

[357]—I can furnish "Clericus" with any information he may desire regarding an improved type-writer known as the Caligraph, with which, by the way, a recent communication of mine to Mathematical Queries in KNOWLEDGE was written.—W. W. BEMAN.

Anne Arbor, Mich., U.S.A., Feb. 23, 1882.

EYESIGHT OF DOGS.

[358]—I had not the good fortune to be a subscriber when the article in KNOWLEDGE on the near-sightedness of dogs appeared, but I have observed this peculiarity. A month ago my brother and

I were out shooting, we parted in the middle of a large field, I homeward with the dogs, he to a corner of the field where was a likely lying place for snipe. A snipe rose and he fired; one of the dogs bolted away from me, but instead of running straight to my brother, who could easily be seen, and the smoke of whose gun was still conspicuous, the dog ran back on my track till he came to where we had parted before, and then followed up my brother's track till he reached him. This dog was a retriever.—Clare.

Queries.

[335]—Tricycles.—Will Mr. Browning kindly say if he knows the "Edinburgh," and will he point out what he considers its faults?—F. H. S.

[336] "In Memoriam."—Can you kindly tell me to whom Tennyson refers in the opening stanza of this poem?

> I held it truth, with him who sings
> To one clear harp in divers tones,
> That men may rise on stepping-stones
> Of their dead selves to higher things.

I have been trying to ascertain for fifteen years past, and I thought Goethe was intended, but now Dr. Gatty tells me that the poet cannot be identified. On p. 91 (Q. 51), you ask if the stanza containing the allusion to "the crimson-circled star" is not LXXXIX.? In my edition (17th), it is LXXXVIII., but Tennyson has inserted an additional stanza about the yew-tree in the later editions, between XXXVIII. and XXXIX.—E. C. Malan.

[337]—Self-Acting Blowpipe.—While cleaning a small tin spirit lamp, I removed the brass top, together with the wick. A small quantity of spirits remained in the lamp, and a few drops stuck to the opening. On applying a light and inverting the lamp, a flame nearly three feet long rushed out with great vehemence, accompanied by a loud and gradually increasing roar. At this pleasing stage of the experiment the lamp became uncomfortably warm, and was promptly dropped, when the flame instantly vanished. Could a modification of the above be used with safety as a blowpipe? Perhaps some of your readers who are insured will kindly carry the experiment a step or two further.—J. H.

[338]—The Breaks in the Singing Voice.—Can any of your readers give a scientific explanation of the breaks in the human singing voice, of their cause and cure? Can they, at the same time, name a few really scientific works bearing on the subject.—Musicus [Manchester].

[339]—Euskarians.—Can Mr. Grant Allen tell us of any Euskarian words to be found in Welsh or Irish? Surely some—names of places, at least—should be traceable by their resemblance to Basque words.—S. C. Wood.

[340]—Calculating Machines.—I shall feel much obliged if any of your mathematical readers will kindly give a description of the various machines which have been invented, also those at present in use. I possess Palmer's disc, improved by Fuller, but find it inaccurate. How can I measure a logarithm on a circle or a plane? Is there any book on calculating machines and their construction? Does the Clearing-house make use of any mechanical contrivance for computing mileages?—Inquirer.

[341]—Can any one give me a recipe to prevent incrustation on the inside of a boiler of about 20 horse-power?—G. Roberts.

Replies to Queries.

[271]—Drawing.—"Eozoon's" query is too vague to admit of a definite reply. There can be no better elementary training for the eye and hand than to copy carefully the free-hand series, in all its grades, of the South Kensington School of Art. For simple objects, there is an excellent series published by Seeley & Co., of Fleet-street, and called, I think, "First Steps in Art." After this, Vere Foster's and J. D. Harding's are good for pencil, Leitch's for water-colour. The rules of perspective for landscape are very simple, and come almost intuitively. But there is—happily for the struggling artist—no royal road here, any more than to the acquisition of a foreign language—"French in six hours" notwithstanding.—R. S. Standen.

[274]—Foraminifera of Chalk.—Let "Eozoon" take a piece of chalk, and with a soft tooth or nail-brush brush it under water, and then wash the sediment well till the water is not coloured, when the residue will be nearly all foraminifera.—John G. Patterson.

[285]—Scientific Terms.—"Prester W." may find the following technical vocabularies of use: For geology and physical geography, "Handbook of Terms," Blackwood & Sons; about 6s. For botany, natural history, anatomy, medicine, and veterinary surgery, Stormonth's "Manual of Scientific Terms," Maclachlan & Stewart; about 7s. 6d. For botanical terms, Mr. C. Cooke's "Manual of Botanical Terms," Hardwicke. General—1, Dr. Henry's "Glossary of Scientific Terms," Smith, Elder, & Co.; 2, Dr. Nuttall's "Dictionary of Scientific Terms," Strahan & Co.; about 5s.—R. P. Porter.

[286] Electricity.—"W. H." will find the simplest and best text-book on electricity and magnetism is Noad's "Text-Book of Electricity," revised by W. H. Preece, price 12s. 6d.; published by Crosby, Lockwood, & Co., London.—S. Francis.

[287] Dr. Ferrier's formula to cure a cold in the head is:—℞ Hydrochlorate of morphia, 2 grains; acacia powder, 2 drachms; trisnitrate of bismuth, 6 drachms. Of this powder, a quarter to a half may be used as snuff in the course of the twenty-four hours. Each time the nostrils are cleared another pinch should be taken. Use a snuff-spoon, and sniff up forcibly. The above is most useful in nasal catarrh, but may not have much effect in influenza. Instead of the 2 drachms acacia I have found 1 drachm acacia and 1 drachm starch in powder less sticky, and I think better than the original formula.—A Former Sufferer.

[289]—"The Burial of Moses" is by C. F. Alexander. It is to be found in "Lyra Anglicana," published by Houlston & Wright.—B. J. Grosjean. [Also, apparently, to be found in a number of works. Answered by nineteen correspondents, who mostly give different references. We cannot insert all.—Ed.]

[291]—Razor.—Because the heated metal causes swelling or expansion of the skin and hair cylinders, and thereby brings them into closer adaptation to the cutting edge of the razor.—K.

[292]—Silver.—Precipitate the silver as chloride by adding to the baths common salt. Well wash the precipitate, and place it in a glass vessel with a few scraps of metallic zinc and a little dilute sulphuric acid (5 per cent.). The silver chloride is speedily reduced to metallic silver (a bronze powder), which can be well washed, and then dissolved in nitric acid to form the nitrate again. Another method is to fuse the chloride obtained as above with carbonate of soda and charcoal at a good red heat. A button of silver is then obtained. Fire-clay crucibles can be bought for a few pence, *i.e.*, from 1s. per dozen upwards. Griffin & Co., of Garrick-street, W.C., or any other chemical instrument makers, would supply them.—Pharmacist.

[292]—Treatment of Silver Residues.—Best method; add to the residues hydrochloric acid in excess to throw down silver chloride; warm, filter, and thoroughly wash the precipitate; dry at gentle heat. Mix with twice its weight of dry sodium carbonate, and one-tenth as much potassium nitrate, with a little borax as flax. Heat a small clay crucible (an ordinary fire may be used); when red-hot, throw in the mixture gradually with an iron spoon, urging the heat with pair of bellows. Stir to prevent frothing; when completely fused, heat a little longer, and pour out on piece of slate. It is well to purify by remelting in a clean crucible with a little borax. The ingot of silver may be boiled in dilute nitric acid till dissolved, the solution evaporated down, and left for silver nitrate to crystallise out. Metallic silver *can* be obtained without using a crucible, by setting up a galvanic action with zinc and dilute sulphuric acid, when metallic silver (black or grey) is precipitated from the silver chloride; but I must warn F. A. B. that the difficulty of avoiding organic impurities and nitro-compounds renders this method unfit for such delicate work as photography. Crucibles may be obtained for 3d. each at any chemical apparatus manufacturers, such as Townson & Mercer, 89, Bishopsgate-street Within.—C. Harris.

[292]—Silver.—This would take two much space to answer here, but F. A. B. will find full particulars in a book published by Hockin & Co., 38, Duke-street, Manchester-square, called "Practical Hints on Photography." Crucibles can be obtained from 1d. per doz. up to 2s. 6d. each.—Alpha Centauri.

[299]—Magic Lantern.—Certainly it is possible. Get to see a lantern, and copy the woodwork; but that is the *least* important part. You had better purchase "Chadwick's Magic Lantern Manual," price 1s., but don't have anything to do with the patent gasholder described in it.—Lewis Arundel.

[301]—Magic Lantern.—An ordinary 3½-in. lantern is what you want. But get "Chadwick's Magic Lantern Manual," price 1s. He is rather good on scientific projections.—Lewis Arundel.

[302].—Vegetarianism.—Mr. J. A. Ollard should obtain publications of the Vegetarian Society (sent free to any address on receipt of post-card by the Secretary, 56, Peter-street, Manchester), and "The Penny Vegetarian Cookery," or "How to Spend Sixpence," price 1d. each.—G. C.

[302].—Vegetable Food.—Sir Henry Thompson's "Food and Feeding."—K.

Answers to Correspondents.

**** All communications for the Editor requiring early attention should reach the Office on or before the Saturday preceding the current issue of* KNOWLEDGE, *the increasing circulation of which compels us to go to press early in the week.*

HINTS TO CORRESPONDENTS.—1. *No questions asking for scientific information can be answered through the post. 2. Letters sent to the Editor for correspondents cannot be forwarded; nor can the names or addresses of correspondents be given in answer to private inquiries. 3. No queries or replies savouring of the nature of advertisements can be inserted. 4. Letters, queries, and replies are inserted, unless contrary to Rule 3, free of charge. 5. Correspondents should write on one side only of the paper, and put drawings on a separate leaf. 6. Each letter, query, or reply should have a title, and in replying to letters or queries, reference should be made to the number of letter or query, the page on which it appears, and its title.*

W. S. JACKSON, C. R., and others. Seventeen correspondents already have "raved" against magic squares. We therefore must not insert anything more about them for a good while. Mr. Miles, however, will willingly, I doubt not, say where Poignard's method first appeared. THREE CHRONOS. Surely you are the ones to answer what you call the "rum watch query."—TARANAKI. The horizon sinks as we rise above the earth's surface; but the dip is very slight, even for a considerable height. The eye cannot detect it readily, even from a height of four or five miles.—A. P. M. Your letter about weather forecasts is long, but we will try to find space for it.—E. S. KENNEDY. Sayce's writings on Assyrian literature contain a good deal about the star-worship of the Assyrians. Published, I think, by Bagster.—E. KELLY. You will not find that Newcomb attributes the acceleration of the moon to the tidal wave. He says that a part of the acceleration is explained by the effects of the tidal wave in retarding the earth's rotation.—CLEMENT W. JEWITT. Cannot identify the year when "two comets came in (some time about 1858)."—J. THRUSTAN calls attention to the fact that the barometer, after being abnormally high for several weeks, fell rapidly on February 25th, yet there were no explosions in coal-mines, as predicted by Mr. R. C. Rapier.—SCRUTATOR. Let P be the length of a perpendicular let fall from one angle of a tetrahedron to opposite *face*, and let p be the length of perpendicular from an angle to opposite side of any face including the angle, and let s be length of a side. Then we know that

$$P=\sqrt{s^2-\frac{s^2}{4}}=\frac{s}{2}\sqrt{3};$$

and

$$P=\sqrt{p^2-\frac{p^2}{9}}=\frac{p}{3}\sqrt{8}=\frac{s}{3}\sqrt{6}=s\sqrt{\tfrac{2}{3}}$$

and the centre of the tetrahedron lies on each of the perpendiculars from the angles on opposite faces, at a distance $\frac{P}{4}$ from each face, of $\frac{s}{4}\sqrt{\tfrac{2}{3}}$ from any face. If $s=16$, then this distance is $4\sqrt{\tfrac{2}{3}}$.

J. MACKENZIE, M.D. Considering how much it is the doctor's interest to discountenance vaccination, supposing it to be really protective (as every one knows it is), you can hardly expect me to believe that doctors advance statements opposed to the truth in this matter, when they advocate vaccination. An epidemic of small-pox would be a fortune to mercenary medical men, protected themselves by vaccination. The paltry vaccination fees are as nothing by comparison with the fees they could fairly claim in the times before vaccination. If small-pox were rife in any city where you or I dwelt, should we not at once be vaccinated, and should we not then feel as safe as if there were no such epidemic? Frankly, I should consider myself a public offender if I admitted one line here in favour of the views maintained by the Society for the Preservation of Small-pox. As for what you say about Whist, doubtless many would prefer to have the space now allotted to Whist given to some other subject; all are not Whist players. But the omission of several subjects which *all* do not like might readily lead to the omission of KNOWLEDGE altogether. Every one must not expect KNOWLEDGE to be *filled* with just those subjects he may chance to like. I think we give our fair twopenny worth of science, without the Whist and the Chess.—SUSAN E. GAY. Your letter was sent at once to printers, and will appear soon.—A. H. H. My papers on Differential Calculus would probably be simpler than you would need.—DONALD KING. An article on sun spots would be necessary to answer your question; will probably have one shortly. Meantime, any good book on astronomy would help you.—H. B. LINDSAY. Part of your letter referred to F.R.A.S. May shortly write about space; but your remarks need not, therefore, be withheld. You know our requirements as to space.—J. MCDOWELL. Your name entered on list for Nos. 2, 3, and 5. You are quite right about our proposal on p. 302; we were but jesting.—F. RAM. To Darwin and Wallace, of course, is due the credit of the recognition of the accepted theory of evolution. Spencer's researches are, however, of great importance.—CARPENTER. If you really want to know its weight, put it in the scales; if you set the query as a "sum," perhaps you will explain what you mean by saying "the surface of the wet part was 209·44 inches." INCREDULOUS. Probably, table turned by pushing; depends how it went. Q. E. D. Hog puzzle right. W. H. HEWETT. The conditions seem equally favourable. SPENCER RYDER. I should imagine that you might more readily infer the existence of a God from your own personal sense of his works, than by the rather roundabout way you suggest. Does the most beautiful painting of a landscape, or the most perfect description of the glories of the heavens, give you so good an idea of the existence of a God as the landscape itself in one case, or the star-lit vault of heaven in the other?—J. A. C. OLLARD. Yes; but suppose 20,000 copies of supplement were printed, "containing letters only or chiefly," and only 1,000 were sold; how then? On the reason of animals question, what you "believe" and "do not believe," might not interest all our readers.—FINITE SPACE. Neither can I imagine space limited: nor can any one; nor can any one imagine infinite space.—C. J. TOOTELL. Thanks for picture of sun-spots, but by time it would appear they would not be there. Your method of focussing, where rackwork is not very delicate, is, I know from experience, excellent. (It is to let the eyepiece be not fully screwed "home," and then adjusting roughly by means of rackwork, to get exact adjustment by turning the eyeglass.) Only it is not safe to have the eyeglass but half screwed in; for if you have many adjustments, and a preponderance chance to be by screwing outwards, the eyeglass may fall out.—G. T. M. Both your suggestions are already under consideration. Thanks.—M. V. M. Thanks; but too long, and scarcely suitable otherwise.—H. W. We do not want metaphysics, melancholy though you think the future of science is to be without it.—SIRIUS. Very doubtful at present about that companion. Spectroscopic evidence shows Sirius to be probably in an earlier stage of sun-life than our own sun.—JAWGE. Dental formula: incisors, $\frac{6}{6}$; canines, $\frac{1-1}{1-1}$; molars, $\frac{7-7}{7-7}=44$,—means that the teeth are arranged in the two jaws, Jawge, thus:—

M M M M M M M C I I I I I I C M M M M M M M
M M M M M M M C I I I I I I C M M M M M M M

where M stands for a molar, C for a canine, and I for an incisor tooth.—THOMAS SMITH, Jun. Describing a man's character from his bumps is not phrenology. Study what Gall and Spurzeim really taught, and you will see what it is that science rejects.—ISHTAR. *Altruism*, derived from *alter*, "another," means the consideration of the interests of others before our own, just as *egoism* from *ego*, I, means the consideration first of our own interests.—G. T. W. M. There is a small book by Abbott, head-master of St. Paul's School, on the "Queen's English." I forget publisher's name and price.—J. H. CORBETT. You are too exacting. Parallax allows only a top to the earth, and you ruthlessly ask for a model. You might as well ask for soundings of the bottomless pit.—W. B. Thank you for reminding me; yes, the ratio of circumference of circle to radius is, as you say, fixed; but in the solution of the rod-tossing problem this constant ratio comes in with a factor which is not constant.—E. L. R. Yes; ice-particles can and do cause parhelia (not perihelia, as the *Gazette* prints it).—G. A. K. The historical value of the play called Shakespeare's Henry VI. is very small. Shakespeare wrote but a small proportion of it. As you say, the Bear and Ragged Staff were not taken by the Nevilles before their alliance with the Warwick family to which the crest belongs.—W. SUMNER. The rotation of the earth has been regarded as appreciably uniform. It is not quite so, but the variation is very slight. The question is, however, too complicated to be discussed here. It wants an article to itself.

Letters Received.

W. S. Jackson, K. Huskisson, I. H. Vulliamy, R. Tollit, Praxis, K. L. P., J. Murray, Metamorphosis, Pertinax ('Tis true, 'tis pity), Cosmopolitan, Anxious One, W. John Grey, J. H., Mongredian, Semper Paratus (so is W. P. B.), Jupiter Tonans (save us), M. Purvis, J. Harmens, F. M. Rogers, Pollaky, H. F. C., J. H. Marvin, P. Tindale, M. Morrison, K. P. M., R. Lecky, Circle-squarer, Triangular, Queer Querist, M. Rambures, S. S. T., Rev. M. M., Castrensis, Ad ardua tendo (Respice finem), C. Collins, Anti-humbug, Simplex, Verge, H. Jowett, K. Prothero.

Our Mathematical Column.

PROBABILITIES.

By the Editor.

LET us next take a case not quite so simple as the tossing of a coin, namely, the casting of a die. We know that the chance of throwing ace in a single trial is $\frac{1}{6}$. Let us consider what is the chance of throwing ace in two trials with one die. The consideration of this will bring before us a very common mistake in dealing with chance questions, an instance of which occurred recently in the discussion of the question about cutting any one of three named cards (of any suit) from a pack, once in three trials. A correspondent asked, with reference to this question, whether, as the chance of cutting one of the cards in a single trial was obviously $\frac{3}{13}$, the chance in three trials must not be thrice this, or $\frac{9}{13}$. This is erroneous, but not very obviously so. So again in the case of a die—it is not obvious at first sight that the following reasoning is unsound. The chance of throwing ace in one trial is $\frac{1}{6}$, therefore in two trials it must be $\frac{2}{6}$, or $\frac{1}{3}$. We see, however, at once that the following reasoning is incorrect, or, at least, leads to an incorrect result, though it is precisely the same in character. Since the chance of tossing a head in one trial is $\frac{1}{2}$, the chance of tossing a head in two trials must be $\frac{2}{2}$, or certainty; for we know there is no certainty at all in the matter. Yet even here it is not quite clear to the beginner where the error comes in, and he is often inclined to think there must be some defect in our method of representing chances, when reasoning which seems correct leads to an obviously incorrect result.

Now the answer usually made to the above incorrect reasoning about the tossing of a die, runs commonly as follows: The chance that an ace will be thrown in the first trial is $\frac{1}{6}$, but the chance that an ace will be thrown in the second trial is not $\frac{1}{6}$, because there may be no second trial, for the first may give an ace. We must, therefore, add $\frac{1}{6}$, the chance in the first trial, to $\frac{1}{6}$ (the chance in the second trial), reduced in a degree corresponding to the chance that a second trial will be required. Now the chance that there will be a second trial is, in fact, the chance that the first trial will fail to give an ace, or $\frac{5}{6}$, so that the chance of throwing ace in a second trial is not $\frac{1}{6}$, but only five-sixths of $\frac{1}{6}$, or $\frac{5}{36}$. Adding this to $\frac{1}{6}$, the chance of throwing ace in the first trial, we get $\frac{1}{6}+\frac{5}{36}$, or $\frac{11}{36}$, for the chance of throwing an ace in two trials.*

But the objection suggests itself to the student that the second trial may be guaranteed, whatever the result of the first trial. The thrower may say to begin with - I *mean* to throw this die twice; what is the chance that one of the throws at least will be an ace?—and then the above reasoning about the contingent nature of the second throw is rendered unmeaning. De Morgan deals with this objection in a very just way, but I am not sure that his reasoning convinces all minds very readily. Todhunter, after noting the objection, says, "The error really arises from neglect of the following consideration: when events are *mutually exclusive*, so that the supposition that one takes place is not incompatible with the supposition that the other takes place, *then, and not otherwise*, the chance of one or other of the events is the *sum* of the chances of the separate events. In the present case success in the first trial is not incompatible with success in the second trial, and therefore we cannot take the sum of the chances as the chance of success in one or other of the trials." But this, after all, amounts only to a statement of the fact that that reasoning is erroneous by which the chance of throwing ace in two trials with a single die is made to be twice $\frac{1}{6}$. Now, this fact we *know*, because we see that the extension of the same principle of reasoning leads to an obviously incorrect result. What we *want* is to learn exactly where the error lies. I do not find that this is clearly shown in treatises on probability.

Let us take an illustrative case from which, as I judge, the true nature of the error may be learned.

In an urn there are six balls, marked from 1 to 6. The chance of drawing ball 1 is, of course, the same as the chance of throwing ace at a given trial with a single die; that is, it is $\frac{1}{6}$. Now suppose that six persons draw each a ball. One of them must have drawn ball 1. The chance that any one of the six has drawn this ball is $\frac{1}{6}$; and the chance that one of a given pair of these six persons has drawn the ball is $\frac{1}{6}+\frac{1}{6}$. This is clearly the case, as shown in paper I.; and that the reasoning is just is proved by the fact that when it is extended so as to include all the six persons, we get six times $\frac{1}{6}$, or unity, corresponding to the certainty that one of the six has drawn ball 1. Now the fallacy in the former reasoning about the die lies in the supposition that two throws with a single die give the same chance of throwing an ace that any pair of our six ball-drawers has of drawing ball 1. Whereas it is obvious that to represent the case of the die-throwing, we must have—*not* two different balls drawn at random from an urn containing six, *but* one ball drawn at random and replaced, and then again one ball drawn at random.

Let it be noted that there is no begging of the question here. It is certain that the chance of throwing an ace is the same as the chance of drawing ball 1 from the urn containing six. It is certain that to represent the second throw, as well as the first, the urn must have its full complement of six—that is, it is certain the ball first drawn must be replaced before the second drawing is made. Whereas it is certain that the case which gives as the resulting chance $\frac{1}{6}+\frac{1}{6}$, is the case where a ball is drawn, and then (or simultaneously, it matters not which) *another* ball.

That the two cases are distinct is rendered obvious, therefore. And not only so, but we can see which case gives the better chance. For in considering the two cases, we can place our finger on the exact spot where the chances differ. Suppose that a person A proceeds as in the former case, a person B as in the latter, each dealing with a separate urn, containing balls numbered from 1 to 6; and let us compare their chances of drawing ball 1. They begin alike. A draws a ball from his urn, and B one from his. Their chances of succeeding in this first drawing are, of course, equal: but if they fail, their chances on the second drawing are not equal. For A has to return the ball he drew into the urn again; and he will have no better chance of success at the second trial than at the first. But B retains the ball first drawn, and at the second trial he has a better chance of success than at the first; for he has to draw now ball 1 from an urn containing only five balls instead of six. But B's chance in his drawings is certainly $\frac{1}{6}+\frac{1}{6}$; A's chance, therefore, is certainly something less than $\frac{1}{6}+\frac{1}{6}$.

We see, then, that we must adopt a more trustworthy mode of reasoning in the case of successive trials under unchanged conditions.

* What follows is quoted, with very little change, from a series of articles on the "Laws of Chance as Applied to Statistics," which I wrote eleven years ago for the *English Mechanic*, where they appeared in August and September, 1871. I shall take occasion, when convenient, to borrow passages from those articles, but with such modifications as my experience of the difficulties commonly found by students of the subject may suggest.

A Pretty Geometrical Problem, and Mogul's Problem.—A great number of solutions of these problems have been received, and of the former ("Kelland's") problem a very complete discussion has been sent to us. It will be a work of some little time to analyse all the solutions, but we hope next week to give an abstract of "Mogul's" solution, and of the paper just mentioned, with suitable figures. Both problems are very instructive.—Ed.

Our Chess Column.

How the Devil was caught. Played at Brighton, 1879.

Allgaier Gambit.

White. Mephisto.	Black. F. Edmonds.	White. Mephisto.	Black. F. Edmonds.
1. P to K4	P to K4	12. B takes P(g)	Kt takes B
2. P to KB4	P takes P	13. P takes Kt	Q to Kt6(ch)
3. Kt to KB3	P to KKt4	14. K to Bsq	Kt to B3
4. P to KR4	P to K5	15. Kt to B3(h)	Castles QR(i)
5. Kt to Kt5	Kt to KR3(a)	16. Kt to Q5	R takes Kt(j)
6. B to B4(b)	P to Q4(c)	17. P takes R	Kt to Q5!
7. B takes QP	B to Kt2	18. P to B3	P to KB4
8. P to Q3	P to KB3	19. P to Kt5	R to Ksq(k)
9. Kt to K6	B takes Kt	20. P takes Kt	B takes P
10. B takes B(d)	P to B6(e)	21. Q to K2(l)	R takes Q
11. P takes P(f)	Q to Q3	22. K takes R resigns.	Q to Kt7(ch)

(a) Not to be commended. Black only obtains a very indifferent game by this move, whereas, by the usual continuation of 5 P to KR3! he ought to get the better game, in spite of White's subsequent attack. If a player is afraid to expose himself to the attack, then the more logical course would be to refuse the Gambit from the beginning.

(b) 6 P to Q4 is the proper move here, for, if Black plays 6 P to KB3, then 7 B takes BP, 7 P takes Kt, 8 (B takes P and White wins his piece back, but we usually prefer 8) P takes P, as this sacrifice yields some interesting play.

(c) Whereas, now he might have played 6 P to KB3 and won the Knight with tolerable safety.

(d) White might also have played B takes KtP, for, although he would thus give up two pieces for a Rook, he would not have the worst of it, as he would capture the KBP and have 7 Pawns to 5 of his opponents.

(e) An ingenious conception, which attained its object, but which might have been met differently.

(f) Instead of P takes P, he should have played 11 B takes Kt! Q to Q3 for Black would not be good now, as White could safely take the Bishop, as he need not fear the series of checks with Black's Queen. 11 P takes KtP would also result in White's favour. He would move his R to Ktsq, and, in reply to B takes B, White would play Q takes KtP. The best line of play would be 11. $\frac{\text{B takes Kt}}{\text{B takes B}}$, 12. $\frac{\text{P takes P}}{}$ (better than 12 B takes P, to which Black might reply with 12 P takes P, 13 R to Ktsq, 13 Q to Q5, 14 R takes P, 14 Q takes QKtP with a fair game), 12 Q to Q3, 13 $\frac{\text{B takes P}}{\text{Q to Kt6(ch)}}$, 14 $\frac{\text{K to Bsq}}{}$, and White has two Pawns with a good defence, as he will now be able to force the Queen to retire either by playing R to R3 or Q to Ksq.

(g) Curious to say that, as we examine the position, we find that even now B takes Kt would have proved effective; in fact, White had nothing to fear from Q to Q3, for, if now in reply to 12 B takes Kt, 12 B takes B, then we have the same position as examined in our former note; or, if 12 Q to Kt6(ch), 13 K to Bsq, the tempting move of 13 P takes P for Black would be met by 14 R to Ktsq, 14 Q takes RP, 15 B to K3, and White has won a piece.

(h) If 15 Kt to Q2, then Black replies with 15 Kt to K4.

(i) This shows good judgment. He does not at once play Kt to K4, but brings his Rook into play, while White's game remains *in statu quo*.

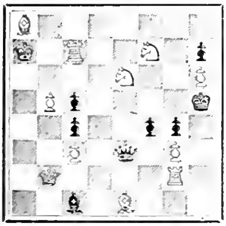
(j) Black is playing in very good style. He obtains a strong attack by this sacrifice.

(k) Excellent play; something of Morphy's style.

(l) We don't see anything better. If Q to Q2, then S to B6(ch), or if Q to B2, R to K8 mate. Of course, he threatens mate on B2.

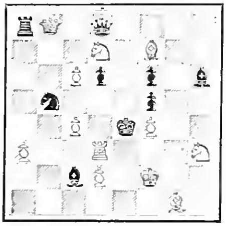
LOWENTHAL PROBLEM TOURNEY, No. II.

No. 26.
FIRST PRIZE SET.
Motto: "Peep beneath."
BLACK.



WHITE.
White to play and mate in four moves.

No. 27.
SECOND PRIZE SET.
Motto: "Too many 'Cooks' spoil the mate."
BLACK.

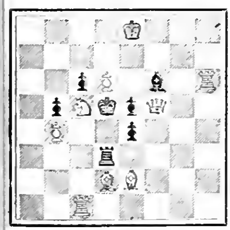


WHITE.
White to play and mate in three moves.

The following two problems have won the First Prize in the Tournament of the *Boys' Newspaper* (taken from the *Chronicle*).

By G. Hume (Nottingham).

No. 28.
BLACK.



WHITE.

No. 29.
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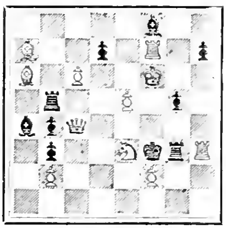

WHITE.

White to play and mate in two moves.

GAMES BY CORRESPONDENCE. (Continued from p. 442.)

GAME I.

Position after White's 17th move.
P to KKt4.

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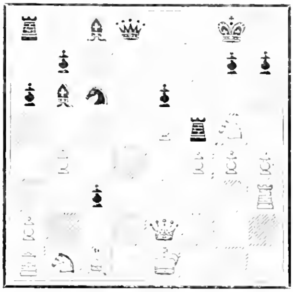
CHESS EDITOR.



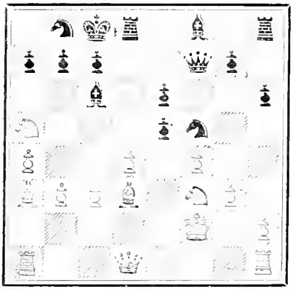
CHIEF EDITOR.
WHITE.

CHIEF EDITOR.	CHESS EDITOR.
	17. R takes Kt
18. RP takes R	18. Q to Q5
19. Kt takes P	19. Q to Kt8(ch)
20. K to Q2	20. B to Q2
21. Q to Q3	21. P to Kt3
22. Kt to K2	22. Q takes P
23. R to K Kt3	23. Q to R4
24. B to R3	24. R to Qsq
25. Q to KB3	

GAME II.

Position after Black's 15th move.
P takes P.

BLACK.
CHESS EDITOR.



CHIEF EDITOR.
WHITE.

CHIEF EDITOR.	CHESS EDITOR.
16. Kt takes KP	16. Q to B3
17. Kt takes B	17. Kt takes Kt
18. Kt takes Kt	18. P takes Kt
19. B to R6(ch)	19. K to Ktsq
20. R to Ksq	20. B takes B
21. R takes B	21. P to KKt4
22. Q to KB3	

CORRECTION.—PROBLEM No. 25, p. 441.
Remove Black Pawn on R3.

ANSWERS TO CORRESPONDENTS.

*** *Please address Chess-Editor.*

J. A. Miles.—We have sent you your problem by post for correction.

C. H. Brockelbank.—Received with thanks.

R. G. Brothers.—We regret to see you disappointed by H. T. Holden. Have given you another opponent, J. B. Groser, who will reply to your moves.

J. B. Groser *versus* R. G. Brothers.

Our Whist Column.

By "FIVE OF CLUBS."

I HAVE been for some time past endeavouring to systematise the play second hand, like the leads, but the task is not an easy one. To begin with, the lead is always guided by one of two considerations: it is either from strength, or, when from weakness, it is played to help partner as much as may be. In most cases it is from strength, and there can then be no question as to the card to be played, and very little as to the meaning of a card which has been played. But the second player may have strength or weakness, or neither strength nor weakness, in the suit led, and his play thus depends on a greater possible variety of positions. Then, again, it depends on the lead; so that we cannot say, as we can in the case of the lead, such and such a card means such and such a suit, but must take into account the card led in the suit. It thus becomes impossible to present anything like such simple rules, either for playing second hand, or for the interpretation of the play second hand, as in the case of the lead. Half a column on p. 310 suffices for the statement of all that need be known about the leads or their interpretation; we cannot present the rules for second play in anything like this space.

To proceed systematically, let us consider the leads as presented in p. 310, and the corresponding play of second hand. Fortunately we can dismiss a number of cases very quickly.

PLAY SECOND HAND WHEN ACE, KING, OR QUEEN IS LED (PLAIN SUITS).—When Ace is led, of course, second hand has only to play his lowest, unless he wishes to signal, when he plays his lowest but one. When King is led, second player, if he holds Ace, puts it on ("covers," is the technical expression*), otherwise plays his lowest, unless to signal—a case we shall not hereafter specially refer to. When Queen is led, we know that the leader does not hold Ace or Queen, and unless the lead is from a weak suit (a forced lead), that he does hold both Knave and ten. If second hand holds both Ace and King, he would, of course, play the King. If, of these two cards, he holds Ace and others, whether long or short in the suit, he plays the Ace. If he holds King and others, his play will depend on his strength in the suit; if short in the suit, it is better to cover; if long, to pass the Queen, playing, in fact, on the same principles which guide in leading from weakness on the one hand and from strength on the other. The play second hand when Queen is led depends on the consideration that if Ace is held by third hand, it will not be played unless King is played second hand, when, of course, it will be played by fourth hand. Now, if third player holds Ace, and second player having King is short in the suit, he can gain nothing by failing to cover. Leader will know Ace lies with third player, and will lead again (the lowest of his head sequence) when the King, if again kept back, will be unguarded, so that a third round will cause the King to fall to partner's Ace. If, however, the suit is long, this danger does not exist, and there is a greater probability that Ace will fall or the suit be ruffed early. On the *other* hand, if fourth player has Ace, it is still second player's interest to keep back the King if he is long in the suit. His partner will take the trick with the Ace, and whether second round is led by original leader or his partner, the King will capture another card of the head sequence, with good chance that the last will fall in the third round. If, however, second player was short in the suit, of course, he gains nothing by thus clearing it: it is best for him, therefore, to cover with King second round, even if his partner holds the Ace.

When Queen is led and second player holds King, ten, and one other, the question may arise whether it is not better to hold up the King, on the chance that partner taking trick with Ace, the return of the suit, when the leader's partner gets the lead, may find second player with the tenace. It is, however, better on the whole to cover in this case. The lead is in all probability from Queen, Knave, and one other; it is certainly a forced lead; and it is an even chance that partner holds the Ace, and also an even chance that he has numerical strength as against third hand. It is three to one that one of these conditions holds, and in either case, playing King second hand is good. If your partner holds the Ace, you and he still have the command; and if he has length, you help to clear his suit by playing the King. The state of the score may occasionally justify departure from this rule, however.

It is scarcely likely that when Queen is led, second player should hold King and Knave, with or without others, for this can only happen when Queen has been led from Queen and a small one, a lead only justified by the absolute impropriety (shown by previous play) of leading from long suit, and clear evidence that partner is strong in the suit so led. (Of course, if this *should* happen, the King is played. In fact, almost invariably when second player holds with other card—a fourchette for a high card led (that is, King, Knave for a Queen; Queen, ten for a Knave; Knave, nine for a ten; and so on) he should cover.

* I desire to call attention to the circumstance that these papers on Whist play are intended for learners, not for those who already understand Whist strategy.

Solutions of Problem 2 by Valnol, A. C. W., H. Barclay, Peter Parley, M. Morrison, correct.

G. THOMPSON.—Sorry we misled you with Problem 2; we leave it open another week.

W. F.—You are right on one point; in lines 13, 15, and 16 for *Z* read *B* (hardly worth correcting, being so obvious). But you say: How can (meaning, perhaps, *why* should) *B* now ruff? *Y* has no means of knowing that *A* holds the winning Heart after Ace, viz., the ten, and we are considering the second and third round from *Y*'s standpoint. Of course, *B* would not have trumped his partner's winning card. Again, you say, How should *B* win the second round with a trump? How or why should he *not*, had *Y* put on a small trump, as he ought to have done?

T. F. gives the following question:—

"How many different hands may a person hold at a game of Whist?" His answer is 635,013,559,600; but this is open to question. Is it the same hand, for instance, when same cards are held, but trump suit is different? or whether holder of hand is dealer or not? or leader or not? or if, being dealer, *any* card of the thirteen be supposed the trump card? Whist players would answer No to all these questions. FIVE OF CLUBS.

DEAR FIVE,—I was shown the subjoined Double-Dummy Problem by a fellow-passenger from Wellington to Napier (New Zealand), last year. No doubt it has been published, and you or one of your Whist correspondents can tell me where and when. But it seems to me (I speak under correction) a good problem.—Yours, &c.,

EDITOR.

PROBLEM 3.—Double Dummy.

THE HANDS.

A.		*Y.*
Hearts—A, Q, 10, 9, 4, 3.	B	*Hearts*—K, 8, 7.
Clubs—10, 6.		Clubs—9, 8, 7, 4.
Diamonds—3.	Dealer.	Diamonds—2.
Spades—10, 9, 8, 7.	Y Z	Spades—6, 5, 4, 3, 2.
B.	*Trump Card, Hearts Two.*	*Z.*
Hearts—Kn, 6.	A	*Hearts*—5, 2.
Clubs—5, 3, 2.		Clubs—A, K, Q, Kn.
Diamonds—A, Q, Kn, 6, 5.		Diamonds—K, 10, 9, 8, 7, 4.
Spades—A, Q, Kn.		Spades—K.

A leads and makes every trick.

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Contents of No. 22.

PATH OF MARS FROM 1875 to 1892.

By a mistake, for which I fear I have no one but myself to thank, 1877 was printed for 1892 in the heading to the looped path of Mars from 1875 to 1892. As there are some 600 positions of the planet (all separately laid down before the path was carried through them) and the constructions for these involved many hours of labour, it was rather annoying to find the diagram appearing as if it only showed the planet's path during two or three past years, instead of showing it for seventeen years, ten of which have still to pass. For a time I felt disposed to reject Liebig's saying that "there is no harm in making a mistake."—Ed.

THE ANTIQUITY OF MAN IN WESTERN EUROPE.

By Edwin Clodd.

It is well known that the period from the unknown date of man's appearance in Europe until about the Christian era, is divided by antiquarians into the Ages of Stone, Bronze, and Iron. Such a division, anticipated nearly 2,000 years ago by Lucretius in his immortal poem, *De Rerum Natura*,* is not restricted to one quarter of the globe, but holds good for every part habitable once or inhabited now; a mass of ever-increasing evidence being producible to show that the use of stone and other accessible and pliable materials preceded that discovery of metals which placed so powerful an instrument of control and advancement in the hands of mankind. These divisions of stone and metal-using periods, it should be borne in mind, are not to be applied to all parts of the world at one and the same time, as if there had been a universal and co-temporaneous abolition of stone at a given epoch in human history, and a universal adoption of the compound metal bronze in its place. In the later periods of the Stone Age, it is certain that Europe was occupied by races in markedly varying degrees of civilisation. The people settled along the shores of the Mediterranean were far ahead of those scattered over Northern Europe, iron being probably known to the former, while the latter still used ground and polished stone implements, or bartered the much-prized amber for Etruskan and Phœnician bronze. So in the present day, widely as metals are dispersed by traders, we find barbarous races who still make shift with tools and weapons of stone.

But it is more important for our present purpose to point out what is not so well known, namely, that the subdivision of the Stone Age into the Palæolithic or Older, and the Neolithic or Newer, marks a difference between these, which is in every respect greater than that between the Neolithic and succeeding ages. Whilst these latter cover a comparatively trifling, although crowded, span of man's tenancy of Europe, and one over which the line of his advance is, if dim and zigzag, unbroken; the Palæolithic is of remote origin, of unknown, but certainly vast, duration, and but sparsely marked with the traces of his presence. The men of Neolithic times, concerning whom Mr. Grant Allen has given the readers of Knowledge a vivid and accurate sketch, are the direct ancestors of peoples of whom remnants yet lurk in out-of-the-way corners of Europe, where they have been squeezed or stranded; but the men of Palæolithic times can be identified with no existing races: they were savages of a more degraded type than any extant; tall, yet barely erect, with short legs and twisted knees, with prognathous—that is, projecting, ape-like jaws, and small brains. Whence they came we cannot tell, and their "grave knoweth no man to this day." The implements of the ancient Stone Age, mainly of flint, sometimes of chert (an impure, flint-like quartz), coarse, rough-chipped and unpolished, can never be confounded with those of the later age, which are made of divers native or imported materials ground to a fine edge and polished, often exquisitely shaped, and highly ornamented. Whilst those of Neolithic times are found in surface remains, cavern-floors, camps, temples, tombs, and mounds, from stately tumuli to rubbish-heaps on the Baltic coasts, all more or less within the province of the antiquary, those of Palæolithic times are unearthed by the geologist from ancient river-valleys, from "caves and dens of the earth," and from deposits so venerable that their contents demand a far higher antiquity for man than many anthropologists, by no means the slaves of Archbishop Ussher's chronology, are as yet willing to allow. Between the Older and Newer Stone Ages there is fixed the great gulf of climatal change and of altered distribution in land and water, for while the early, if not the earliest, Neolithic immigrants into Britain traversed a continent which, certain northern parts excepted, has undergone only local changes since they crossed it on their westward path, Palæolithic man passed without hindrance where now flows the English Channel; the mainland stretched northwards beyond Ireland and Iceland; through a forest-covered area over which the German Ocean rolls ran the Rhine, its waters swelled by streams now known as the Thames and Humber, to empty itself in the North Atlantic. Southward, the continent was joined to Africa at Morocco and Tunis, dividing the Mediterranean into land-locked seas, and making easy passage for man and brute from tropical to cooler zones. Whilst the animals brought by Neolithic races from the East were, in the main, those familiar to ourselves, those with which Palæolithic man waged war in forest and swamp were mammals now wholly extinct, or found only in arctic or tropical latitudes. The curious admixture with human relics of remains of animals adapted to widely different regions

*Bk. v., 1,282. Munro's tr., p. 268.

has raised some interesting questions concerning the causes and duration of changes in climate which permitted, now northern species (as the mammoth or woolly haired elephant, the musk sheep, the reindeer, etc.); now southern species (as the hippopotamus, hyæna, lion, etc.), to roam over the same areas, finding at last a common grave. But to discuss this at present would be to digress.

The above outline of the leading contrasts between the two Stone Ages may suffice to show that our subject lies far beyond the historic horizon. Ordinary landmarks and methods of reckoning, therefore, failing us, we can have some idea of man's place in geological time only by ascertaining the relative position of those deposits in which traces of him are believed to occur to the general system of organic-bearing rocks. Such traces are in truth indicated by what man has done, rather than by himself, for of him scanty are the relics only a jawbone or fibula (small legbone) here, only a skull fragment there, a paucity for which we must hereafter seek an explanation.

Vast as is the period in this world's history since the appearance of man even in Europe, it is but a fraction compared with that which extends from the beginning of life upon the earth to Palæolithic times. Hæckel* remarks that if we divide that period into a hundred equal parts, and then, corresponding to the thickness of the systems of strata, calculate the relative duration of the five main divisions or periods according to percentages, we obtain the following result:—

Primordial Epoch .... (70,000 feet.)	Laurentian System Cambrian Silurian	53·6
Primary .... (12,000 feet.)	Devonian .... Coal Permian	32·1
Secondary .... (15,000 feet.)	Trias..... .... Jura Chalk	11·5
Tertiary......................... (3,000 feet.)	Eocene .... Miocene Pliocene	2·3
Quaternary (or Pleistocene) (500 or 700 feet.) †	Palæolithic Man ... Neolithic and Historical Period	0·5

Now, it would be in defiance of all that the doctrine of evolution teaches, and, moreover, win no support from believers in special creation and the fixity of species, to seek for so highly specialised a mammalian as man at an early stage in the life-history of the globe. Even in the Secondary epoch, the only mammals which have been discovered in Europe are the fossil remains of a small marsupial or pouch-bearer; and although the placental mammals and the order of Primates, to which man is related, appear in Tertiary times, and the climate, tropical in the Eocene age, warm in the Miocene, and temperate in the Pliocene, was favourable to his presence, the proofs of his existence in Europe before the close of the Tertiary epoch, although considered sufficient by many foreign *savans*, are not generally accepted here.

It is at this point, however, that the interest of the matter deepens. No anthropologist of repute denies the *à priori* probability of man's presence in Europe under the favourable conditions of mid-Tertiary times; and the remembrance of what incredulity met the discovery in 1847 of relics indisputably of human origin in hitherto undisturbed deposits in the Somme Valley, checks incautious and hasty treatment. Let us glance at the evidence on which the French advocates of Miocene man now rest their case. Recognising the enormous duration of the Palæolithic Age, one of the most eminent of their number, M. Gabriel de Mortillet, has divided it into several well-marked stages, certainly with such warranty as all that we have at hand in support of the slow rate of human advance gives. Of these divisions, five in number, M. de Mortillet places the earliest in the mid-Miocene period.

* "Hist. of Creation," vol. ii. p. 20.

† The thicknesses of this and the other deposits are only approximately true.

FOUND LINKS.

By Dr. Andrew Wilson, F.R.S.E., F.L.S.

PART IV.

THERE are no two classes of animals between which exists a greater dissimilarity than birds and reptiles. The active organisation of the one and the sluggish ways of the other, the warm blood of the former and the cold blood of the latter, are points in the popular natural history of the two groups which technical zoology has but emphasised in its turn. Yet the scientific examination of these beings reveals bonds of connection between them, all unsuspected by the ordinary reader, and demonstrates further, in the most suggestive fashion, that the likenesses to be presently alluded to must possess some origin and meaning. That origin, evolution maintains, is "descent" from a common stock; the meaning is that seen throughout all similar series of likenesses, namely, the natural result of the laws of animal development. In the case of birds and reptiles, the same considerations appeal to us which I have already indicated as existent in the details of frog-development. Either the likenesses science discovers between apparently distinct groups of animals are explicable, or they are not explicable. If the former, then science declares, with unanimous voice, that the likenesses are due to common descent, as the unlikenesses are due to the variations and modifications produced during the evolution of the race. If, on the other hand, the likenesses are inexplicable—as I hold them to be on any other theory save that of evolution—then must mankind fold their hands in the acknowledgment of an ignorance that might legitimately, by its avowal, close the door to astronomical research, to geological work, and to scientific investigation of every kind.

I am led to make these remarks because several correspondents have remarked to the editor, that because likenesses can be proved to exist between two different groups of animals in their young state, they do not understand why the evolutionist should lay such stress upon these facts as proving his contentions. One correspondent, for instance, says that he cannot admit that because one thing is like another, the two things must stand in the relation of parent and offspring. I reply, likeness does not necessarily imply similarity of origin, but, on the other hand, it is one of the proofs of such similarity. If likeness is to be denied its place as a proof of common origin—apart from other and equally powerful proofs known to biologists—what guarantee should we possess that unlikeness means dissimilarity? That the likeness of child to parent is a natural likeness, every one must admit. The reasons are clear enough, and they derive their force from the fact that the latter begets the former. I hold that the likenesses existent—especially in the *early* stages of development—between different groups, are to be judged on the same basis, namely that of *heredity*. A manifest resemblance in the young frog to a fish is, I repeat, inexplicable, equally on scientific principles and on common sense grounds, unless on the hypothesis that some bond of relationship connects the two. The duty of

disproving this idea rests with those who deny evolution. Until we receive a fuller and more likely explanation of such likenesses as those we are at present discussing, we are entitled to hold to the only theory, which, so far as I know, satisfies the requirements of a good hypothesis — these requirements being that *it explains all the facts and is contrary to none.* This end the theory of evolution attains in explaining both the likenesses and the dissimilarities of living nature.

Returning, after this needful digression, to the case of birds and reptiles, let us firstly note the structural points in which these classes agree. To begin with, the skull of both is joined to the spine by one bony process or *condyle*. There are two of these processes in frogs and their neighbours, and a similar number in quadrupeds, including man. Then, secondly, the lower jaw of a reptile agrees with that of a bird in its compound nature. This jaw, instead of being simple and composed of two simple halves (as in quadrupeds), consists in birds and reptiles of from eight to twelve distinct pieces, which are amalgamated to form one bone. Furthermore, whilst the quadruped's lower jaw is joined directly, and of itself, to the skull, that of the bird and reptile is attached to the skull through the medium of a distinct bone, which is named the *quadrate bone*. Curiously enough, this bone in the quadruped is pushed upwards into the middle of the skull in the course of development, and becomes one of the three small bones (*malleus*) of the internal ear. Again, reptiles and birds agree in possessing lungs alone as their breathing organs. No gills are developed (as in frogs and fishes) at any period of reptile or bird-life, although both, like quadrupeds, possess *gill-clefts* in the neck in early life. These "gill-clefts," seen in the early life of man himself, are to be viewed as feeble survivals of the aquatic ancestry from which, according to evolution, all Vertebrate animals have sprung. Furthermore, instead of the ankle-joint (as in man and quadrupeds) being situated between the end of the leg, so to speak, and the beginning of the ankle-bones, this joint in reptiles and birds exists in the middle of the ankle-bones themselves. This curious feature will be further alluded to later on.

The technical naturalist would enumerate other points of agreement between birds and reptiles, but sufficient has been said to show the close affinities which lie just beneath the surface of their organisation. Their differences, however, are also of pronounced type. The causes to which in the far-back past the evolutionist conceives the likeness between these animals to be due, have operated, through variation, at a less remote period, to produce the divergent lines of development. Thus we discover that birds are warm-blooded, whilst reptiles possess cold blood; the bird's feathers are unknown in the reptile-world; and the perfect heart and circulation of the bird—similar to that of man—are also unrepresented in reptiles. Crocodiles, which possess a four-chambered heart, like birds and quadrupeds, nevertheless exhibit the same imperfect and "mixed" circulation seen equally in frogs and reptiles. The lungs of birds are of "open" structure, and part of the air inspired passes through the lungs to fill certain "air sacs" in the bird's body, and also fills the interior of the bones in most birds. Such a distribution of air in the bird's body is evidently adapted for the exigencies of flight. On the whole, then, with certain well-marked likenesses—which, be it observed, evolution accounts for on the idea of a common origin—the classes of birds and reptiles are demarcated from one another by certain highly-distinctive characters.

The dissimilarities on the hypothesis of evolution are due to variation and modification; but, if this idea be correct, can we show the stages through which the variation has led these two groups? In other words, have the "links" which should hypothetically connect them, any existence whatever? Such an inquiry would have been answered in the negative only a few years ago; but, thanks to recent research, we are now enabled, satisfactorily enough, to bridge the gulf between birds and reptiles, and in a measure to reconstruct the pedigree of these curious races.

To render my remarks clear, it may be well at this stage to show in a tabular form the relative positions of the rock-formations with which we shall have to deal. Placed in the order in which they occur in the earth's crust, the rocks in question lie thus:—

Tertiary Rocks.	Recent.
	Pliocene.
	Miocene.
	Eocene.
Secondary Rocks.	Chalk.
	Oolite.
	Trias.

The meaning of this table becomes clear, if it be borne in mind that the rocks as here noted are divided into the older Secondaries and the newer Tertiaries. The Eocene in turn is the oldest (or lowest) series of the Tertiary rocks, as the Trias is the oldest of the Secondary rocks.

The fossil remains of birds are few and far between, and this for the reason pointed out by Lyell— namely, that the body of a bird falling into water, prior to its entombment in the deposits which form the rocks of the future, would float, and would afford a likely object of prey to other animals; thus escaping the chances of preservation. For long, fossil birds were regarded as limited to the Tertiary rocks; but we now know of their existence in the Chalk, or Cretaceous Period; and we have also obtained fossil specimens from the rocks immediately preceding the Chalk in time, namely the Oolitic or Jurassic Period.

It is almost needless to remark that the bird-remains of the Tertiary rocks, as a rule, resemble closely the birds of our own day. In this light they only testify to the age of some of our existing groups of birds, and do not directly support the theory of evolution, whilst, of course, they do not in any way negative it. But in the deposits of the London clay of Sheppey, belonging to the Eocene (Tertiary) period of geology, the remains of a bird, belonging apparently to the swimmers, were discovered. This form was named *Odontopteryx* by Professor Owen, and its remarkable jaw-armature at once attracted the notice of naturalists. No existing bird has teeth; and no bird possesses any structures approaching teeth in function—save, perhaps, such birds as the Mergansers, in which the horny margin of the jaw is cut into a series of projections, adapted for retaining a secure hold of the finny prey on which these birds subsist. But in the *Odontopteryx*, the jaws were beset with strong bony processes, which, though resembling teeth in appearance, nevertheless are mere projections of bone—for, as most readers know, teeth are not of bony nature, but possess a special and distinct structure of their own. Nevertheless, the fact of this extinct bird of the Eocene rocks possessing toothed projections of its jaws, serves to link it, in the opinion of naturalists, to the reptile hosts; for teeth are as stable and characteristic possessions of the reptile class as their absence is a natural feature of existing birds.

(*To be continued.*)

The Sound, or Swim-bladder of Fish.—*Erratum.* In my letter (Knowledge, March 17, p. 439), for "a swimming pair," read "a swimming *paw*"; for "straight," read "strait."—W. Houghton.

PHOTOGRAPHY FOR AMATEURS.

By A. Brothers, F.R.A.S.

PART II.

FEW subjects connected with science present more points of interest than the discovery of photography. M. Niepce, as we have seen, laboured most patiently for many years, and only achieved partial success. Independently, Daguerre was working with a similar object—that of fixing the image produced by light when projected by means of a lens on to a sensitive chemical compound. Also independently, Mr. H. Fox-Talbot commenced experiments for fixing the photographic image. In his work, "The Pencil of Nature," Talbot relates that in 1831, while sketching the scenery of Lake Como by means of the camera lucida, the idea occurred to him that it ought to be possible to fix the image produced by light by chemical means; and on his return to England he commenced researches, and continued them with such success, that in 1839 he read a paper before the Royal Society announcing his discovery, and in 1841 he secured his process by patent. This patent, however, he very generously resigned a few years later.

In his "History and Handbook of Photography," Tissandier relates a circumstance which I have not seen elsewhere, and it deserves to be repeated, as it shows that others besides those savants already named were busy trying to solve the difficult problem of painting by sunlight. The authority quoted is "Guide du Photographie," Paris, 1856, and the narrator of the fact is M. Chevalier, an optician. Daguerre and others were in the habit of frequenting Chevalier's shop, and in 1825 a young man called to inquire the price of a new lens which he had heard Chevalier was making. The price appearing to be beyond the man's means, he was asked for what purpose he wanted the lens, when he said that he had succeeded in fixing the light-produced image. Chevalier thought "Here is another of those poor fools who want to fix the image of the camera obscura!" But Chevalier was astonished when the man placed a paper on the counter. "That," he said, "is what I can obtain." Chevalier looked at it, and was astonished to find a view of Paris as sharp as the image of the camera. The inventor had fixed the view of Paris as seen from his window.

Here was an instance of success achieved, but for want of means, the discovery could not be carried further, and when we remember the importance of the matter, it is strange that the discoverer is never heard of again.

It is unlikely that Talbot could have known what others were doing, as it was seen, when the two processes were compared, that they were totally different. Daguerre's pictures were on polished silver, made sensitive with the fumes of iodine. This image was *latent*; that is, nothing could be seen until the vapour of mercury was caused to *develope* the image; that is, the mercury was deposited in proportion to the action of the light on the plate. Talbot's process was altogether different. The image was produced on paper, and was a *negative*. The advantage over the Daguerreotype in this case was that an almost unlimited number of copies could be produced as *positives*: that is, prints having the lights and shades as in nature. Both of these valuable discoveries were to some extent due to accidents. Daguerre noticed the effect produced on a plate coated with iodine, on which an object had been accidentally placed; and Talbot observed that gallic acid, which had been spilt on his prepared paper after exposure to light, developed the latent image.

These are the main facts relating to the discovery of the photographic art, and further detail here is unnecessary. Both of the processes named were extremely slow, several minutes being necessary to produce a portrait. But improvements were rapidly made. After bromine was introduced into the process by the late Mr. John Goddard, the Daguerreotype plates were so sensitive to light, that a rapidly-revolving wheel was photographed while illuminated by an electric spark—a feat which can scarcely be surpassed in the present day.

Many new, and modifications of old, processes were introduced, but it remained for the late Mr. F. Scott Archer to simplify and improve the method of obtaining the image formed by light—the great improvement being the use of glass to bear the film on which the image was to be fixed. Various attempts had been made by the late Sir John Herschel and others to utilise glass, but it was Mr. Archer who succeeded with *collodion*—a fluid substance, formed of gun-cotton dissolved in ether and alcohol. Archer did not *discover* collodion, but used it as a vehicle to hold iodides and bromides which were made sensitive to light in the way we shall see in a future paper.

NOTES ON ROWING.

By an Old Club Captain.

WHAT sort of stroke is necessary properly to drive at racing speed the modern racing boat, is sufficiently clear, as we have shown. How that stroke is to be given remains to be considered.

I take it, there can be no room for doubt on one point. The old-fashioned rule, according to which the arms acted merely as stretchers in the first part of the stroke—in fact, until the body became upright—can no longer be right. A stroke beginning under those conditions must necessarily be sluggish at the beginning, and want the sharp grip or catch which we have seen to be absolutely essential. On the other hand, arm work alone at the beginning of the stroke is utterly bad. But the stroke is taken with such sharpness, to give the sledge-hammer blow necessary for propulsion in the modern racing craft, that it is by no means easy, even for the *possessor* of a perfect—that is, of the most effective—rowing style, to say exactly how his work is done. Mr. Muybridge's photographs of rapid movements by athletes have shown that men who can accomplish some feat in gymnastics, and even champions in such ordinary actions as running and jumping, often have (Mr. Muybridge, when I was talking to him on the subject at San Francisco, said they *always* have) the most incorrect ideas as to the way in which they perform their feats. Accordingly, we must not take it too surely for granted that because an oarsman says he does this or that in taking his stroke, he necessarily does so. Rapid photographs alone could tell us precisely how the best oarsmen take the stroke; but, for my own part, I feel assured, as well from practical as from theoretical considerations, that while the body is moving from its most forward position to uprightness, the arms do so much of their work that they are very considerably bent by the time the body is upright. Many Oxford men say, and doubtless believe, that in the Oxford style the arms are not bent at all till the body begins to pass the upright position. All Cambridge men know that at Cambridge the old rule used to be regarded as absolutely to be observed, that till the body was upright the arms should do no work. At Cambridge the rule was rigidly followed. In the Oxford style I feel satisfied, from repeated observations, it is not followed; and not a few Oxford men have, on carefully studying the matter, admitted to me that this is the case.

Of course, I do not advocate the undue use of the arms at the beginning of the stroke. To say truth, I in a sense advocate nothing except the intensely energetic stroke, which we have seen is absolutely essential to swift propulsion in a racing craft of the lighter sort. That such a stroke must be taken is certain. The efforts to be made in taking it are scarcely matter for theorising. Nature solves the question for herself. Where the good and the bad styles differ is not *here*, but in the choice of the kind of stroke to be given. The man who sets himself to row a long, sluggish stroke in the water, with a very rapid recovery, will row in bad style, because the kind of stroke he sets himself to row is inherently bad. The oarsman, on the other hand, who decides to take his oar through the water with all the energy he can bring to bear on the work, using the recovery to gather his energies for the next great effort, will row in good style (assuming always that he has mastered elementary matters in rowing), because *that* is the kind of stroke which ought to be taken, and there is only one way in which it can be taken.

Albeit, as the learner may be helped to acquire a good style (we speak always, be it remembered, of a racing boat at racing speed) by being told how to take his stroke, almost as well as by being told what sort of stroke to take, it may be useful to note that if at the very beginning of the stroke the muscles of the arm are all, as it were, tautened, so that the arm feels the work actively—not merely passively—from the beginning, the stroke will be begun under much more favourable conditions than if the arms be regarded merely as stretchers to bear the strain resulting from the action of the legs. There must be no jerking, no arm work done alone, but from the beginning the arms must assist the body and the legs. The other way of rowing, however suitable for the old-fashioned boats and for tub practice, is as unsuitable where a sledge-hammer stroke (long but full of energy) has to be taken, as would be the alternate action of the two arms. [And in passing, I may note that quite a large proportion of oarsmen who consider they know something of rowing, fail to use both arms with equal vigour, either in each stroke, regarded as a whole, or in the different parts of one and the same stroke. With many oarsmen the outside arm (that is the arm furthest from the rowlocks) does more than its fair share of work at the beginning and less than its fair share at the end of each stroke. It should hardly be necessary to say that this is a very bad fault indeed.] The great difference between the old-fashioned and the modern style, as regards the way in which the work is done, consists in this: that whereas in the old-fashioned style the best way to bring arms, legs, and back into effective co-operation was to let the stress of the work fall on these at slightly different times, in the modern style the best way is to bring all these forces into action simultaneously.

But the effort thus made, though it lasts a shorter time, is more exhausting than in the old-fashioned style. It cannot possibly be followed by the old-fashioned lightning feather. There must be a recovery of energy as well as a recovery of the oar. Hence the necessity for but a moderately quick recovery, and for a momentary pause just before the beginning of the next stroke.

The oar being a shorter time in the water, and only the recovery somewhat slower than in the old-fashioned stroke, it might seem as though more strokes could be taken per minute than in the old-fashioned boats, or at any rate, the effort be less; but as a matter of fact this is not the case. In the old "Hints on Rowing by Oarsmen," a stroke of 44 or 46 to the minute is spoken of as suitable for racing speed. This rate could not possibly be maintained now, even during a spurt. From 36 to 40 strokes per minute is about the quickest rate obtained by the best oarsmen of the day, and some races have been won, and well won, with a stroke never exceeding 35 to the minute. Again, as every oarsman who has tried both well knows, it is much more exhausting to row at racing speed in a light boat than in the comparatively heavy racing boat of former days. (I believe that, regarded as exercise, the older style of rowing is the better; but the object of racing is not exercise, but to attain the greatest possible speed).

There are some who, despite the absolute demonstration which, as we have seen, can be given to the proposition that with our modern racing-boat the oar *must* be a shorter time in the water than with the old-fashioned boats, insist on reiterating their belief in the necessity for long, dragging strokes,* and a very rapid recovery. For them, I venture to quote some very sensible and apt remarks by "Wat Bradwood." They were written at a time (1868) when Cambridge exaggerated as far as possible the faults (for modern racing-boats) of the old-fashioned style, and when Oxford had carried the later style almost to perfection. Observing from the Umpire's boat, he says:—"The styles of progress of the two boats themselves are palpably distinct: Cambridge take a shorter time to come through the air than to row through the water; they go much farther backward than Oxford, and are very slow in getting the hands off the chest; their boat is drawn through the water at each stroke, but has hardly any perceptible 'lift.' Oxford, on the other hand, are just the reverse of Cambridge, a long time in getting forward, and very fast through the water, 'driving the oar through' (really against the water) 'with a hit like sledge-hammers.' The general style of Oxford has not deteriorated; though many outsiders fancied that Oxford rowed a shorter stroke, it was more that the time occupied by them in slashing the oar through the water was short than the reach itself; this deceived inexperienced eyes, especially when compared with the slow 'drag through' of Cambridge, which often appeared, for similar reasons, a longer stroke than it really was."

And here let me make a few remarks on the utterly absurd criticisms of racing crews made in many of the daily and weekly journals. It would be interesting to inquire how the writers of some of these criticisms have put their notes together. I fancy their plan must be to talk with half-a-dozen watermen who have seen the boats at practice (or *say* they have), and to "average" what they get from these authorities. At any rate, the criticisms are often ludicrous in the extreme. To begin with, it seems *en règle* to say every year of the University crews that they are far below the average. I have seen this repeated *every year since* 1856, so that as in some years we have been told that the best of the two crews would not have a show with a "second eight" of any London Club, the University crew of this year ought to be a very bad one indeed. Some of these clever critics ought to man an eight, and after getting into due training and practice, take her on the top of a good tide from Putney to Mortlake, in less than $20\frac{1}{2}$ minutes, as the Oxford crew did this year in their first trial (under 20 minutes at the second trial, but rowing down stream); or else learn, when they had taken nearer 30 minutes in covering the course, to describe the Oxford

* "Pendragon," in the *Referee*, three years since, fell into this very natural mistake—especially natural in his own case, knowing, as he does, much more from personal experience about sprint running than about rowing. It requires practice both in racing craft and in so-called tubs (really very much lighter boats than those in which most people have ever tried rowing) to understand the difference between a sharp stroke and a quick stroke, between a long-lasting stroke and a stroke long in the water.

and Cambridge crews as something better than "inefficient mediocrities." Then again, to hear these critics, one might well believe that the eight picked oarsmen of either University hardly knew how to get their oars out of the water without crab-catching. The way in which they mistake cause and effect is amusing. They watch the Cambridge or Oxford boat rowing with a strong wind on the starboard beam, so that she is down on the stroke side for awhile, and we hear the terrible news that, stroke and six, or perhaps all the stroke side, "row too deep, the consequence being that they bring the boat down on their side." The height of absurdity, however, was attained last week in the *Referee*, which assuming the inferiority, or rather the utter worthlessness, of both the University crews (of "inefficient mediocrities") this year, gravely hinted that it had been attributed to favouritism in the selection of the men to form the crews. This well informed sporting paper hopes the report may not be true, but considers that if it is untrue, it should be contradicted: so that Tom and 'Arry, after reading the paragraph, are left under the pleasing impression that "them nobs" can "nobble a race as thoroughly as the worst "dark rowers" on the river. If Pendragon had ever attended Captains' meetings at either University, he would know how pleasant would be the fate in store for a "President" who should make up a University crew of inefficient mediocrities, or from any but the picked oarsmen of the best eights on the river. That on some occasions there may have been one or two men out of the "eight" who would have done a shade better than one or two in the 'Varsity crew, may be admitted: but readers of the *Referee* may rest assured that the President of either University Club selects *always*, to the best of his judgment, the eight men he believes to be fittest.

THE SUN IN APRIL.

[Notice.—The following notes are prepared on the supposition that those who will read them are provided with some almanac in which the astronomical phenomena are announced. The advanced student of astronomy usually has the *Nautical Almanac*, but for the amateur, Whitaker's Almanac will be found amply sufficient. Many other almanacs, though not so specially adapted to the requirements of the amateur astronomer, contain much astronomical information: and scarcely any fail to indicate such matters as the epochs of the lunar phases; the hours of rising and setting of the sun, moon, and planets; the equation of time, and so on. It has therefore not seemed desirable to occupy space with such details, but rather to provide matter which might prove of real assistance to the student.]

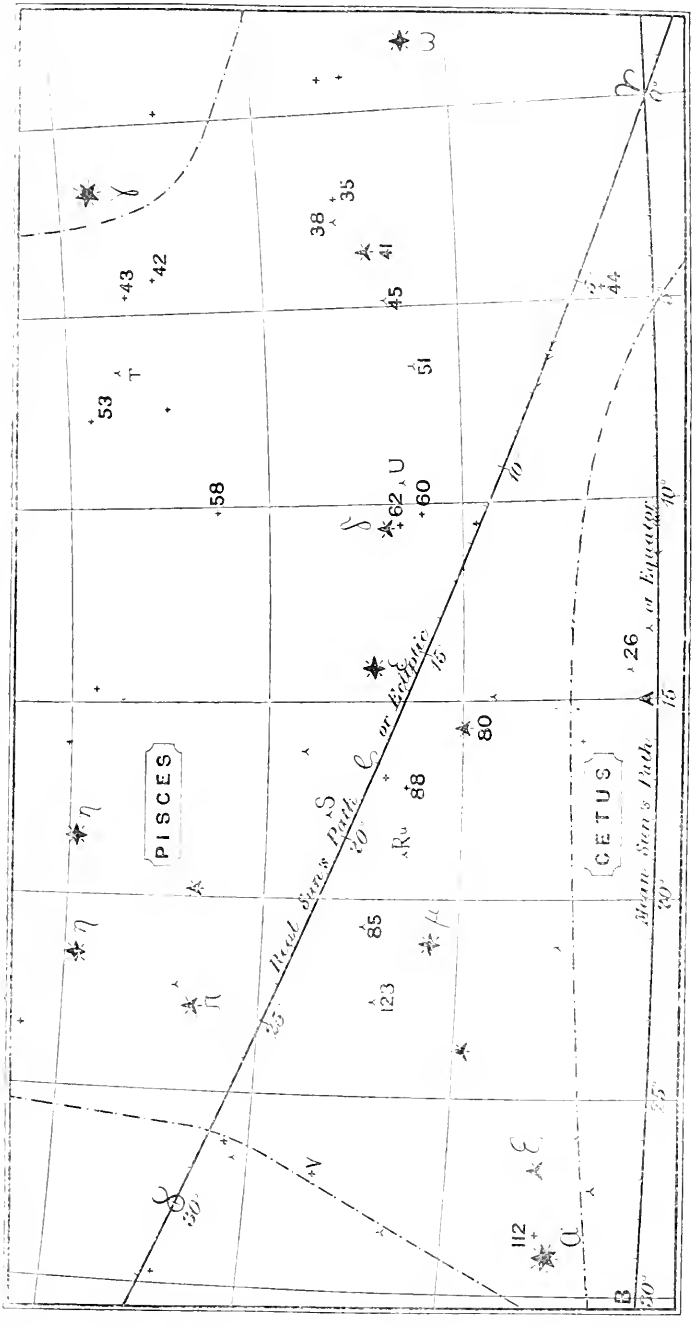
THE sun's path during the month of April, or rather from March 20 to April 20, is shown in the accompanying map from ♈ to ♉: that is, from the place where the sun's centre crosses the equator ascendingly, or northwards, through the part of the ecliptic corresponding to the sign Aries. On March 20, at 11 a.m., the sun passed the point called the First Point of Aries, and spring commenced. His rate of motion at this time does not differ much from his mean rate, and in the course of his passage through the first sign, Aries, he passes one of the points of his annual course where he is at his mean distance and moves with his mean rate of motion. (This occurs on April 1, and again after the autumnal equinox on October 3.) But although during March and April the sun moves at nearly his mean rate along the ecliptic, his path is at its greatest inclination to the equator. Thus, while he travels along 30 degrees of the ecliptic, reaching the point ♉, he has not advanced 30 degrees in right ascension, which is measured parallel to the equator. We see that the point ♉ instead of being in R. A. 2h., or 30°, he is only in R. A. 1h. 50m., or $57\frac{1}{2}°$ (see the illustrative map). The mean sun, therefore, which is supposed to travel at a uniform rate along the equator, gains during March and April on the real sun. But we must not fall here into the mistake made in some elementary treatises on astronomy (Mr. Lockyer's, for instance), where it is said that *therefore* the clock gains on the sun. The sun's loss in right ascension acts the other way. For as he moves annually in a direction contrary to that of the diurnal rotation of the heavens, the more slowly he moves in right ascension the shorter distance has he to make up in coming to the meridian. The eastwardly motion of the mean sun (from right to left in our map) causes the difference of nearly four minutes between a mean solar or civil day, and the sidereal day. The smaller amount of the real sun's eastwardly motion, measured parallel to the equator, causes the real solar day in March and April to exceed the sidereal day in less degree. Solar noon occurs sooner than it would if the real sun moved like the mean sun. Accordingly, whereas on March 20, at noon, the clock was nearly $7\frac{1}{2}$ min. faster than the sun, this amount has been gradually diminishing day by day since then; it will be less than 4 min. on the day when this number of Knowledge is published, little more than 2 min. on Thursday, April 7, 11·51 sec. on April 14; while at noon on April 15 the sun will be 3·17 sec. before the clock, and about 1 min. before the clock when the sun completes his passage through the sign Aries.

It is to these changes that the seeming irregularity of the day, as measured from sunrise to sunset, by the clock, is due. If the time of solar noon corresponded to noon by the clock, the hour of sunrise would be almost exactly as many hours and minutes *before* as the hour of sunset would be *after* the time of noon. But when the hour of solar noon follows the hour of clock noon, the time of sunrise comes so many minutes nearer to clock noon, while the time of sunset is as many minutes farther from clock noon: in other words, the sun seems both to rise later and to set later than it does by solar or natural time. Thus, on March 20 the sun rises at 6 h. 5 m., only 5 h. 55 m. before noon, and sets at 6 h. 10 m., or by so long (15 m. longer) after noon.* Whereas, on April 18th, when the equation of time vanishes, or the real sun and the mean sun are together (in right ascension), the sun rises at 5 h. 6 m., and sets at 6 h. 54 m., in each case 6 h. 54 m. from noon.

Asthma and Tobacco.—In reply to J. W. Brookes, I beg to state that tobacco is sometimes recommended to persons suffering from asthma, on account of its influence in deadening temporarily those nerves upon whose irritation the asthmatic spasm depends. Trousseau, an eminent French physician, who suffered from this complaint, mentions that "it often sufficed for him to take a few whiffs from a cigar to free himself from his asthmatic trouble." And, on the other hand, he says that he got an attack of asthma whenever he remained in a room in which there was a bouquet of violets. The aroma from the violets acted as a stimulus to the nasal nerve-twigs, which stimulus was communicated to the nerves governing the bronchial tubes, and thus contraction of those tubes, and consequent difficulty of breathing, were produced. Tobacco, on the other hand, causes temporary paralysis of the same nerves, and renders them incapable of producing contraction of the air-tubes. But its effects are merely palliative, not curative.—J. Muir Howie.

* Note that on this day, the sun, being on the equator, would rise at six and set at six were it not for refraction (almost exactly, but the progression of the sun across the equator causes a slight difference, the sun being south of the equator when rising, and north of the equator when setting). But refraction, by raising him into view before he has really crossed the plane of the horizon, and keeping him still in view after he has really passed over it, makes the day last rather longer than twelve hours on the day of the equinox.

THE SUN IN APRIL.



Reviews.

THE TWO HEMISPHERES.*

MR. CHISHOLM gives as complete an account of the continents and countries, the oceans and seas, of the two hemispheres, as could well be presented in a single volume (but of nearly a thousand pages). The book is pleasantly written, and the information for the most part accurate, though in places rather behind the time. For instance, we are told that the largest of the rivers in North Island, New Zealand, are the Waikato and the Manawatu, no mention being made of the Wanganui, a much more important river than the Manawatu. Some of the information respecting the United States is singularly out of date, and serves to give but inadequate ideas of the progress made in the Western States during the last quarter of a century, in some cases even within the last ten years or so, though statistical information is in most cases brought up to date. The illustrations of natural scenery are, for the most part, good; but the views of towns are in many cases behind the time. A "street in Chicago" gives no correct idea of any of the principal streets in that wonderful city; and the view of Madison-square must have been taken before the gigantic Hand of Liberty had been set up which has now for several years formed a characteristic feature of the square. Still, these are not points of great importance; and in the main we are struck rather by the amount of exact and recent information which the book contains, than by occasional shortcomings.

MESSRS. BLACKIE & SON note that Messrs. Marcus Ward & Co. have not published Vere Foster's Drawing Books (see reply to query 274) for 3½ years; these books are published by Messrs. Blackie & Son.

DID THE EGYPTIANS KNOW OF THE MOVEMENT OF THE EARTH IN SPACE?

IN confirmation of the translation of the Berlin papyrus by M. Chabas, a paper is published in the "Transactions of the Congress of Orientalists at St. Etienne," by M. Lieblein, one of the greatest authorities on the Egyptian vocabulary, pointing out another text alluding to the motion of the earth in very similar terms. This second sentence occurs in a kind of chant to the god Ptah, found in what is known of the great Harris papyrus, contained in the British Museum, and speaks of him as "creator of the gods, maker of heaven, and founder of the earth circulating in the great ocean of heaven." This text had formerly been translated by Dr. Birch and Professor Eisenlohr as "encircling the earth with the waters of the great sea," and thus it will be seen at once that the difference between the two readings is that where one translator reads travelling or *circulating in*, the other reads *surrounding with*. M. Lieblein, consequently, in order to prove his case, gives a large number of instances of exactly similar words, where if the verb were really to surround, and the preposition with, instead of in, such a translation would make nonsense, and also refers to many texts where this very word has been rendered to circulate, several of these having reference to the path of the sun. In controverting any decision arrived at by one so pre-eminent as Dr. Birch, the very greatest caution must be observed, but no one, after reading M. Lieblein's essay, can doubt that there is at least as much right to his idea of the true interpretation of the sentence as to that of our greatest Egyptologist.

Before approaching our subject from a point of view not purely Egyptian, it will be well to point out that these remarkable texts, in speaking of the stars or sun navigating the heavens in boats and the innumerable allusions to the sky as a celestial ocean, must not be taken to do more than clothe in elegant imagery the phenomena of the universe. Their writers no more considered the interstellar space as a liquid than we do. The idea itself undoubtedly originates in the beautiful account of the creation given in the seventeenth chapter of the Ritual of the Dead, when it states that the Deity, there alluded to as Toum, separated the terrestrial waters which gave rise to all rivers and oceans, from the celestial waters, suspending the latter in heaven. That behind and beyond all these allegorical references to the surroundings of our planet existed a conception well-nigh parallel to our own, can be seen by comparing many texts; for instance, in the inscription of Amenemheb, known as "The Praise of Thotmes III.," in speaking of the King's death at evening he says: "Then he fled up to heaven when the disk of the sun went down, and the servant of God joined himself to his Creator" (see Eccles., xii., 7). If it were legitimate to consider that the Egyptians spoke of a liquid heaven literally, then they contradict themselves, for almost as frequently it is spoken of as forming the body of the goddess Nout, whose figure is delineated adorned with the stars, her hands touching the horizon on one side, her body extended above, and her feet forming the horizon on the other. Then, again, there is another figurative theory mentioned by M. Maspero as being common, which speaks of the stars being fixed as lamps suspended in the celestial vault, and lighted each night by divine power to illuminate the earth.

A MEMBER OF THE SOCIETY OF BIBLICAL ARCHÆOLOGY.

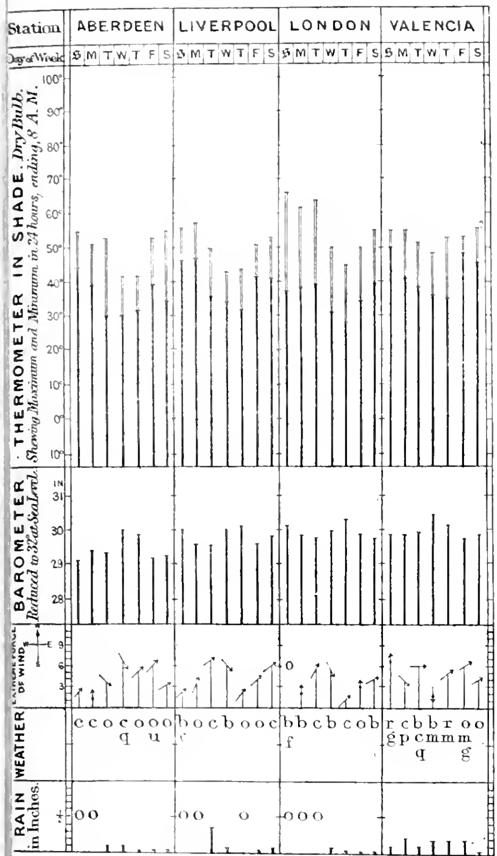
* "The Two Hemispheres: A Popular Account of the Countries and Peoples of the World." By G. G. Chisholm, M.A. (Blackie & Son, London.)

THE SOLAR SPECTRUM.—For more than twenty years I have had on my study table a prism, brass-mounted, and moving on a ball-and-socket joint. The apparatus may be had at any optician's. For two months in the spring, and for an equal period in the autumn, the sun is in a suitable position for showing through the prism a fine spectrum on the wall. It is one of the most lovely sights in nature. It never tires or becomes indifferent; and if I felt disposed to idolise a phenomenon, it would not be a sun-flower, but a sun-spectrum. Its interest to myself has been heightened by a spice of mystery. In the inexpressibly tender shading-off of the violet rays may be seen a tremulous motion; not always, but whenever the spectrum is very fine. Some of my family and friends can see it, but not all. This secret of many years I now suspect to be the passage across the violet of undulations of air ascending from the lower and more heated red portion of the spectrum. Any convenient method by which a curl of white smoke can be disengaged, so as to ascend gently through the path of the refracted rays, will afford a floating spectrum of exquisite beauty. Seen through the prism, the bars of the window, especially about sunset, are gorgeous. —HENRY H. HIGGINS.

THE LAMSON CASE.—Dating from the Lamson trial, we may expect to find the history of cadaveric alkaloids will be more fully investigated; and, although in this particular case none of the conditions essential to their existence were fulfilled, it will not be without some advantage, by stimulating inquiry into a very important and at the same time all but unknown class of decomposition products. In the whole masterly structure of the defence there was no more ingenious point raised than this of cadaveric alkaloids. Against the possibility of any reliance on it, however, were three fatal objections, which effectually and for all disposed of it. These were that the body of the murdered boy was not decomposed when the poison it contained produced fatal effects on animals it was injected into; that the vomited matter, preserved in alcohol, and thus free from decomposed substances, gave indisputable proof of aconitine; and that we have no proof that cadaveric alkaloids are capable of producing the physiological effects of normal vegetable alkaloids. Application has been made to the Home Secretary for a respite of the sentence passed on Lamson, on several grounds, among the reasons given being two chiefly interesting to us—viz., that the jury were not composed of medical experts, and the unreliability of the experiments made with mice. The first of these can need no serious consideration. The evidence was of a kind, more than is usually so in murder cases, to approve itself to the non-scientific mind. The symptoms of poisoning by aconite were carefully explained to the jury, and the evidence was such as to prove conclusively that these very symptoms were observed. Dr. Stevenson's testimony, moreover, was especially such as a child might comprehend; the only difference an expert jury would have made would have been to shorten the time in which the verdict was arrived at, and which even now is made one of the grounds of application for remission of sentence. Of the objection to the experiments made by Dr. Stevenson it is needless to speak further. No competent judge of their value and importance can hesitate a moment as to their weight; we venture to think none does so. On every ground, the Lamson trial will take rank as of high importance in a medico-legal sense; and as to the justness of its result we think no question can be raised.—*The Medical Press and Circular.*

WEATHER DIAGRAM,

FOR WEEK ENDING SATURDAY, MARCH 25.



WEATHER.—*Beaufort Scale* is, b. blue sky; c. detached clouds; d. drizzling rain; f. fog; g. dark, gloomy; h. hail; l. lightning; m. misty (hazy); o. overcast; p. passing showers; q. squally; r. rain; s. snow; t. thunder; u. ugly, threatening; v. visibility, unusual transparency; w. dew.

MESMERISM.

REPLYING to "A Startled One," KNOWLEDGE, p. 301 (260) the only work I have seen treating this subject scientifically is that ("Animal Magnetism") by Professor Heidenhain (Kegan Paul & Co.), with a preface by G. J. Romanes, F.R.S.

The subject is deeply interesting, and well worthy of attention by every medical man.

1. Heidenhain arrives at the conclusion that the cause of the phenomena of hypnotism lies in the inhibition of the activity of the ganglion-cells of the cerebral cortex, the inhibition being brought about by gentle prolonged stimulation of the sensory nerves of the face (by passes), or the optic nerve (by looking at a bright object), or the auditory nerve (by a monotonous sound), and that in consequence of the depression of activity of the brain there is a great increase of reflex irritability which may be made to extend over the body upon cutaneous stimulation of local parts, causing all the usual mesmeric phenomena, such as muscular rigour, insensibility to pain, hallucinations, &c.

2. The effect on the subject is temporary, causing only an increase of nervous irritability which passes off in a few days. The effect is similar to that produced by inhaling nitrous oxide gas.

3. The subject is restored to his senses by a change of stimulation, such as stroking the skin in a direction contrary to the original one; by sudden change of temperature, as by blowing on the face; or left alone, he will in a short time come out of the hypnosis by himself.

The opinions here expressed are entirely opposed to those of professional mesmerists, who maintain that the operator is possessed of some sort of electrical influence by which he can act voluntarily on the person experimented on. Actual facts adduced both for and against this theory would be highly interesting, for Heidenhain's hypothesis does not afford a satisfactory explanation of many of the extraordinary phenomena of hypnotism.

Perhaps you will allow discussion on this point in your interesting paper? F. GIBSON.

THE CARNIVOROUS PARROT.

IN November, 1879, Mr. J. Wood showed at a meeting of the Pathological Society the colon of a sheep in which the operation of colotomy had been performed by the *Nestor Notabilis*. The specimen, along with one of the birds, was from Otago, New Zealand. The *modus operandi* appeared to be as follows:—The bird, settling on the sacrum, tears off the wool with its beak and eats into the flesh until the sheep falls from loss of blood and exhaustion. It is doubtful whether the birds attack dead sheep, and it was stated that they single out the strongest sheep in the flock rather than those that are sick, dying, or disabled.

It is difficult to account for this acquired carnivorous habit. Professor Flower has suggested that the bird has in view the object of getting at the contents of the intestines, while others state that the loin and the underlying intestines are especially wounded owing to the position the bird takes upon the sheep's back. This is not an explanation, however, of the cause of the habit, for although we may, and do, easily alter the dietary of individual parrots, it would be interesting to know why the *Nestor Notabilis* in its freedom on the sheep-runs of New Zealand should voluntarily change its mode of living. Perhaps some of your naturalist subscribers will give us the reason. ARRECTIS AURIBUS.

EASY LESSONS IN BLOWPIPE CHEMISTRY.

By LIEUT.-COLONEL W. A. ROSS, LATE R.A.

LESSON V.—THE PHYSICS* AND CHEMISTRY OF THE THING.

ALTHOUGH the merely mechanical study of blowpipe manipulation; its, comparatively with other chemical studies, childlike simplicity and absolute economy, combined with the surprising rapidity, beauty, and correctness of results, and portability of apparatus required, have all such a charm for the man who seriously takes it up, as to exercise in most cases a complete fascination over him—the physics and chemistry of the thing must on no account be neglected; and although, technically speaking, this is called the "dry" method of chemistry, it will, when properly appreciated, be found infinitely more amusing, and quite as instructive, as the "wet" method, which involves the use of violent acids, expensive and delicate glass-apparatus, a knowledge of "atomic theories," &c.

Let us now, therefore, collect and recapitulate the physical and chemical phenomena casually mentioned in the four preceding lessons, as follow:—

1. A cone of blue fire produced from a candle, oil-lamp, or coal-gas flame by a constant confined blast.

2. The probable cause of the change of colour in hot beads cooling.

3. The power of the pyrocone to produce and abolish colour in solutions of oxides.

4. The solubility of *silica* in P. salt, and its insolubility in P. acid B.B.

(1.) As regards this fact, there is an obvious and very general error, arising from optical misapprehension, or, as Mr. Foster would call it, "untrustworthy information of the senses"—entertained as to the shape and nature of the fire-cone alluded to, in consequence of all or most writers on the subject making pictures of the blowpipe "flame" (as they call it) taken at the *angle of depression*, in which they view the pyrocone, while using the mouth-blowpipe. This "optical illusion" has the effect of causing the blast to appear *within* the pyrocone, whereas, in reality, it passes over the surface. I have

* Greek, *Phusikè*, Nature; Natural Philosophy.

now before me ten standard works on chemistry and blowpipe analysis, in not one of which is the figure of the pyrocone correctly drawn; but, as it would never do to take up the space of Knowledge with erroneous drawings, I shall here merely insert two; (*a*) from

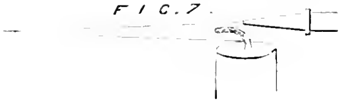
FIG. 5.

FIG. 6.

Bloxam's Chemistry; (*b*) from Thorpe's. In these pictures it will be seen that the "nozzle" of the blowpipe appears to be sending a blast into the *centre* of the pyrocone, which latter is therefore said by most chemical writers to be as hollow as the luminous flame of a candle undoubtedly is; a statement which, as shall be abundantly proved in the course of these "lessons," is also quite incorrect.

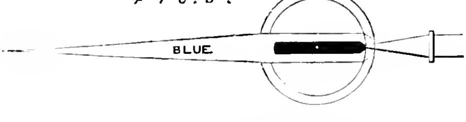
Now, let us light a common candle in daylight, and apply a blowpipe blast to it, near a window, viewing the resulting pyrocone by transmitted light (that is, holding the candle between us and the window) *on a level with the eyes;* or, speaking concisely, in the

FIG. 7.



horizontal plane of vision. This is something like what we see, but by slanting the candle a little, we can see quite distinctly the actual path of the blast on the *upper surface* of the pyrocone; and moreover, we can easily prove, while holding the candle or lamp upon this level, that it is *impossible* to force the blast inside of the pyrocone. Another proof which I take from a former paper (Proc. Roy. Soc., Vol. XX.) is that, in looking directly down (or perpendicularly) on a pyrocone formed upon a thick wick, as that of a

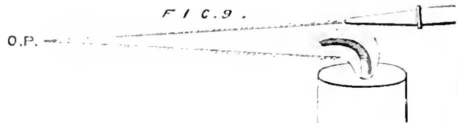
FIG. 8.



Berzelius' lamp, we see the black carbonised wick through the space which is always formed in the centre of every luminous flame, and not through blue fiery matter, as would be the case were the blast in the *centre* of the blue pyrocone. Yet another proof lies in the fact that upwards heat-radiation is almost entirely stopped by the superposed blast.

Simple learners like the poor workmen to whom I chiefly address these little "lessons"—though I myself have quite as much to learn in the matter as to teach—will scarcely credit the fact that none of the eminent authors who have condescended to include blowpipe-operations in their chemical writings, have thought it worth their while to make the least inquiry as to the *cause* of the phenomenon (1) now under discussion. No doubt, if such men as Plattner or Berzelius had done so, speaking as they would, too, *ex officio* (which you must not translate by "as a Jack in office"), they would have at once cleared up the matter; as it is, the following suggestion of an ex-artillery officer must be taken in lieu of anything better. As before, we must rest our inquiry on what that corrupt, old, marvellous judge (I wish we had such judges now-a-days!) and genius, Lord Bacon, called "Induction;" that is, reasoning from experiment. Most young Englishmen possess uncommonly good eyesight, so that they will find little difficulty in proving the truth of the following extract from my smaller book, "Alphabetical Manual of Blowpipe Analysis," page 102. "If the blast from a mouth-blowpipe be propelled by an operator with good eyesight across the heat-undulations rising from a lighted candle in broad daylight by *transmitted* light, he will *see* it (the blast) in the shape of a *straight line*, about the thickness of a fine sewing-needle. If now he propel a similar blast through the blue thin flame of a spirit-lamp, which is penetrable by the blast, and observe it by *reflected* light, that is, when he is between it and the window, he will again see it, but this time in the form of a minute *cone* of air, synaxial (that is, formed upon the same line, as it were) with a flame-cone projected in the same direction, from the spirit-lamp. What causes this difference in the appearance of the same thing? The only reply to this question seems to me to be that a confined, continued blast appears to create round it, in air, a *vortex*, the gyrative rapidity of which is least at the commencement, or greatest *direct* rapidity of the blast, and most when that begins to slacken. This aerial vortex is, of course, invisible in air when the blast is passed through the heat-waves above a candle, although the blast itself is perceived there, because a space is formed by it within which the waves themselves are checked; but the air vortex, on the contrary, is distinctly visible when formed within a coloured (blue) fluid of greater consistence than itself, like spirit-flame. It seems obvious, if this explanation be correct, and the other fact kept in mind, that the blast from a blowpipe is not really projected *into* the candle or oil-lamp pyrocone, but passes over its upper surface, that the vortex thus created includes within its gyrations the flame

FIG. 9.



blown on one side in the direction of the blast, and forces it to retain a horizontal position, as well as its own conical form. It follows from this, that in order to produce a perfect pyrocone, there should be a constant ratio between the strength of the blast (or air pressure) and the size or bulk of the flame acted on—and this is the fact." If the blast be now propelled into a spirit-lamp flame held about half-an-inch in front of the point marked OP in the figure, that is, about half-an-inch distant from the apex of the aërial vortex, a short *inverted* cone of large diameter appears; and this, according to Sir J. Herschel (Essay on Meteorology, page 67) is "a necessary consequence of the vorticose motion." The existence of this atmospheric vortex may be further confirmed by holding about three inches of the *finest* platinum wire, which has very considerable "spring" or elasticity in it, so that one end just touches the *left side* of the blowpipe pyrocone. That end will immediately commence a series of gyrations, from left to right, rapid in direct proportion with the strength of the blast, and *therefore* with the shortness of the diameter of the base of the cone. Another proof is obtained by holding an extremely hot bead—as nearly red-hot as possible—of fresh P. acid just under the base of the pyrocone; when a "mantle" of pale green flame surrounds the blue pyrocone, spreading from base to apex. This "mantle" consists of infinitely small particles of the volatile phosphoric acid burning in the vortex of intensely heated air surrounding the pyrocone. But the most conclusive evidence of all—evidence which seems to me simply confirmatory of the *fact*—is this:—An elastic, expansible cone, such as that formed by an air-vortex, *must*, if compressed at its base, *extend in the direction of its gyrations*. This is a self-evident law of Dynamics,* and I can never forget the delight with which I found, when I placed a globular platinum dish so that its curved bottom almost touched the base of the blue pyrocone, and thus was bound to *squeeze* the air-vortex at its commencement, that my pyrocone instantaneously *increased in length at least half an inch.*

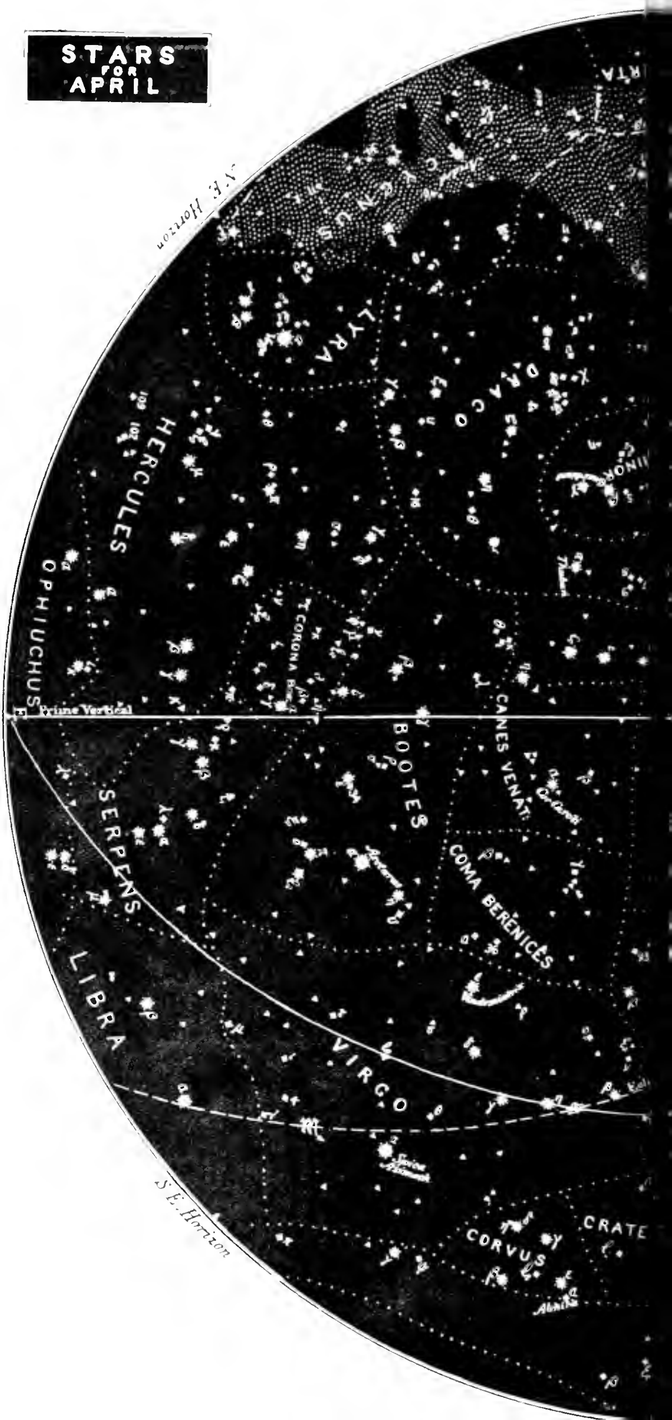
We thus possess in this hypothesis†—for, of course, chemists will not allow, for another ten years at least, that there is here the absolute proof of the existence of these facts which they have obtained of the existence of proportional atoms—a simple and "pretty" explanation, not merely of the conical shape of the "flame" or fire, but of the oxydising properties of the position OP; of the still more intensely oxydising properties of the position PP, where the inverted aërial vortex exists; and of the hydrogenising or "reducing" properties of the position HP; that is, of the inside of the *solid* blue pyrocone, which, according to the present accepted "theory" of blowpipe pyrocones, must be full of oxygen contained in the air of the blast.

But, as the poor ghost in "Hamlet" says (at the beginning, by the way, instead of the end of his oration), "My hour is almost come;" and space permits no more on this head at present, so that I must reserve the discussion of the other chemical and physical effects mentioned, for Lesson VI.

* Greek, *Dunamis*, power; he branch of Physics which treats of bodies in motion, as opposed to *Statics*.

† "Hypothesis, a supposition. Something not proved, but assumed for the purpose of argument" (Ogilvie's Dictionary).

STARS FOR APRIL



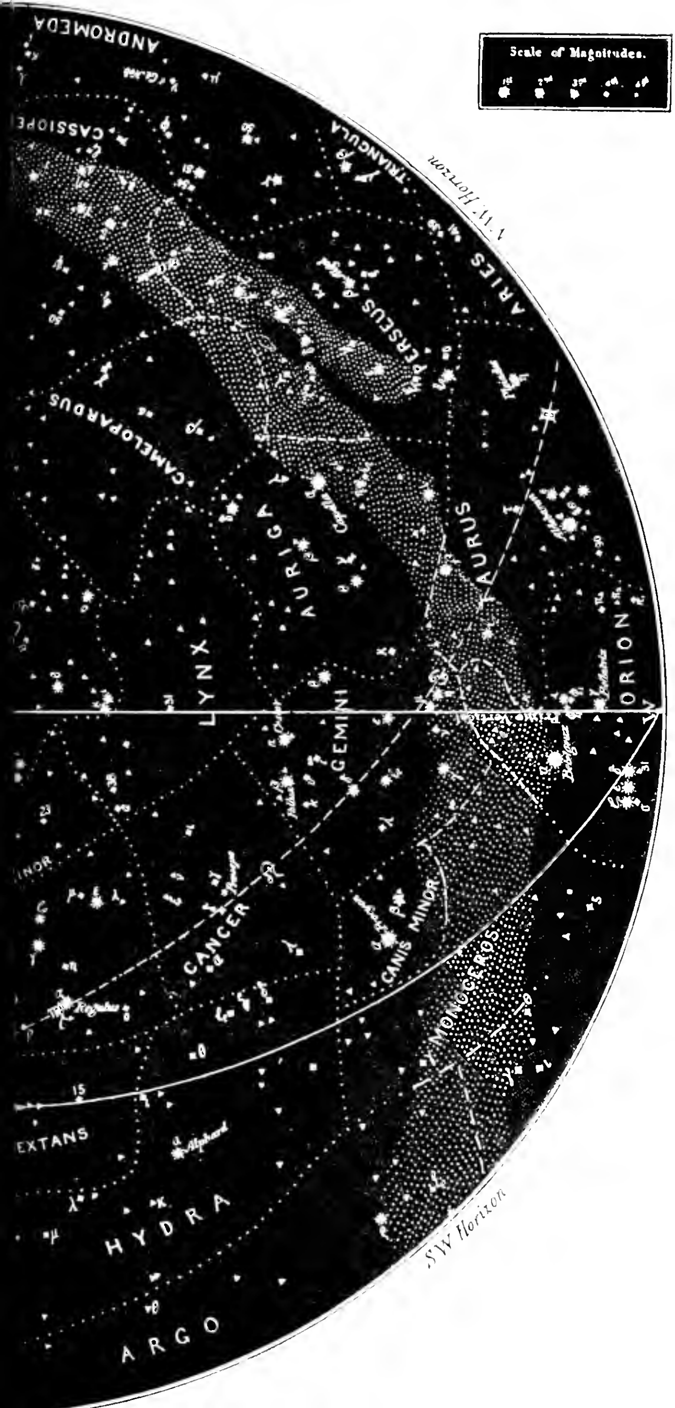
Our Star Map. The circular boundary of the map represents the horizon. The map shows also the position of the equator and of that portion of the Zodiac now most favourably situated for observation. For the motions of the planets Jupiter, Mars, and Uranus, consult the Zodiacal maps in Nos. 11 and 19. The names of ninety nine stars of the first three magnitudes are given below.

On March 31, at 10.30 p.m.
On April 3, at 10.15 p.m.
On April 7, at 10.0 p.m.
On April 10, at 9.45 p.m.
On April 14, at 9.30 p.m.
On April 18, at 9.15 p.m.
On April 22, at 9.0 p.m.
On April 26, at 8.45 p.m.
On April 30, at 8.30 p.m.
On May 3, at 8.15 p.m.
On May 7, at 8.0 p.m.

ARABIC NAMES OF STARS.

The following table exhibits the names of all the stars of the first three magnitudes whose names are in common use:—

α Andromedæ	*Alpheratz*
β ———	*Mirach, Mirac*
γ ———	*Almach*
α Aquarii	*Sadalmelik*
β ———	*Sadalsuud*
δ ———	*Skat*
α Aquilæ	*Altair*
β ———	*Alshain*
γ ———	*Tarazed*
α Arietis	*Hamal*
β ———	*Sheratan*
γ ———	*Mesartim*
α Aurigæ	*Capella*
β ———	*Menkalinan*
α Bootis	*Arcturus*
β ———	*Nekkar*
ε ———	*Izar, Mizar, Mirach*
η ———	*Muphrid*
α Canum Venat.	*Cor Caroli*
α Canis Majoris	*Sirius*
β ———	*Mirzam*
ε ———	*Adara*
α Canis Minoris	*Procyon*
β ———	*Gomeisa*
α Capricorni	*Secunda Giedi*
δ ———	*Deneb Algedi*
α Cassiopeiæ	*Schedar*
β ———	*Chaph*



α	Cephei	*Alderamin*
β	———	*Alphirk*
ι	———	*Errai*
α	Ceti	*Menkar*
β	———	*Diphda*
ζ	———	*Baten Kaitos*
ο	—	*Mira*
α	Columbæ	*Phact*
α	Coronæ Borealis	*Alphecca*
α	Corvi	*Alchiba*
δ	———	*Algores*
α	Crateris	*Alkes*
α	Cygni	*Arided, Deneb Adige*
β	———	*Albireo*
α	Draconis	*Thuban*
β	———	*Alwaid*
γ	———	*Etanin*
β	Eridani	*Cursa*
γ	———	*Zaurac*
α	Geminorum	*Castor*
β	———	*Pollux*
γ	———	*Alhena*
δ	———	*Wasat*
ε	———	*Mebsuta*
α	Herculis	*Ras Algethi*
β	———	*Korneforos*
α	Hydræ	*Alphard, Cor Hydræ*
α	Leonis	*Regulus, Cor Leonis*
β	———	*Deneb Aleet, Denebola, Deneb*
γ	———	*Algeiba*
δ	———	*Zosma*
α	Leporis	*Arneb*
α	Libræ	*Zuben el Genubi*
β	———	*Zuben el Chamali*
γ	———	*Zuben Hakrabi*
α	Lyræ	*Vega*
β	———	*Sheliak*
γ	———	*Sulaphat*
α	Ophiuchi	*Ras Alhague*
β	———	*Cebalrai*
α	Orionis	*Betelgeux*
β	———	*Rigel*
γ	———	*Bellatrix*
δ	———	*Mintaka*
ε	———	*Alnilam*
α	Pegasi	*Markab*
β	———	*Scheat*
γ	———	*Algenib*
ε	———	*Enif*
ζ	———	*Homan*
α	Persei	*Mirfak*
β	———	*Algol*
α	Piscis Australis	*Fomalhaut*
ε	Sagittarii	*Kaus Australis*
α	Scorpionis	*Antares, Cor Scorpionis*
α	Serpentis	*Unukalhai*
α	Tauri	*Aldebaran*
β	———	*Nath*
η	———	*Alcyone* (Pleiad)
α	Ursæ Majoris	*Dubhe*
β	———	*Merak*
γ	———	*Phecda*
ε	———	*Alioth*
ζ	———	*Mizar*
η	———	*Alkaid, Benetnasch*
ι	———	*Talitha*
α	Ursæ Minoris	*Polaris*
β	———	*Kochab*
α	Virginis	*Spica Azimech, Spica*
β	———	*Zavijava*
ε	———	*Vindemiatrix*

COD-SOUNDS AND SCIENTIFIC PRIVILEGE.

WHEN I stated (page 295) that the tough leathery membrane of the cod-fish, known to epicures as the "sound," is an organ of different structure and anatomical relation to the swim-bladder of other fishes, I had no idea that the subject was sufficiently interesting to call forth the critical correspondence it has elicited, and should hardly have prolonged the discussion, but that another and far more important subject has been connected with it. "Old Fossil" says (page 380), that I have illustrated "the old saying that a cobbler should stick to his last," and that on "passing into the domain of the biologist" I must "be regarded as an intruder." Dr. Wilson (page 429) quotes and supports this statement which assumes that scientific inquiry, scientific discussion, or scientific criticism is the exclusive privilege of labelled specialists, who must never invade each others' domains.

This is a mischievous dogma, too often asserted with less courtesy than by the gentlemen above-named, and sometimes even with downright insolence by certain narrow-minded pedants. "There is no man old enough to be an expert in all the sciences, and yet all the sciences are but one science, and all our subdivisions are merely artificial devices for the convenience of study. Hence, if every man confined himself to his own particular branch of special knowledge, the divine unity of creation would remain unknown, and the highest object of all science—the uplifting and purification of the human mind by the unselfish contemplation of the marvellous harmonies of the universe—would be unfulfilled. The new-born science of celestial chemistry could not have come into existence without the previous wedding of the laboratory to the observatory; and if we take a general survey of the progress of human knowledge during the present generation, it will be seen that the greatest strides have been made by those who have boldly stepped across the conventional boundaries that mark the customary subdivisions of the sciences." I wrote the above protest thirteen years ago; I now repeat it with especial emphasis in the columns of KNOWLEDGE, the value of which I regard as largely due to its general freedom from the pedantry of the self-sufficient specialist.

Has not Tyndall invaded the domain of the biologist in conducting his researches on atmospheric germs, and have not the truly philosophical biologists good reason to thank him for doing so? All such biologists acknowledge the importance of Herbert Spencer's profound contributions to the theory of evolution; but can he be labelled a biologist? I need only mention the names, of Humboldt, Bunsen, Kirchoff, Helmholtz, Huggins, Huxley, &c., as illustrations of men who, by forsaking their special lasts, have ceased to be scientific cobblers, and have thereby become true philosophers. This very magazine could have had no existence had its editor submitted to be strapped down to the astronomical last with which his earlier literary efforts are associated, and some of his best essays must have been suppressed had he not invaded other "domains."

Such specialists are unquestionably necessary to the building up of the glorious edifice of inductive science, just as special masons, bricklayers, carpenters, joiners, &c., are demanded for physical buildings, and I should be the last to dispute their dignity and importance, even when protesting against their undue assumptions of exclusive privilege.

Dr. Wilson and "Old Fossil" are quite right in asserting that I am not a biologist, though my earliest studies were biological, and date from a period preceding the invention of the term "biology" and the birth of Dr. Wilson, viz., 1841, when I was a pupil of Professor Jamieson in "Natural History," and attended the lectures of "Monro *tertius*" on "Anatomy and Physiology" in the University of Edinburgh.

"Old Fossil" tells us that he dissected a cod-fish of 8 or 9 lb. weight on the day of writing. I have dissected many twenty-five to thirty years ago, and therefore depend upon memory. The reader, however, may judge for himself, by simply cutting such a fish in half, or asking a fishmonger to do so for him, and he will then be able to judge by the diameter of the blood-clot enclosed by the stout membrane in question whether it can possibly be contained within an aorta of $\frac{1}{12}$th of an inch diameter. If the section is made at about the posterior termination of the abdominal cavity, he will find that this blood-clot is nearly half-an-inch in diameter, and confined between the spines and the thick membrane in question. If he follows this membrane forward, he will find it still adherent throughout its whole length to the spines, and underlapping the blood-clot, which now becomes divided, and lies on each side of the body of the vertebræ, accumulated in the hollows formed by the bases of the vertebral arches. It also contains air, and this has probably led "Old Fossil" to suppose that it is a true swim-bladder, the organ which modern biologists regard as an homologue to the lung-bag of the amphibia and reptiles; one of Dr. Wilson's "found links."

All such bladders differ essentially in structure and anatomical relations from the cod-sound. They are formed of a thin, translucent, delicate membrane, corresponding to the pleura, or membranous envelope of the lung-bag of air-breathers; the cod-sound has a tough leathery coat, like that of our own arteries. It is a contradiction to all anatomical analogies to suppose that a mere air vesicle should have walls strong enough for a fire-hose. The true air-bladders, like the lung-bags of the amphibia, are, as John Marshall says, "off-shoots from the upper part of the digestive canal," and come away freely from the abdominal cavity along with the other viscera when these are removed. The cod-sound is connected with the heart (as a prolongation or modification of the *bulbus arteriosus*) of the fish, and so firmly attached by its edges that it has to be forcibly torn or "söndert," by the Norsk fishermen, after all the rest of the viscera are removed, and thus, as I believe, obtains its name. When not thus sundered for salting, it is ripped open in order to remove the blood which it contains throughout its entire length. I have examined the swim bladder of many fishes—the single-lobed bladder of our common fresh-water fishes; the slippery double bladder of eels; the curious three-lobed bladder of the gurnets (the proportionate capacity of which is at least twenty times that of a cod-sound, and yet is composed of thin membrane); and the still more delicate, collapsing bladders of the herring and other similar fishes—but have never found any blood within them, still less that they enclose the great dorsal clot which I find in all fishes after death enclosed in a special membrane corresponding to the cod-sound, though generally thinner, and always quite independent of the swim bladder.

In spite of the scalpel of "Old Fossil," I still regard the sound of the cod-fish, and the corresponding membrane of other fishes similarly adherent to the spine, as the main bloodvessel of the animal, for the simple reason that its blood is always contained therein, but I do not deny that the air which it also contains may assist the buoyancy of the fish, seeing that this buoyancy is obtained in other fishes by other arrangements than that of the ordinary lung-like swim-bladder.

As regards the contents of this dorsal aorta during life, I may mention one experiment. I have on several occasions lashed myself to the martingale of a schooner sailing in the Mediterranean, and stood so near to the water that my feet have dipped when the vessel pitched. From this favourable position I have speared bonettas, and observed that when one of the five barbs of the "grains" (as the sailor calls the murderous implement), has pierced the adherent membrane in question, that the water all around the fish has suddenly become deeply stained with blood to a distance of a foot or eighteen inches, and the usual blood-clot under the spine has diminished accordingly, indicating more contractile work than could be done by the tiny heart attached to the gills, and suggesting the probability of direct and powerful contraction of this sub-dorsal membrane, which I believe does much more in circulating the blood through the body of fishes than Dr. Wilson and "Old Fossil" imagine. Rymer Jones (who was a biologist especially strong in comparative anatomy) tells us that there is "no systemic heart in fishes, the aorta itself serving to propel the slow-moving blood in its course through the arterial system." Hence the demand for the strength and thickness of the cod-sound, which I commend to the biological attention of the readers of KNOWLEDGE when they next partake of boiled cod-fish. Even though not "accustomed to the use of the scapel," they will then be able to consider the probability of this being merely devoted to holding air, while the "delicate thin-walled tube, about one-twelfth of an inch diameter," which "Old Fossil" found in the 8 or 9 lb. fish, propels the blood throughout its body without any help from the outer tough membrane of the sound.

W. MATTIEU WILLIAMS.

ERRATUM.—In letter on the Radiometer, p. 457, last line but one, for Sodium read Iodine.

COLLIERY "SPOIL BANKS."—You are doubtless familiar with the large heaps of refuse removed from coal-pits, technically known as "spoil banks." These heaps are usually, though not always, on fire, and when seen after dark present an appearance of wild and magnificent grandeur. I have often heard expressions of wonder and admiration from persons who have seen these burning mountains for the first time. It is my habit to look at these "mountains" from a less romantic standpoint, as the sulphurous and other noxious fumes arising from them are doubtless as injurious to animal life as the appearance of the immediately surrounding district proves them to be to vegetable life. I frequently hear the assertion that these heaps fire "spontaneously." Will you kindly give your opinion, through the columns of KNOWLEDGE, as to whether this is likely to be; and, if so, how it occurs? How is the chemical combination necessary to produce flame brought about?—ONE WHO WANTS KNOWLEDGE.

TO CORRESPONDENTS.

Letters to the Editor.

[The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with their writers. All communications should be as short as possible, consistently with full and clear statements of the writer's meaning.]

All Editorial communications should be addressed to the Editor of Knowledge; *all Business communications to the Publishers, at the Office, 74, Great Queen-street, W.C.*

All Remittances, Cheques, and Post-Office Orders should be made payable to Messrs. Wyman & Sons.

⁂ *All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.*

All Letters or Queries to the Editor which require attention in the current issue of Knowledge, *should reach the Publishing Office not later than the Saturday preceding the day of publication.*

(I.) Letters to have a chance of appearing must be concise; they must be drawn up in the form adopted for letters here, so that they may go untouched to the printers; private communications, therefore, as well as queries, or replies to queries (intended to appear as such) should be written on separate leaves.

(II.) Queries and replies should be even more concise than letters; and drawn up in the form in which they are here presented, with brackets for number in case of queries, and the query number (bracketed) in case of replies.

(III.) Letters, queries, and replies which (either because too long, or unsuitable, or dealing with matters which others have discussed, or for any other reason) cannot find place here, will either be briefly referred to in answers to correspondents, or acknowledged in a column reserved for the purpose.

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

"God's Orthodoxy is Truth." *Charles Kingsley.*

Our Correspondence Columns.

SCREW-DRIVER.

[359]—I am almost ashamed to make my first communication to Knowledge on such a humble subject as a screw-driver. But though a humble servant, it is one that we are never likely to be able to do without; and I should think there are few of its employers who have not been inclined sometimes to use strong language at its perversity in slipping out of the nick, and in refusing to enter it. I wonder that it never occurred to me until the other day, when I saw a man continually meeting with this trouble, that it may easily be prevented by the simple contrivance of putting on a tube to embrace the screw-head.

There are, however, a few details to be attended to. Screw-heads, for which the same driver is suitable, are not all of the same size. Moreover, the point of the screw-driver of the now usual round shape (which is the nicest to handle) is widened out, so that a tube which will go over it will be loose on the shank above. The way to meet both those difficulties is to put a piece of leather round the shank while you slip the tube on, of such thickness that it will hold moderately tight. The consequence of that also will be that as the screw-head goes down into the wood, the tube will be spontaneously pushed up. For larger screws than the driver is generally used for, you only want a larger tube and thicker leather.

I never believe in anything that involves motion till I have tried it. I have tried this with a tube made only of strong tin, like an old-fashioned slate-pencil case, and it answers perfectly. You need not even look at your screw. Just put the socketted driver on and turn; it straightway walks into the nick, and stays there until the screw-head is imbedded in the wood, or metal, as the case may be.

I also abhor patents, and I therefore neither patent this, nor tell it to a tool-maker. The last time I suggested a practical improvement (not to a tool-maker, certainly) the man straightway went and patented it. Luckily, however, it turned out not to be new, and so the older makers laughed at him. Edm. Beckett.

COLOUR OF PALÆOLITHIC MAN: CHRISTMAS ROSES.

[360]—There is no evidence that Palæolithic man was black: it is a sheer stretch of imagination on the part of the present writer. But when we consider that black is the common colour of the skin in all the anthropoids, that all the existing lowest human races are black, and that the early Palæolithic skulls of the so-called Canstadt type closely resemble those of the modern Australians, I think we are fairly justified in assuming that the drift-men at least were black and woolly-haired. Whiteness is, after all, a mere acquired trait of the very highest races, and it may well be doubted whether anybody was white till a very recent period, a mere trifle of forty thousand years or so ago. Prof. Boyd Dawkins, indeed, believes that the cave-men or later Palæoliths resembled the Eskimo; but if he means us therefore to conclude they were as light-skinned as those modern people, I think the burden of proof certainly lies with him.

The little cup-like organs in the Christmas rose are really degraded petals; the white petal-like outer leaves are really sepals. The petals secrete honey, and therefore were described by Linnæus as nectaries. It is a common habit of the buttercup family (to which the Christmas rose belongs) to develope coloured sepals for the attraction of insects, and then either lose the petals altogether, or dwarf them into small honey-secreting organs. Green hellebore and bears-foot, two closely allied plants, grow wild sparingly in England, and have the same arrangement of parts as the Christmas rose. The bee alights on the centre of the flower, visits the eight or ten tubular petals one after another, and dusts himself in doing so with pollen from the stamens, which he then carries to a neighbouring blossom. The pistils of each flower mature before the stamens, and so, even if the bee drops some of the pollen on the same flower, it does not interfere with cross-fertilization, because the pistil will in all likelihood have been already impregnated. But when he passes from an old blossom, in which the stamens are shedding their pollen, to a young one, in which the pistils are mature while the stamens are yet unripe, he at once fertilises the ovary, and thus ensures a plentiful crop of healthy seed. Grant Allen.

HIGH NUMBERS.

[361]—A parenthetical remark of yours in an early number of Knowledge prompts me to ask you if you will be kind enough to say what you consider to be the scientific method of notation in respect to very large numbers. That is to say, since a million is a thousand thousands, should we call a thousand millions a billion, and a thousand billions a trillion, and so on; or should we reckon a billion as a million millions, a trillion as a million billions, and so on, multiplying by a million for each one? Or, peradventure, is neither of these methods right? Winter.

[It has always seemed to me the English system of calling a million million—that is, a million to the *second* power—a billion, a million to the *third* power, a trillion, and so on, is sounder than the American system of calling a thousand millions a billion, a million millions a trillion, and so on. No meaning can be given to the *bis, tris,* &c., in the compound word on the American plan, whereas there is a very obvious and natural meaning on the English plan.—Ed.]

JUPITER IN CASSIOPEIA.

[362]—I presume that the Rev. H. H. Higgins, who sets down the statement that "Wallenstein saw Jupiter in Cassiopeia," as an absurd mistake, (*vide* "Answers to Correspondents," p. 392), refers to a passage in Schiller's "Wallenstein's Tod," where the famous warrior is made to say (Act V. sc. 3):—

"Kein Sternbild ist zu sehn! Der matte Schein dort,
Der einzelne ist aus der Kassiopeia,
Und dahin steht der Jupiter."

If such be the case, I hope you will allow me to state that the error is not Schiller's but must be laid to the charge of his commentators and translators, who have construed the word *dahin* as if it meant "therein," whilst it is employed in the sense of "yonder:" for the speaker points with this expression to another direction than that where he had seen Cassiopeia. I may add that I have interpreted the above passage in the same sense in my commentary to Schiller's "Wallenstein."

C. A. Buchheim, Ph.S.

[Does Dr. Buchheim mean that the interpretation he puts upon Schiller's words is the one which would naturally be put upon them by German readers?—Ed.]

SPACE PARADOX.

[363]—I fancy the following problem might interest your readers. Two bodies, A and B, have been travelling in the same path through space from eternity. B goes a mile an hour faster than A. Since they have been travelling for an infinite number of hours, there is an infinite number of miles, or any other unit of length between them. Hence a line drawn from A to B will be infinite, although bounded at each end by A and B.

Would you be kind enough to give an explanation of this apparent paradox? If this is not one, is it ever true that the conditions of human thought involve contradictions where it transcends the finite? Of course, Kant says that our ideas of space and time are forms of thought founded on no realities. But apart from this hypothesis, can a logical explanation of such problems as the above be supplied? Perhaps this might be combined with the suggestion of J. S. T., in letter 295, if, as I and many others hope, you are going to give an article on space. Eton.

[This paradox resembles Aristotle's proof of the finiteness of space, which never satisfied any one (as Sir J. Herschel remarks), though unanswerable,—viz., Since whenever we take any two points in the universe, the straight line joining them is finite, the universe itself is finite. But as this depends on the first postulate of Euclid, so the answer depends on the second;—Each of Aristotle's finite straight lines can be produced to any distance in the same straight line; therefore the universe is not finite. Lastly comes the third postulate, which since it asks us to admit that a circle can be drawn having any centre and at any distance from that centre, corresponds with Pascal's saying, that the centre of the universe is everywhere, its circumference nowhere.—Ed.]

AN IDIOT DOG.—AN EXCEPTION PROVING A RULE.

[364]—I once possessed a black curly-haired Newfoundland and retriever, weighing 125 lb., and standing 2 ft. 4 in. high, as you would measure a horse. Despite his beauty and magnitude, he was, however, if not an idiot, certainly the least intelligent dog I ever saw. As a yard dog he was quite worthless, except that his amazing size terrified alike the honest and dishonest—*i.e.*, in daylight, for let come what or who would, he opened not his mouth. In the house he was equally crass. In his movements he seemed to have but one idea, and if a table or a child were in his way, down they would go—Lion cared not. If taken out of doors he would go straight ahead, neither knowing his master nor his way home, his sole and great delight being to slay any dog approaching him in size.

I have recently read a translation of a work on "Mind in Animals," by Ludwig Buchner. Although he only describes ants, bees, wasps, and spiders, yet he succeeds in annihilating the old "instinct" superstition. Students of the subject of mind in animals should read this book. Joseph Wood.

WOOD-GAS.

[365]—I repeat that carbonic anhydride can be perceived by the organ of taste and by the organ of smell. If "F. C. S." considers that the taste and odour are due to impurity in the CO_2, as is the case with hydrogen, will he state what this impurity is? The flame of CO was observable as I wrote, over a red-hot fire without flame, which I call a "sluggish fire." The process of CO from CO_2 and heated carbon is nothing new; it is to be found in many text-books. The main point, after all, in the use of such gas, would be the danger of an escape. Lewis Arundel.

NOTES ON ROWING.

[366]—"Notes on Rowing" are very acceptable, and, happily, promise more. Rowing, too, is not the only instance of propulsion by pressure upon a lever of the second order. The tractive force of a locomotive engine can be explained in the same way. To draw a parallel, one might say that the rail corresponds to the water, for it is the fulcrum. The axle-boxes are the rowlocks; the crank-pin is the "spool" of the oar, and the periphery of the driving-wheel is the "blade." Each end of the cylinder (which, by the way, sits like a rower, forward of its work) is in turn a "footboard" against which the steam reacts with a force approximately equal to the pull or thrust upon the crank-pin. And, to make the parallel complete, I may say that the "slip" of a driving-wheel when an engine is running at high speed is at the present time the subject of experiments. Of course, the action of a driving-wheel when the crank is below the axle has no parallel in rowing, but it is a good exercise for the student of mechanics to prove that the forward pressure upon the engine is the same on each stroke, in spite of the difference in leverage. Lastly, the motion of the piston of the engine corresponds pretty accurately with the motion of a rower's body—backward and forward with respect to the boat, but continuously forward (although with a varying velocity) with respect to the water. Take any point in the periphery of the driving-wheel, too, and see how it goes from a condition of rest when in contact with the rail to acquire a velocity equal to double the velocity of the train when at its highest point.

In the same way, the oar blade, from being nearly stationary in the water, suddenly leaps forward with a velocity (that is, if nothing happens) at least double the boat's speed. Is the parallel complete?—Yours, &c., A. N. S.

ELECTRIC TELEGRAPH.

[367]—Ronald's electric telegraph (see Knowledge, XIX., p. 401), is by no means the oldest known piece of telegraph apparatus. Professor Sommering (I am not quite sure of the spelling of the name), of Munich, constructed, in 1809, an *electric telegraph*, which is preserved in one of Munich's numerous museums, and was originally laid down between Professor S.'s laboratory and that of one of his learned colleagues. A description and drawing of it are given in "Das Buch der Erfindungen," publ. Otto, Spamer, Leipzig. I give an extract in the following:—

There were as many circuits as the alphabet has letters, further figures, signs, &c., between the two stations. Each of the circuits was an apparatus for decomposition of acidulated water, by means of the galvanic current. The tubes containing the water represented letters, &c. The rising of gas bubbles in a certain tube indicated to the receiver a certain letter. There was only one battery, which could be connected to all circuits by means of switches. There was also a cleverly-designed signalling apparatus to attract the receiver's attention. One tube contained a glass bell, under which the poles were situated, gas evolution would drive out the water of the overturned bell, and cause it to rise, move a lever, and set a clockwork going. This is written after a lapse of years since I read the description, and there may be some slight incorrectness in the details, but I have given the general idea correctly.—Yours, &c., F. Stern, D.Ph.

VEGETARIANISM.

[368]—I wish to make a few remarks on the letter 207, p. 251, in which the writer politely calls vegetarians "amiable fanatics," and further adds that they seem determined to force their way into the columns of Knowledge. This is a very grave charge indeed. Will the writer kindly state the number and date of Carper's Quarterly Journal, in which Dr. Wald's statement about the prisoners in a castle at Waltenburg is to be found: for in the interests of truth and humanity this case requires careful investigation. For example, it would be important to know whether the bread given to the prisoners was white, and if much salt was taken with the leguminous food mentioned.

It might be also useful to know the proportions of meat to vegetable diet partaken of, as a rule, by the writer of letter 207.

So far as my knowledge extends, I find that the greatest intellects of the present and the past have been either altogether or almost akreophagous.

As I write for the sake of information and not of controversy, I am thankful that the columns of such an excellent paper as Knowledge are open to the discussion of this important question—viz., What is the best food for the million?

Why should abstainers from flesh, fish, and fowl be honoured with so many conflicting titles? In the few numbers of Knowledge, in which the subject of vegetarianism has been mooted, I find them described as "phytophagists," devourers of "potatoes and turnip-tops," "eaters of greens," &c. C. L. Porcher.

TELEPHONE.

[369]—I have been much interested in "G. E. V.'s" description of the telephone, and should like very much if he would follow it up by an account of the microphone transmitter. In making the telephone, "G. E. V." does not say how much wire he puts on his bobbins, nor the number of it. Would he oblige by stating what length and number he considers most suitable? W. B.

Notes on Science.—The former pupils of University College School have raised a fund for the encouragement of science amongst the boys at their old school. Besides an exhibition for practical chemistry and a prize for experimental physics, they have founded a medal, which will be awarded, at most annually, for original work of sufficient merit in any branch of experimental science done within a stated period of leaving the school. The medal, for the design of which the contributors are indebted to Mr. Thomas Woolner, R.A., will be exhibited in the Royal Academy this year, and a copy will be deposited subsequently in the British Museum. It is not for us to criticise the work of that distinguished sculptor; those who have seen the copy belonging to Mr. Temple Orme, pronounce it to be one of the finest medals ever struck.

Queries.

[312]—G[illegible].—Will any reader kindly inform me how I could separate [illegible] from paper? It is daubed from the brush which has been [illegible] distance. I enclose specimen.—PUZZLED.

[313]—[illegible].—Why does the lobster-shell turn from blue-black to red w[illegible] boiled?—A. D. G.

[314]—[illegible].—Is there any explanation of the inability of a [illegible] to draw the electricity from a charged electroscope?—Boz.

[315]—CRYSTALLISATION.—Will someone in KNOWLEDGE tell us something about dendritus moss agates or frost on our window-panes? How it is they so much resemble the growth of plants?—R. W. R.

[316]—SINKING FUNDS.—Are there any tables published showing what sum per annum must be paid in order to repay a loan with interest, so as to clear off both principal and interest in a certain number of years by equal instalments, and what is the formula for arriving at the result?—A. N.

[317]—CARDS.—We have had an argument on the question as to whether it is proper grammar to say "Club" *is* trump or "Clubs are trump," or "Clubs *are* trumps. Would the Editor, or any reader, kindly decide the question?—R. P. S.

[318]—NON-CONDUCTOR.—Would some kind friend assist me in this matter? I want to know some ingredients to make a kind of paint that will be a non-conductor of heat.—J. H.

Replies to Queries.

[218]—SEA-BLUE BIRD OF MARCH, "*underneath the barren bush,*" is explained by an ornithological friend to be probably the blue-tit (as shown in a Christmas card). The lines quoted by "Oudeis" refer to the kingfisher, but why should the other lines?—W. W. F.

[273]—STRENGTH OF MATERIAL.—In reply to "F. M.," the distance, 8 feet, is measured horizontally between the rail and the pivot; 9·17 feet is the length of the *slanting* line shown between rail and top of pivot. To make Anderson's figures correct, the surface of rail on which roller runs must be sloped so as to touch this line, the roller being, of course, set at a corresponding angle. When this is done, "F. M." will see at once that the force acting on pivot is no longer vertical, but in the direction indicated by the arrow to the left of diagram, marked 82 tons. The resolution of this into vertical and horizontal components is shown correctly. "F. M.'s" figures are correct for the diagram as it stands.—C. H. WINGFIELD.

[276]—PHOTOGRAPHY.—1. In reply to W. E. F., he will find it much better to purchase collodion. Price according to quality, say from sixpence per ounce. 2. The thin metal plates can be purchased at any shop where photographic materials are sold; but neither collodion nor plates are of any use without a lens, camera, and other requisites; also some experience in their use. 3. The crystals referred to are probably prosulphite of soda.—A. BROTHERS.

[307]—"Descriptive Geometry," by Edgar and Pritchard, 3s. 6d. or 4s. 6d. (Macmillan & Co.); also Angel's "Practical Plane and Solid Geometry" will be helpful (Collins' "Elementary Series," at 1s.). Don't attempt the subject without models, which can be made of pasteboard, strings, and wire. You would not study plane geometry without diagrams.—A. H. H.

[310]—QUARTZ IN COAL.—Veins of quartz are frequently found in shales and coal. Such veins appear to have once been chinks, or small cavities, caused, like cracks in clay, by the shrinking of the mass, which has consolidated from a more or less fluid state, or has simply contracted its dimensions in passing from a higher to a lower temperature. Siliceous, calcareous, and occasionally metallic matters frequently have found their way into such empty spaces by infiltration from the surrounding rocks. And see Lyell's "Elementary Geology," 1855, p. 627.—LEWIS E. EMMET.

[311]—HEATING ROOM.—"J. W. B." asks me whether there is any sanitary objection to heating a room with Bunsen burner, without stovepipe or vent. He evidently supposes that "with a perfectly blue flame" the atmospheric burner has some sanitary advantage over ordinary burners. This appears to be a commonly prevalent idea, but it is without foundation, provided always that the ordinary flame with which the Bunsen is compared is burning properly, with ample supply of air. Both supply the products of combustion of bisulphide of carbon and the other impurities of coal gas, while a paraffin lamp or candle, burning a pure hydrocarbon, only produces carbonic acid and water. A bad burner may supply a smoky luminous flame, and even a Bunsen may go wrong if it lights inside the tube.—W. MATTIEU WILLIAMS.

[312]—PYROLOGICAL.—Colonel Ross has given his address in KNOWLEDGE, No. 17, p. 351, for the convenience of pyrological inquirers, to save the editor and printers the trouble of inserting inquiries on this head, and to avoid replying to anonymous communications, which he declines to do.

[313]—BOTANICAL.—Jemima will find just the things required in "Manual of Botanic Terms," by M. C. Cooke, publishers, London; Hardwicke & Bogue, price 2s. 6d. This little work is eminently useful for reference.—AMATEUR BOTANIST.

[317]—SEAL FISHERIES.—Seals are skinned (in a few minutes), their carcases are left (alive) on the ice until the cold or death puts an end to their sufferings. If allowed space, I will give you the details of a day in Greenland. Some call it sport, but I have another name for it.—T. D. RENNIE.

[320]—BOTANY.—If "F" requires an inexpensive work, I should recommend him to procure "Lindley's School Botany." It is a capital book, giving an insight into "Structural Botany." If he intends making a collection of wild flowers, he will find "John's Flowers of the Field" (profusely illustrated) an extremely useful book.—J. C. L.

[326]—DORSETSHIRE "VALLEY TERRACES."—These are the ramparts of the ancient "camps," or fortified towns, for which Dorset is remarkable. Several are very prominent objects from the railway.—R. N. WORTH, F.G.S.

[329]—PHOSPHORESCENCE OF FISH.—It seems most probable that this phenomenon is due to the process of putrefaction, the decomposition by oxidation of the bones, of which phosphorus is an important constituent. It is observed in the decay of all animal substances, but more especially in the case of fish; so that in the instance of the cured haddock, chemical action would take place in a sufficient degree to liberate the phosphorus in small quantities, forming P_2O_3 with the O of the air. From recent investigations, it has been set forth (I believe by M. Chappuis) that phosphorescence is due to the generation of ozone.—R. C. F.

[332]—MOSSES.—Let "Eupteris" try Wilson's "Bryologia Britannica;" it is an excellent work on British mosses.—ALEXANDER BLAKE.

[333]—SAKKARA TABLET.—This list of the Kings of Egypt was discovered by the late M. Mariette about 1863, and is now in the Boolak Museum. It was found in the tomb of a priest named Tounari, who is represented rendering homage in the name of Rameses II., in whose reign he lived, to fifty-eight of the monarch's ancestors. Among these are a large number of names, especially of the first six, and extending to the nineteenth dynasties. This invaluable list was supplemented in 1864 by the so-called second Tablet of Abydos, also found by Mariette, which, with the sixty-four names of kings engraved on the "Hall of Ancestors," from Karnak, at Paris, and the first Abydos tablet in the British Museum, made up the monumental lists of the kings. M. Maspero's arrangement of these is to be found in his "Hist. Ancienne des Peuples de L'Orient," published by Hachette, of King William-street. Any recent history of Egypt will give an account of the tablet, as also does Lenormant in his "Ancient History of the East," published by Asher; for a complete commentary see De Rougé "Monuments des six Premières Dynasties.—A MEMBER OF THE SOCIETY OF BIBLICAL ARCHÆOLOGY.

Answers to Correspondents.

*** *All communications for the Editor requiring early attention should reach the Office on or before the Saturday preceding the current issue of* KNOWLEDGE, *the increasing circulation of which compels us to go to press early in the week.*

HINTS TO CORRESPONDENTS.—1. *No questions asking for scientific information can be answered through the post.* 2. *Letters sent to the Editor for correspondents cannot be forwarded; nor can the names or addresses of correspondents be given in answer to private inquiries.* 3. *No queries or replies savouring of the nature of advertisements can be inserted.* 4. *Letters, queries, and replies are inserted, unless contrary to Rule 3, free of charge.* 5. *Correspondents should write on one side only of the paper, and put drawings on a separate leaf.* 6. *Each letter, query, or reply should have a title, and in replying to letters or queries, reference should be made to the number of letter or query, the page on which it appears, and its title.*

STUDENS. Our edition of Ganot later than yours, and cannot identify the passage. However, if $n(\gamma+\delta)=\alpha+\beta+\gamma+\delta$, it follows that

$$(n-1)(\gamma+\delta)=\alpha+\beta$$

How you "eliminate $(\gamma+\delta)$ from both sides" passes my comprehension. If you have 10 marbles and 10 tops, and are told they are equal in value to 3 shillings and 6 pence + one marble and one

top, how are you going to eliminate the marbles and tops from the statement? Do you not at once learn from it that 39 marbles and 39 tops are worth 3s. 6d.?—Boreas. (1.) Axial rotation of earth constant all the year round. (2.) No evolutionist supposes there were germs of life in the earth during its incandescent condition.—C. J. C., S. C. Sargisson, Philo, Sympathy, C. H. Oliver, G. F. S. Call, Dispenser, G. Gordon, H. W. Z., Peter Parley, J. Harkinson, G. W. R., M. N., L. Lucas, and lots of others, have answered queries about "Burial of Moses," Ferrier's Influenza Powder, and plants in bedroom.—H. A. Bulley. (1) We are not, so far as I know, approaching another glacial period. (2) Inclination of earth's axis remains almost unchanged. (3) Present condition in sense that earth is nearest to sun in winter of northern hemisphere, will be reversed some eleven thousand years hence; but whether that would be favourable to glacial northern epoch or not, is open to question.—Ashbridge H. Robinson. Certainly not for reason named that Mr. Grant left out date of Stone Age period. Have you seen any, even the slightest trace of unwillingness on our part to put facts as we find them, because they do not square with preconceived ideas (whether relating to facts, fancies, or faiths)?—Fifty thousand years ago, eccentricity of earth's orbit was less than now (0.0109; now 0.0168). Longitude of perihelion, 28° 36', so that earth was nearest to sun towards end of October. This was not the glacial epoch, or time of Stone Age. For a time when eccentricity was very great take 850,000 years: eccentricity 0.0747 (and longitude of perihelion 239° 28'—earth nearest to sun towards end of August. Very glacial time for northern hemisphere then.—Dr. Llitton Forbes. Many thanks: a most interesting subject.—J. L. Sinclair. Theory that satellites were expelled from primaries by explosive force will not bear examination.—F. C. It will make no difference which way you put the lens.—W. J. L. (1) Ophite is a synonym of serpentine, but also sometimes applied to speckled green porphyry. (2) Mr. Brough was librarian at the London Institution, successor of Brayley in the office, and predecessor of Mr. Nicholson, now Bodleian librarian. He was a well-known writer on scientific, chiefly chemical, subjects.—F. H. S. About the eye—the experiment proves nothing as to inversion of image. We know from optical laws that the image must be inverted. The other questions too wide.—such queries remind me of the lady who, just before supper, at a ball, said to Humboldt: "Now we have five minutes to ourselves, do tell me all you know about the earth." Sir R. Phillip's idea sheer nonsense.—Geo. E. Newton. Thanks for the pamphlet against vaccination. I do not wonder that no notice has been taken of it by Dr. Carpenter. Ill-written, ill-reasoned, and offensive, how does it deserve notice?—J. McDowell. Probably Sir Henry Thompson meant the term vegetables to include fruit. Can one say that it does not? What are tomatoes, pumpkins, vegetable marrows, and so forth? Consider again the lines—

> Amidst them stood the tree of life,
> High eminent, blooming ambrosial fruit
> Of vegetable gold.

Does not this seem to imply that Milton at any rate thought fruit a vegetable production? Science thinks so, too. You would "define a vegetarian as one who excludes from his articles of diet flesh, fish, and fowl;" surely strict vegetarians would exclude eggs, milk, butter, cheese, and so forth. These are distinctly animal food. Then again, do you not rather cavil about words than facts, when you dwell on his remark about debarring ourselves from the *right* to use such and such food? "Surely he knows," you say, "that vegetarians do not debar themselves from the right," &c. Might he not reply that, in holding it to be wrong to use vegetable food, they *do* debar themselves from the right to use it? It seems to me that Sir H. Thompson's letter was eminently fair and judicious, and conceived entirely in the spirit of his own doctrine, that it is unwise to conclude over hastily that what suits ourselves must necessarily be the best for others. If your letter had not been so very long, it would have appeared.—R. H. I doubt if any reader could tell you of a way by which you could successfully make a Rainband spectroscope.—Liverpool. The zodiacal light is certainly more than the mere radiance from the sun after he has set. It is very seldom distinctly seen in our latitudes. When seen (in February or March in the evening, and in the autumn in the morning), it appears as a slant cone, the apex being some 80° or 90° from the sun's place. It is incorrectly depicted by Dr. Phipson, in his excellent little work on meteors, as a vertical cone. It is best seen an hour or two after sunset.—F. Pratt. You will pardon my remarking I did not "permit the planets to be described here as the cause of weather changes." I distinctly said that I must draw the line somewhere, and I drew it just there.—E. P. C. Geologists estimate the thickness of strata (without digging directly down through the depths they name), by noting the thickness of successive layers, as they crop up to the surface, and adding these thicknesses together. I do not myself think that the moon's mass was thrown off in a single effort, so to speak, thinking it more probable that the moon was first formed of multitudinous small bodies separately thrown off, like spray, from the top of the great tidal wave. I think the rings of Saturn represent this earliest or embryonic stage of the moon's history.—S. Hall. The star you refer to just below "the Little Bear" is Vega. You will be able to find the place of the planets now visible to the naked eye, from the zodiacal maps in Part III., except Saturn, which you will find about as far to the right of Jupiter as Mars is to the left.—A. M. S. Queries noted.—The Village Philosopher. One of the ways you mention must certainly be used to find the error, unless you resort to Logarithms.—A. G. S. Your cat story is rather long: besides, the cat which follows the sweeper may "wish to gain some private ends."—N. Have already noted that that query about noise in trams was lost in postal transit.—Canvass. Thanks.—Alex. Allan. The only book I know of that would suit you is "Chauvenet," in two volumes. It is expensive and rather difficult. For the other purpose you cannot do better than use "Guillemin."—Gravatt. Shall probably write on the subject of space.—Daniel Jackson. I believe no small part of the success of Knowledge has been due to just those sections (Whist and Chess) which you wish to see removed. If your own editorial experience had been half as satisfactory as mine, you would understand my unwillingness to make any changes. The contributor to whom you refer is well able to tell me himself, if dissatisfied. As he has not, I cannot but think your reference to what he said when at Hamilton a breach of confidence. Is mischief-making one of the devices which your editorial experience suggests as desirable for extending the circulation of a magazine? On the whole, I am inclined to believe I know how to conduct this paper at least as well as you can teach me.—Jos. Baxendell. I did not insert your reply to Mr. Ranyard, because it seemed written under the impression that he meant to offend you personally, which I am sure was not the case. Your remarks to says at had better not be inserted, because readers might imagine they were meant to offend me personally, which I am sure is not the case. Still, they are digressive—if not aggressive. I shall be glad to publish any arguments you may wish to advance. In the meantime, I do not see how you can deny that if there are five equations in each of which cosmical elements appear on one side, while one and the same number 1881·59 appears on the other, there can be formed no less than ten equations (the number of combinations of five things two together) having cosmical elements on both sides—giving, therefore, ten instances of remarkable but quite extra-pyramidal coincidences. You say you do not know what "Fudging" means, and that a young schoolboy friend, whom you have asked, cannot tell you. Perhaps in the North the term is not used. It means humouring numbers or facts so as to get some result you want. For instance, the answer to a sum should be, let us say, £1. 13s. 4d., and a schoolboy gets £1. 13s. 1d.: well, he puts the right answer in the proper place, and alters several of the preceding steps to correspond, taking his chance that the teacher will only glance over the few first steps and the few last, not finding out either the original error, or the place where it has been corrected by an artificial error. That is one form of fudging. For other instances, take your quiet assumption that the sun's real diameter has a certain value fitting in with your other results, or your other quiet assumption that the number 1881·59, which comes out among cosmical relations, is the exact length of the Great Gallery.—Wm. Wilson. Well, since you ask me, I do think your letter in singularly bad taste. I "need not" (need I not?) "trouble to reply." No trouble whatever, my dear sir; as you assume the office of censor, I venture, following suit, to tell you that, while there is no irreverence at all in what I said (quoting Shakspeare), there is gross irreverence in the way in which you, to all intents and purposes, attribute to the Almighty the susceptibilities of a soured pedagogue. *Dat veniam corvis* (W. W.), *vexat censura columbas* (R. A. P.).—C. F. B. Hairston. Had not heard that "cats lie on their heads, with one or both ears on the ground, when it is going to rain." It may be the case; but I wonder how it is done. The "sun entering a sign" is quite different to the sun being in a "house"; the former relates to the sun's passage along the ecliptic, the latter to his position in these imaginary divisions of the heavens with reference to the horizon on which astronomers base their predictions. When astrologers said a person was born under a particular planet, they meant that that planet was in the ascendant (the part of the heavens about to rise above the horizon) at the time of the person's birth.—C. Do you call the series $(2 \times \frac{2}{1}) + (4 \times \frac{3}{2}) + (6 \times \frac{4}{3}) + (8 \times \frac{5}{4}) + (10 \times \frac{6}{5}) + (12 \times \frac{7}{6})$, geometrical? The definition of a geometrical series is sufficiently precise, and certainly does not include such series. Surely "Teacher" is justified in his criticisms.—X. Surely you are not in earnest in asserting that the oar is not a lever of the second kind. If the object of the oarsman was to push the water along, the oar would be a lever of the

first kind, but as the object is to move the boat, and as the boat's weight is urged from the rowlock, the rowlock cannot possibly be regarded as the fulcrum; for the fulcrum in every lever is the *point d'appui* from which the weight is lifted. ERIN. The dates and hours mentioned beside the monthly star map, mark the times at which the stars are in the position shown in the map. The map, of course, can be used at any time within an hour or so before and after the times named. Your suggestion noticed; you will find we are not at all inattentive to suggestions intended to enhance the usefulness of the maps.—GRAVATIM. We know of no general solutions for such problems as "Find two consecutive integers > 100, which shall contain no factors other than powers of 2, or 3, or 5," &c. Such questions are scarcely suited for our columns. E.D.G. Queries about plants in bedroom, scientific terms, and salt already sufficiently answered. F. J. STEVENSON. We intend soon having some papers on the Arabic names of stars. ONE NOT CONVINCED. (1.) Have you ever tried to bring a plumb-line into line with a star, from a distance of (say) 200 yards? You will find it easier to talk of than to do. (2.) A pie-dish full of water would have reflected Alpha Draconis as well as a body of water in the plugged-up hole; so also a passage an inch in diameter would have shown the star as well as the four-feet-square descending passage,—*if* you chanced to get it in the right direction. The difficulty is to do it. By taking only a small reflecting surface you produce precisely the same effect as if you diminished to corresponding degree the cross-section of the descending passage. With water poured in, as I have suggested, the ascending passage would be equivalent optically to the prolongation of the descending one. 3. You "cannot debit such a waste of material as the grand gallery, used for so short a time, to men otherwise so clever as the Pyramid builders." How are you going to avoid the difficulty? It applies far more to Smyth's theory. You might have waited till my theory was fully described. The question you asked over name "Bumberam" has been asked, and, by the way, has not been answered. A spinning peg-top presents many difficulties.—M. H. C. Thanks; but that was not what Newcomb meant. He has admitted the erroneous nature of his explanation.—JAS. CRAIN. Thanks for magic cube; will put it by in the hope that ere long I may use it.—JAS. GRUNDY. Many thanks.—L. W. H. When you look at a landscape in an inverted position (best not in public) a part of the retina receives the image which in the ordinary attitude is not used and is therefore more sensitive.—ECLIPTIC. There is no general formula by which magnitude of eclipse and times of beginning and ending for any place can be computed.—W. M. Science cannot yet explain the qualities which the ether of space seems to possess. Read Herschel's Essay on Light (Familiar Science Lectures).—W. A. C. Mr. Hutton does not assert, but admits that were animals not used for food many must starve. He is intensely humanitarian, some think extravagantly so. Of every class of animals, including man, many times as many are brought forth as could possibly coexist. Some *must* perish. D'ARTAGNAN. It is a matter of opinion. It seems to me unnecessarily confusing to describe same number repeated several times as an arithmetical and also a geometrical series; and to ask what proportions of gold, silver, and copper exist in an alloy, when the equations show that there is no copper at all. The purpose of examinations is to ascertain what the examined know, not to perplex them.—W. F. See our monthly papers, beginning with No. 21.—J. S. P. Paper on the Transit of Venus soon. Have written no work on Practical Astronomy.—E. GREATOREX. We scarcely take such matters as part of our regular subjects. For general chemistry provision already made. Other questions answered.—K. N., Lepus, J. Minor, C. A. E., Ripple, Alphard, Knowledge, G. P. Benstead, Carryl, S. S. S. S., Myrven, F. M., Amateur Reader, T. Boyer, D. A. N., Lomax, Boy, P. P. J., Scientia cum Legibus, Novice. Questions either too vague, or trivial, or unsuitable, or already discussed, or for other reason inadmissible.—ANTI-TARANAKI. Thanks.—W. S. Yes to both questions. THEO. JAMES. Already answered.—G. H. MORTIMER wants G. E. V. to tell him how to slip the bobbin of wire over magnet, and what size wire is.—C. C. C. Newton's theory of comets' tails would only account for a certain definite rate of emission, just as known density of our air and known force of gravity gives a certain calculable rate for rising of a balloon in still air. Comets' tails are formed at a much greater rate. C. HARVEY. Questions have been referred to botanist. Cannot find space for replies so lengthy.—W. H. HARLAND. Thanks, but coal-sack holes now disposed of, and space crowded.—J. C. L., T. J. WOODROW, and others. Your stories are interesting, but, unfortunately, many subscribers consider enough said for present about intelligence in animals.—E. COX. Observe the influence of your protest. J. C. L. There *has* been a good deal of it.—MORE LIGHT. Will see about it.—R. HUMPHREYS. Writer of articles on Brain Troubles not acquainted with the hymn tune "Midian;" the addition to the echo chords he devised himself. OMICRON. What is Rhyolite?—R. S. STANDEN. Trembling of fixed stars due to "movements" in our air, not moisture, as misprinted, p. 112. Planets do not tremble, because they have discs; stars are appreciably the merest points.—HOWARD WILLIAMS. Letter forwarded to printers. No, you did not mention our former acquaintance, and I therefore inferred you were only a namesake of the H. W. I knew at St. John's. THOS. MACTAGGART. Thanks, but no space. J. GIBSON. Oh, but excuse me; the squares of $+a$ and $-a$ *are* equal.—CORNWALL. Thanks; query referred to electrician. H. A. N., J. M., ARTHUR BUCKHEIM, BRENTON, KIT, and others. What is the logarithm of a negative quantity? The logarithm of a number to a given base is that power to which the base must be raised to equal the number. To what power must any given positive number be raised to make it negative? It seems scarcely worth while to discuss a paradox thus arising from misuse of a function invented for a special purpose.—C. HARRIS. Paradoxes well known. J. MCGREGOR ALLAN. If you could only put your objections against vegetarianism into smaller space.—W. H. WOOD. Question answered.—PHOSPHOR. Germ theory not inconsistent with evolution. One of your other queries inserted. When one correspondent sends six queries at once we begin to think of closing the Queries column.—E. M. (Cantab). When the theory has been established it will be time to consider how it was originated.—HALLYARDS. Axis of Venus does not point towards sun. No; I think you saw what you thought you saw, but that what you saw was not what you thought it. You are quite mistaken in attributing the origin of the usage you mention to KNOWLEDGE. It has been customary for years, in press, pulpit, and lecture-room, though of course not customary with all. It has been adopted in my own case repeatedly. Your paper about jelly fish is in type. Will you excuse me if I hint that you have evidently much more leisure than I have. If one correspondent in twenty wrote at such length, or if our circulation increased twenty-fold, as we hope it will, what could a poor editor do? Replies to such letters as yours would in that case fill all our space.—M. B. ALDER. Pardon me. I by no means welcome Dr. Siemens' theory. I have given Dr. Carpenter's report of it; but it seems to me (I venture to say, *it is*) utterly untenable. There is no such centrifugal tendency as he imagines, and suns cannot at same time do the work he describes and shine through interstellar space as they do.—M. WYATT. You don't explain why you take 22 to the power 5, and not 21 or 23, or some other number between 20 and 30.—ZARFS. You have attacked too difficult a subject.—HERBERT PUCKLE. Thanks; but we would rather not encourage such kindness. Your book might not be returned, and we should feel (though you, no doubt, would not hold us) responsible.—W. H. PICKUM. Theory too vague.—W. SMITH. It would be much more convenient if two of the 31-day months gave up one day each to February. But in such matters the human race is very slow to change.—JOHN CARTER. I was thinking rather of cases where the training from the beginning had come into the teacher's hands; in other words, of cases in which a parent had been the teacher. I can well believe there are cases where bad home training leaves the teacher little choice but to use some form of corporal punishment.—E. V. H. The indications of the spectroscope are reliable to a certain point; they do not tell us everything, however. Spectroscopic evidence respecting larger comet presently.—CABINET MAKER. Cannot give addresses of correspondents.—SAMUEL STETTARD. There was a transit of Mercury in Nov. 11, 1861, and one in Nov. 5, 1868; none eighteen years ago. You could not have seen an intra-Mercurial planet in transit, if there were one. Most probably, what you saw was a spot, and though "apparently the size of a five-shilling piece" (how far off?) was, probably, considerably larger.—H. H. HARRIS. There are many cases in which the old poets made their words resemble in sound what they were describing. I know of none much more striking than this, in a description of frying:—

Quis non norit
Stridentesque focis opsonia plebis alausas.

—N. The illusion is practically the same which Mr. Foster has described and illustrated in No. 1. However, it shall appear.—HENRY CARR. It is science which is exactly worded and plainly described, not the magazine. The point you notice was carefully considered in full conclave.—JOHN HAMER. Thanks for kindly letter. As for your explanation, we wait.

Notes on Art and Science.

DOLLOND'S SIDEREAL WATCH.—Almost every amateur astronomer requires to know approximately, if not exactly, the sidereal time. He can calculate it, of course, from ordinary time, for any given instant, but the difference between sidereal and ordinary time is always changing, so that the calculation made at one time will not avail at another. On the other hand, not every astronomer can afford an instrument so costly as a sidereal chronometer. Mr. Dollond, the well-known optician, has devised a neat, simple, and very useful sidereal watch—such an instrument as every amateur astronomer should carry in his pocket.

PROFESSOR PASTEUR'S PREVENTIVE INOCULATIONS OF CHARBON.—The Prussian Minister of Agriculture, the *Deutsche Med. Woch.* (Feb. 11) states, has appointed an influential scientific committee to superintend and report upon a series of inoculations to be performed by one of Pasteur's assistants. This gentleman then proceeds to Russia for the same purpose, and on his return to Saxon Prussia, where the experiments are to be made, will perform a second series of inoculations. Besides some celebrated veterinary professors, Professor Virchow is expected to take part in the inquiry; but regret has been expressed that Professor Koch, the able critic of Pasteur, has not been nominated.

PLANTS IN BEDROOMS.—Plants are unhealthy in bedrooms for this reason, that during the night they give out carbonic bi-oxide, which, as is well known, is injurious to life. Plants, like animals, are constantly breathing—taking in oxygen, and giving out carbonic dioxide. During the day-time they feed as well as breathe, one of their chief articles of diet being the very same poisonous gas which they are constantly expiring. This carbonic di-oxide, under the influence of sunlight, and by means of the colouring matter (chlorophyl) is separated, the carbon being assimilated, and the oxygen evolved. In the daytime there is more oxygen given off than carbonic di-oxide, so that plants may be said to be healthy in the light, but unhealthy in the dark. I may add that the quantity of either gas given off in a room from a few plants is so small as to be hardly worth noticing.—F. D. H.

Our Mathematical Column.

MOGUL'S PROBLEM.

THE problem being "Given any rectangle, divide it by the fewest possible straight cuts, so that the parts can be put together to form a square," my solution is as follows:—

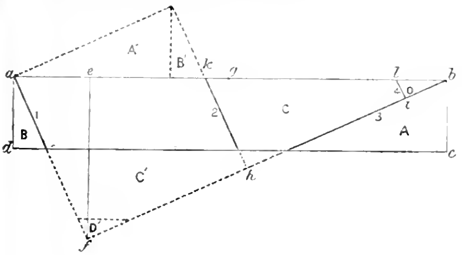
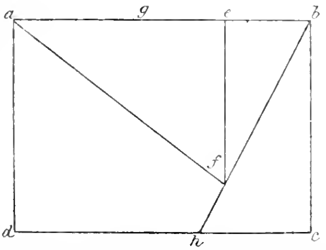
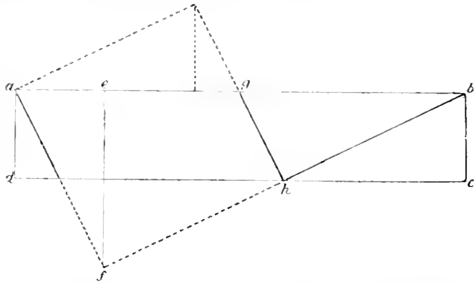


Fig. 1.



On the line $a\,b$ of the rectangle $a\,b\,c\,d$ take $a\,e$ equal to $a\,d$, and make $e\,f$ perpendicular to $a\,b$; bisect $a\,b$ at g, and, with the radius $a\,g$ and centre g, describe a circle cutting $e\,f$ at f, join $a\,f$ and $b\,f$, and make $f\,h$ and $h\,i$ equal to $a\,f$; draw $h\,k$ and $i\,l$ parallel to $a\,f$.

By cutting the rectangle at such parts of the lines $a\,f$, $k\,h$, $l\,i$, and $f\,b$ as pass through it, you will obtain pieces which will form the desired square, *vide* Fig. 1., in which rectangle, $a\,b$ is six times $a\,d$. The principle, however, will be the same, whatever may be the proportion between the sides; but in cases where the proportion does not exceed two to one only two cuts will be necessary; not exceeding five to one, three cuts; not exceeding ten to one, four cuts; not exceeding seventeen to one, five cuts; and so on.



It will be observed that my method of finding the side of a square whose contents are equal to that of a given rectangle, is different to that given by Euclid. II., 14. Calling the sides of the rectangle x and y respectively, Euclid's method is equivalent to the mathematical proposition that—

$$xy = \left[\frac{x+y}{2}\right]^2 - \left[\frac{x-y}{2}\right]$$

whereas my method is equivalent to the proposition—

$$xy = \left[\frac{x}{2}\right]^2 + y^2 - \left[\frac{x}{2} - y\right]^2.$$

It may puzzle some of your readers to discover how my method involves this last equation.—MOGUL.

[Correct solutions by R. Horne (two, both very neat), P. E. M., H. W. Partial solutions by H. J., N., E. Whitby, and others. Solutions by G. H. Bonner, and H. Jones incorrect.—ED.]

PROBABILITIES.

The true method in dealing with problems of the kind considered in our last, is to reduce them to the general law first established by determining—(1) *How many possible events there are*; (2) Whether these are all equally likely; and (3) how many are favourable.

Our question is: What is the chance of throwing one Ace at least in two trials with a single die? Now, when such a die is thrown twice, the following are the possible throws:—

1, 1	2, 1	3, 1	4, 1	5, 1	6, 1
1, 2	2, 2	3, 2	4, 2	5, 2	6, 2
1, 3	2, 3	3, 3	4, 3	5, 3	6, 3
1, 4	2, 4	3, 4	4, 4	5, 4	6, 4
1, 5	2, 5	3, 5	4, 5	5, 5	6, 5
1, 6	2, 6	3, 6	4, 6	5, 6	6, 6

The table being formed by combining first throw 1, with any one of the second throws 1, 2, 3, . . . 6: first throw 2 with any of the same set of 6; and so on. The total number is 36, or 6 times 6. Any pair in *the first column*, or in *the top line*, gives at least one Ace—that is, there are 11 favourable pairs out of 36 possible pairs. Also, it is obvious that any pair of the 36 is as likely to be thrown as any other. Hence, by what was shown in paper I., the chance of throwing Ace at least once in two casts of a single die is $\frac{11}{36}$. The chance of failing is $\frac{25}{36}$. It will be noticed that the number of unfavourable cases is 5 times 5, the total number of cases being 6 times 6. It is clear that a table containing all the unfavourable cases would be formed in precisely the same way as the above table: and that, in fact, such a table is actually included in the above table, omitting the upper line and the left-hand column.

Now the way in which the above result is obtained would be inconvenient in practice. Suppose, for instance, that instead of a die

we had a t[illegible] numbered 1, 2, 3, up to 20. Then th[illegible] we would take alone time [illegible] to notice how the above [illegible] rule for all such cases. W[illegible] the six number 1, 2, [illegible] order that they may appear [illegible] 1, 2, 3, [illegible] that the total num[illegible] and 2 [illegible] only, [illegible] number, as [illegible] rep[illegible] total number of p[illegible] twice, 5 times 5 an[illegible] had been the number of [illegible] we had ($n-1$) times ($n-1$) [illegible] favourable case. Hence the chance [illegible] would happen once at [illegible] $-1)$, or, $\frac{2n-1}{n^2}$.

But [illegible] we can [illegible] applying to the case of [illegible] in order to obtain the total number [illegible] we [illegible] conceive first 1 taken [illegible] 2 [illegible] or 36 times, then 2 taken with all [illegible] 36 [illegible] throws, then 3, then 4; and so on. We [illegible] 36 sets of 3 throws, or 216 such sets. And [illegible] 3 throws, not containing 1, we have [illegible] 3, then 4, 5, and 6, with the 25 possible s[illegible] not appear at all, and this gives [illegible] 25 [illegible] 125.

Hence the [illegible] will not be thrown in 3 trials is $\frac{125}{216}$; and the [illegible] thrown once at least in three trials is $\frac{216-125}{216}$, or $\frac{91}{216}$.

And [illegible] general rule. If there are the same n possible ev[illegible] in [illegible] trials, the chance that some particular [illegible] will not [illegible] in any of the three trials is $\frac{(n-1)^3}{n^3}$, and the chance that it will occur once at least in one of the three trials is $\frac{n^3-(n-1)^3}{n^3}$. [illegible] proceeding in this way we get the yet more general rule [illegible] the same n possible events in each of r trials, the chance that some particular event will not occur in any of the r trials [illegible] $\frac{(n-1)^r}{n^r}$; and the chance that it will occur once at least in the r trials is $\frac{n^r-(n-1)^r}{n^r}$.

This general result is of ext[illegible] importance, as we shall presently see. It is of importance, not only in inferring the antecedent probability, as to the result of successive trials, where the conditions of each trial are known, but also in inferring from the results of successive trials (or observations or experiments, if we please) the conditions (supposed unknown) under which those trials have been made.

I close this paper with two simple examples of the application of this rule.

1. *What is the least number of trials which would give a person at least an equal chance of throwing an ace with a single die?*

We have [illegible] his chance of failing to throw an ace in two trials is $\frac{25}{36}$, and in three trials $\frac{125}{216}$. In four trials the chance of failing will be $\frac{625}{1296}$. Now we note that $\frac{125}{216}$ is greater than a half, and $\frac{625}{1296}$ is less than a half. Hence in three trials he is more likely to fail than to succeed, and in four trials he is more likely to succeed than to fail. Therefore four is the number of trials required. In any continued series of sets of three trials, he would fail somewhat oftener than he would succeed, in throwing one ace at least. But in any continued series of sets of four trials, he would succeed oftener than he would fail.

2. *What is the chance that, in three tossings of a coin, head will appear [illegible]?*

In each trial there are two possible events, *i.e.*, the n of our rule is equal to 2. Thus the chance that head will not be tossed in three trials $\frac{1^3}{2^3}$ or $\frac{1}{8}$. Therefore the chance that head will appear once at least in three tossings is $\frac{7}{8}$. The odds are 7 to 1 that one head at least will be tossed in three trials; and if there were to be repeated sets of three trials, a bettor backing the appearance of one head at least in each set should lay these odds. Further, if a person is to receive £8 in case a head appears in three trials, he ought to pay £7 for his chance.

MATHEMATICAL QUERIES.

39.—CHANCES.—Required:—

1. The chance of dealer (at Whist) holding only one honour in any particular suit.
2. The chance of the dealer holding at least one honour in any particular suit.
3. The chance of the dealer holding only one honour in Trumps.
4. The chance of the dealer holding at least one honour in Trumps. GRADATIM.

[Gradatim sends solutions of these problems.—ED.]

40. What is the general solution of the equation

$$\frac{d^2u}{dx^2}+a\left(\frac{du}{dx}\right)+bu=0$$

and show how it is obtained. WILHELM.

ANSWERS TO QUERIES.

27.—*The value of a diamond varies as the square of its weight. A diamond is broken into three parts; determine the probable value of the parts, compared with that of the unbroken diamond.*

Let x, y, and $a-(x+y)$ be the weights of the parts into which the diamond is broken, a being the weight of the whole diamond, and $\therefore x^2+y^2+(a-x+y)^2$ = sum of value of parts. To find the mean average value, assume that x remains constant, and that y has every value between 0 and $(a-x)$; add up the value of the above expression; and divide by the number of values. That is, integrate the above expression, with reference to y, from 0 to $a-x$, and divide by $a-x$. Hence we get

$$\frac{x^2(a-x)+\frac{(a-x)^3}{3}+\frac{(a-x)^3}{3}}{a-x}=\frac{3x^2+2(a-x)^2}{3}.$$

Give to x every possible value from 0 to a, and divide by the number of values. That is, integrate the above expression from 0 to a, and divide by a. Hence, we get

$$\frac{a^3+\frac{2a^3}{3}}{3a}=\frac{5}{9}a^2$$

Hence, the required proportion is $\frac{5}{9}$. T. R.

HORNER'S METHOD.—In reference to the method of extracting "All Roots," given in "Our Mathematical Column" at p. 440, allow me to observe that it was not discovered by "the late Mr. Horner, M.P.," but by William George Horner, of Bath. I have a special personal interest in mentioning this; for on the publication of my "Practical Arithmetic for Senior Classes," in 1858, the *Athenæum*, while commending the introduction of Horner's method in the "Extraction of Roots," pointed out that I had made the very mistake of referring to Francis Horner, which your correspondent now does. On writing to Bath, I got the full name as above, and I made the necessary corrections in the second edition.—HENRY G. C. SMITH.

Our Whist Column.

THE following game, from the Westminster papers for August, 1874, is selected as illustrating the weakness of lead from short suit; even when the odd trick only has to be made to win, and the idea is not to bring in long suit, but to use long trump suit to ruff. Our correspondent, Mr. Lewis, was Y. His lead at trick 7 is worth noticing.

THE HANDS.

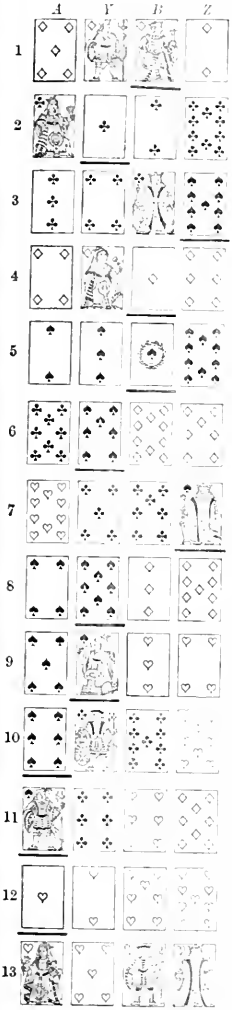
A.		Y.
Spades—Kn, 6, 5, 4, 2.	B	*Spades*—Q, 8, 7, 3.
Diamonds—5, 4.	Y — Z (Dealer)	Diamonds—Q, Kn.
Clubs—Q, 8, 3.	*Trump Card, Spade King.*	Clubs—A, Kn, 6, 5, 4.
Hearts—A, Q, 10.	A	Hearts—5, 2.

B.	Z.
Spades—A.	*Spades*—K, 10, 9.
Diamonds—A, K, 10, 3.	Diamonds—9, 8, 7, 6, 2.
Clubs—K, 9, 7, 2.	Clubs—10.
Hearts—Kn, 7, 6, 3.	Hearts—K, 9, 8, 4.

Score.—*A B*, 4; *Y Z*, 4.

THE PLAY.

NOTE.—The underlined card wins trick, and card below it leads next.



REMARKS AND INFERENCES.

1.—*A*, with five trumps, one honour, leads from his shortest suit, hoping to play a ruffing game. His hopes, it will be seen, are not fulfilled by the event.

2.—*Y* having five Clubs, and seeing lowest Club led, which shows that *B* is not leading from short suit, can pretty safely infer that *Z* has played his only Club. Being strong in trumps himself he returns his opponent's lead (which is from his own long suit), forcing *Z*.

3.—*Z* being short in trumps, would ruff even if the trick were a doubtful one.

4.—*Z* like *Y* returns his opponent's lead. Having five Diamonds, and noting that neither the three nor the four fell to trick one, he can infer, with some degree of probability, that *A* has led from a short suit, in which (from his play) *Y* is also short. Trick 4 shows exactly how the case lies, and *Z* can place every Diamond. *Y* also sees how the Diamonds lie.

5.—*B*, having the winning Diamond, takes out a round of trumps before leading it, knowing his partner's play, and that *Y* lying over him, *A*'s plan is not likely to prove very successful.

6.—*Y* ruffs, of course, though holding four trumps, and

7.—Leads his lowest Club to draw his partner's trump card. He can count the Clubs, knowing that *B* must originally have held four, and he knows, therefore, that if he leads the best, *A* will trump. By playing the lowest, he causes his partner's King to fall separately. The odd trick and the game are won *at this point*.

8.—*Z* leads the best diamond, knowing his partner to lie over *A*. It matters not how *A* plays as the cards lie, but, "for the sake of uniformity," (*Y* having already renounced), *A* should have played his best trump or none.

10.—*Y*, finding all trumps left with *A*, throws the lead into his hand, knowing that he must lead a Heart either after or before last trump, and that the trick wanting to win the game must in that way be secured, unless *A* and *B* between them have entire command of Hearts, in which case the game is gone anyhow.

PROBLEM II.

Trick 1. *A* leads Spade Ace.
2. *A* leads Club Ace, trumped by *B*.
3. *B* leads small Spade, trumped by *A*.
4. *A* leads King of Clubs, trumped by *B*.
5. *B* leads Spade, trumped by *A*.
6. *A* leads Queen of Clubs, trumped by *B*.
7. *B* leads Spade, trumped by *A*.

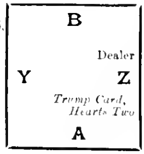
In Problem 3, hands *A* and *B* were inadvertently transposed. [The fault was mine, not "Five of Clubs."—ED.] However, it is so obvious that *A* cannot win every trick, as the hands are set, that we suppose no Double Dummy problem solver has been for a moment deceived. Teddington, J. K. L., R. Morrison, and F. N. Y. have correctly solved the problem, all of them, however, first transposing the hands of *Y* and *Z*, which does not quite make the problem right, though, as it chances, not affecting the solution. The problem is a pretty one, and we now give it correctly, and shall leave it for a fortnight for solution.

PROBLEM III.—Double-Dummy.

THE HANDS.

A.
Hearts—Kn, 6.
Clubs—5, 3, 2.
Diamonds—A, Q, Kn, 6, 5.
Spades—A, Q, Kn.

B.
Hearts—A, Q, 10, 9, 4, 3.
Clubs—10, 6.
Diamonds—3.
Spades—10, 9, 8, 7.



Y.
Hearts—K, 8, 7.
Clubs—9, 8, 7, 4.
Diamonds—2.
Spades—6, 5, 4, 3, 2.

Z.
Hearts—5, 2.
Clubs—A, K, Q, Kn.
Diamonds—K, 10, 9, 8, 7, 4.
Spades—K.

The lead being with *A*, *A* and *B* make every trick.

E. F. B. Harston. Yes: line 30 from top, first column, p. 462, for Ace and Queen read Ace and King. The correction was obvious. Problem 2 is sound. In problem 3, a transposition has to be made. When you say that player at Double Dummy has made every trick, do you necessarily imply that he made them all out of his own hand? In ordinary Whist a player would say, "We have made every trick," when every trick falls between his own hand and partner's, but in Double Dummy he would hardly say that.—FIVE OF CLUBS.

Our Chess Column.

GAMES BY CORRESPONDENCE.—(*Continued from p. 461.*)

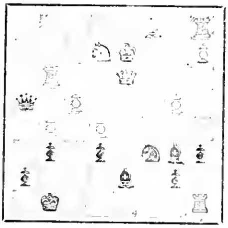
Black's 23rd move in Game 1. was not Q to R4, but Q to R5. With that rectification the following are the positions corrected from last week:—

GAME I.

Position after Black's 23rd move.

23. Q to R5.

CHIEF EDITOR.
WHITE.



BLACK.
CHESS EDITOR.

WHITE.	BLACK.
24. B to R3	R to Qsq
25. R to R3	Q to B7
26. R to B3	Q to Kt7
27. R to Kt3	Q to B7
28. R to B7	Q to Kt7

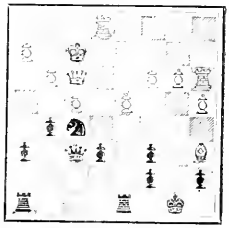
Drawn game (perpetual move).

GAME II.

Position after White's 22nd move.

22. Q to KB3.

CHIEF EDITOR.
WHITE.



BLACK.
CHESS EDITOR.

WHITE.	BLACK.
22.	R to Q3
23. R to K5	K to Rsq
24. R to Kt5	R to Q Ktsq
25. R takes R(ch)	K takes R
26. R to R2	P takes P
27. R to K2	Kt to K6
28. P takes P	Kt to Q4
29. P to KB5	

Had Chess Editor played as indicated by mistake to Chief Editor, viz., 23. Q to R4; 24. B to R3; 24. R to Qsq; 25. Q to KB3, he might have proceeded as follows:—25. Q takes Q; 26. R takes Q; 26. Kt to Q5. With best play White may draw.

For if (*) 27. $\frac{\text{Kt takes Kt}}{\text{B to B3}}$ 28. $\frac{\text{R to Kt3 best}}{\text{B takes Kt}}$ 29. $\frac{\text{R to KBsq}}{\text{B to Q4}}$

30. $\frac{\text{R to Ktsq}}{\text{B takes RP}}$ threatening B to Kt7 disc. ch. and to win the Bishop. The variations arising from this line of play are highly interesting, but we find that the two Bishops aided by the Rook get the best of the struggle in every case. White's best course would

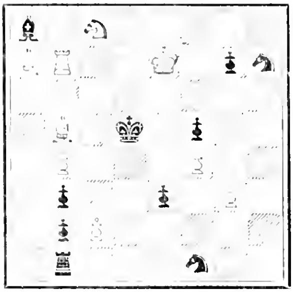
be to give up the exchange voluntarily, and then a draw might result.

(C) If, in reply to 26. Kt to Q5, White does not play Kt takes Kt, but 27. R to R3, then follow:

27. —— / B to B3 28. Kt takes Kt / B takes Kt 29. R to QKtsq / B to KKt7

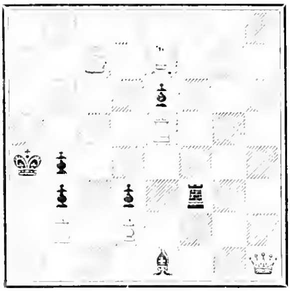
and again, by proper play, the Bishop will force the exchange. In addition, Black might follow another line of play, viz., by discovered check and other judicious play he could capture the Pawns on White King's side, and then Black's Pawns would become formidable.

PROBLEM No. 30.
By J. A. Miles.
BLACK.



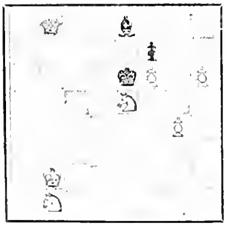
WHITE.
White to play, and mate in three moves.

PROBLEM No. 31.
By Leonard P. Rees.
Black.



WHITE.
White to play, and mate in two moves.

PROBLEM No. 32.
By B. G. Laws.*
Black.



WHITE.
White to play, and mate in three moves.

* Published anonymously in the *Boys' Magazine*.

CORRECTION. Problem No. 25, p. 441.
Remove Black Pawn from B3.

Our numerous correspondents who have kindly drawn our attention to this problem, please take the above correction (which already appeared last week) in consideration. We shall be happy to receive the correct solution of this problem, which embodies a very neat idea.

A great international Chess Tournament will be held at Vienna, beginning on May 10. Prizes to the amount of £400 will be given. The first prize will be £200. Messrs. Steinitz, Blackburne, and Zukertort, the three best players of the whole world, will compete. This Tournament promises to eclipse anything hitherto attempted in Chess Tournaments.

ANSWERS TO CORRESPONDENTS.

*** *Please address Chess-Editor.*

J. A. Miles.—Thanks for Problem, which has an improved appearance.

Leonard P. Rees.—We apologise for mistake in your Problem No. 25. Problem and Game welcome. In the last variation of the "Giuoco Piano," p. 442, if White does not play 10 Kt to B3, threatening to capture the Black Knight, but 10 Castles, then Black need not reply with 10 B to Kt5; a likely line of play for Black in reply to 10 Castles would be Castling, to be followed by P to KB4. White's centre Pawns always remain weak, and liable to attack and isolation by P to QB4. Whichever way you play, White is on his defence, instead of having the attack in hand as first player.

W. Wood.—Received with thanks.

Muzio v. H. Grinold.

A. H. E., W. W. Morgan, 23, Great Queen-street.

Contents of Knowledge *No.* 21.

KNOWLEDGE
AN ILLUSTRATED
MAGAZINE OF SCIENCE
PLAINLY WORDED—EXACTLY DESCRIBED

LONDON: FRIDAY, APRIL 7, 1882.

Contents of No. 23.

DOMESTIC VENTILATION.

A LESSON FROM THE COAL-PITS.

By W. Mattieu Williams.

We require in our houses an artificial temperate climate which shall be uniform throughout, and at the same time we need a gentle movement of air that shall supply the requirements of respiration without any gusts, or draughts, or alternations of temperature. Everybody will admit that these are fundamental *desiderata*, but whoever does so becomes thereby a denouncer of open-grate fire-places, and of every system of heating which is dependent on any kind of stoves with fuel burning in the rooms that are to be inhabited. All such devices concentrate the heat in one part of each room, and demand the admission of cold air from some other part or parts, thereby violating the primary condition of uniform temperature. The usual proceeding effects a specially outrageous violation of this, as I showed in my last paper.

I might have added domestic cleanliness among the *desiderata*; but in the matter of fire places, the true-born Briton, in spite of his fastidiousness in respect to shirt-collars, &c., is a devoted worshipper of dirt. No matter how elegant his drawing-room, he must defile it with a coal-scuttle, with dirty coals, poker, shovel, and tongs, dirty ash-pit, dirty cinders, ashes, and dust, and he must amuse himself by doing the dirty work of a stoker towards his "cheerful, companionable, pokeable" open fire.

It is evident that in order to completely fulfil the above-named requirements, we must, in winter, supply our model residence with fresh artificially-warmed air, and in summer with fresh cool air. How is this to be done? An approach to a practical solution is afforded by examining what is actually done under circumstances where the ventilation problem presents the greatest possible difficulties, and where, nevertheless, these difficulties have been effectually overcome. Such a case is presented by a deep coal mine. Here we have a little working world, inhabited by men and horses, deep in the bowels of the earth, far away from the air that must be supplied in sufficient quantities, not only to overcome the vitiation due to their own breathing, but also to sweep out the deadly gaseous emanations from the coal itself. Imagine your dwelling house buried a quarter of a mile of perpendicular depth below the surface of the earth, and its walls giving off suffocating and explosive gases in such quantities that steady and abundant ventilation shall be a matter of life or death, and that in spite of this it is made so far habitable that men who spend half their days there retain robust health and live to green old age, and that horses after remaining there day and night for many months actually improve in condition. Imagine, further, that the house thus ventilated has some hundreds of small, very low-roofed rooms, and a system of passages or corridors with an united length of many miles, and that its inhabitants count by hundreds.

Such dwellings being thus ventilated and rendered habitable for man and beast, it is idle to dispute the practical possibility of supplying fresh air of any given temperature to a mere box of brick or stone, standing in the midst of the atmosphere, and containing but a few passages and apartments.

The problem is solved in the coal-pit by simply and skilfully controlling and directing the natural movements of unequally-heated volumes of air. Complex mechanical devices for forcing the ventilation by means of gigantic fan-wheels, &c., or by steam jets, have been tried, and are now generally abandoned. An inlet and an outlet are provided, *and no air is allowed to pass inwards or outwards by any other course than that which has been pre-arranged for the purposes of efficient ventilation.* I place especial emphasis on this condition, believing that its systematic violation is the primary cause of the bungling muddle of our domestic ventilation.

Let us suppose that we are going to open a coal-pit to win the coal on a certain estate. We first ascertain the "dip" of the seam, or its deviation from horizontality, and then start at the *lowest* part, not, as some suppose, at that part nearest to the surface. The reason for this is obvious on a little reflection, for if we began at the shallowest part of an ordinary water-bearing stratum we should have to drive down under water, but, by beginning at the lowest part and driving upwards, we can at once form a "sumpf," or bottom receptacle, to receive the drainage, and from which the accumulated water may be pumped. This, however, is only by the way, and not directly connected with our main subject, the ventilation.

In order to secure this, the modern practice is to sink two pits, "a pair" as they are called, side by side, at any convenient distance from each other. If they are deep, it becomes necessary to commence ventilation of the mere shafts themselves in the course of sinking. This is done by driving an air-way—a horizontal tunnel from one to the other, and then establishing an "upcast" in one of them by simply lighting a fire there. This destroys the balance between the two communicating columns of air: the cooler column in the shaft without a fire, being heavier, falls against the lighter column, and pushes it up just as the air is pushed up one leg of an U tube when we pour water down the other. Even in this preliminary work, if the pits are so deep that more than one air-way is driven, it is necessary to stop the upper ways and leave only the lowest open, in order that the ventilation shall not take a short and useless cut, as it does up our fire-place openings.

Let us now suppose that the pair of pits are sunk down to the seam, with a further extension below to form the water sumpf. There are two chief modes of working a coal-seam, the "pillar and stall" and the "long wall," or

more modern system. For present illustration, I select the latter as the simplest in respect to ventilation. This method, as ordinarily worked, consists essentially in first driving roads through the coal from the pits to the outer boundary of the area to be worked, then cutting a cross road that shall connect these, thereby exposing a "long wall" of coal, which is cut away towards the pits, the roof remaining behind being allowed to fall in.

Let us begin to do this by driving, first of all, two main roads, one from each pit. It is evident that as we proceed in such burrowing, we shall presently find ourselves in a *cul-de-sac* so far away from the outer air that suffocation is threatened. This will be equally the case with both roads. Let us now drive a cross cut from the end of each main road, and thus establish a communication from the downcast shaft through its road, then through the drift to the upcast road and pit. But in order that the air shall take this roundabout course, we must close the direct drift that we previously made between the two shafts, or it will proceed by that shorter and easier course. Now, we shall have air throughout both our main roads, and we may drive on further until we are again stopped by approximate suffocation. When this occurs, we make another cross cut, but in order that it may act, we must stop the first one. So we go on until we reach the working, and then the long wall itself becomes the cross communication, and through this working gallery the air sweeps freely and effectually.

In the above I have only considered the simplest possible elements of the problem. The practical coal pit in full working has a multitude of intervening passages and "splits," where the main current from the downcast is divided, in order to proceed through the various streets and lanes of the subterranean town as may be required, and these divided currents are finally reunited ere they reach the upcast shaft which casts them all out into the upper air. In a colliery worked on the pillar and stall system (*i.e.*, by taking out the coal so as to leave a series of square chambers with pillars of coal in the middle to support the roof) the windings of the air between the multitude of passages is curiously complex, and its absolute obedience to the commands of the mining engineer proves how completely the most difficult problems of ventilation may be solved when ignorance and prejudice are not permitted to bar the progress of the practical applications of simple scientific principles.

Here the necessity of closing all false outlets is strikingly demonstrated by the mechanism and working of the "stoppings" or partitions that close all unrequired openings. The air in many pits has to travel several miles in order to get from the downcast to the upcast shaft, though they may be but a dozen yards apart. (Formerly the same shaft served both for up and down cast, by making a wooden division (a *brattice*) down the middle. This is now prohibited, on account of serious accidents that have been caused by the fracture of the *brattice*.) But it would not do to carry the coal from the workings to the pit by such sinuous courses. What, then, is done? If any direct road were left open, the air would choose it, but this is prevented by an arrangement similar to that of canal locks and gates. Valve doors or stoppings are arranged in pairs, and when the hurrier arrives with his *corve*, or pit carriage, one door is opened, the other remaining shut; then the corve is hurried into the space between the doors, and the entry door is closed; now the exit-door is opened, and thus no continuous opening is ever permitted. Only one such opening would derange the ventilation of the whole pit, or of that portion fed by the split thus allowed to escape. It would, in fact, correspond to the action of our open fire-places in rendering effective ventilation impossible.

The following, from the report of the Lords' Committee on Accidents in Coal Mines, 1849, illustrates the magnitude of the ventilation arrangements then at work. In the Hetton Colliery there were two downcast shafts and one upcast, the former about 12 ft. and the latter 14 ft. diameter. There were three furnaces at the bottom of the upcast, each about 9 ft. wide and about 4 ft. length of grate-bars. The depth of the upcast and one downcast 900 ft., and of the other downcast 1,056 ft. The quantity of air introduced by the action of these furnaces was 168,560 cubic feet per minute, at a cost of about eight tons of coal per day. The rate of motion of the air was 1,097 ft. per minute (above 12 miles per hour). This whole current was divided by splitting into 16 currents of about 11,000 cubic feet each per minute, having, on an average, a course of $4\frac{1}{4}$ miles each. This distance was however, very irregular; the greatest length of a course being $9\frac{1}{10}$ miles; total length 70 miles.

All these magnitudes are greatly increased in coal mines of the present time. As much as 250,000 cubic feet of air per minute are now passed through the shafts of one mine.

The ventilation of our houses may be conducted on the same principles, and with corresponding efficiency, as I will endeavour to show in my next.

FOUND LINKS.

By Dr. Andrew Wilson, F.R.S.E., F.L.S.

PART V.

BACKWARDS in time, and in the course of the geological aeons, we find the Cretaceous or Chalk rocks. To the naturalist these deposits have yielded a rich and suggestive harvest of bird-fossils, which, in their approximation to the reptiles, certainly serve as "found links" in more ways than one. In the Chalk rocks of North America we discover the remains of "toothed birds," whose teeth in every respect agree with the structures of that name, and are not mere bony projections, as in the old swimmer of the London clay. The curious *Hesperornis* (Fig. I) and its neighbours the *Ichthyornis* and *Apatornis*, thanks to the exertions of Professor Marsh, appear before us as veritable links, connecting the birds and reptiles in respect of their teeth, as well as in other features of their economy. Hesperornis stood at least five feet high, and in respect of its bony framework exhibits a close alliance with the grebes of our own day. But strange to say, Hesperornis (Fig. I) wants one marked peculiarity of other birds (save the ostrich-group), namely, the prominent "keel" or bony ridge on the breast-bone, to which the wing-muscles of birds are attached. The wings were certainly of rudimentary character, but this is a feature we see exemplified in the auks and penguins of our own day; and it is probable that the tail of this great diver of the chalk seas was unusually mobile, and adapted possibly to serve as a rudder. The reptile characters crop out, however, most clearly in the teeth of this bird. There were no teeth in the front of the upper jaw, and presumably this region was covered with a horny beak. The teeth themselves are curved structures; but they are set in a common groove, and not lodged each in a socket, as is commonly the case in higher animals. In living reptiles themselves, it may be added, the teeth, save in crocodiles, are not implanted in sockets. Thus, in serpents and lizards the teeth are simply united by bony union to the bones which bear them; but certain extinct lizards had socket-fastened teeth, and the giant fossil reptiles (*Ichthyosaurus*, &c.) of the Lias, Oolite, and Chalk, possessed teeth

Fig. 1.—Hesperornis regalis.

Fig. 2. { A. Skeleton of Pterodactyl.
{ B. Restoration of Pterodactyl.

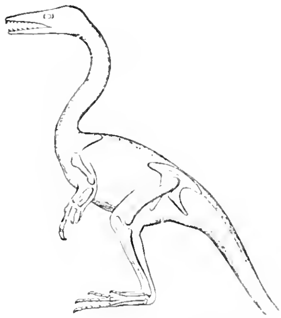


Fig. 3.—Compsognathus (restored in outline).

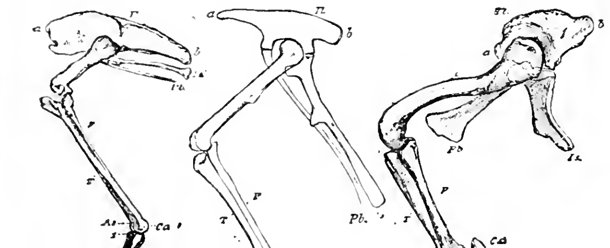


Fig. 4.

Fig. 5. { A. Hind leg of bird.
{ B. Tail bones of bird.

FOUND LINKS.

which also were [illegible] from sockets in the jaws. In so far as Hesperornis is concerned, it removes the bird class, on the tree of things, a step nearer to the reptile hosts. Formerly, part of the naturalist's definition of a bird was included in the declaration that teeth were wanting. Now, the definition requires stretching, to include a character which is shared in by certain reptiles, just as others, represented by the tortoises and turtles, imitate the toothless condition of existing birds.

But the Ichthyornis of the chalk is even a more remarkable bird fossil than Hesperornis. For the teeth of the former are implanted in distinct sockets, whilst its breastbone had a keel, and its wings are of large size, and indicate the possession of bird habits, united to structures of reptilian kind. But more peculiar still, as a departure from bird characters was the nature of its vertebræ or the joints of the spine; for Ichthyornis possessed vertebræ, which, like those of the fishes and of extinct reptiles, were hollow at either end. Such a feature must naturally be made much of in estimating the relationship of this old bird to the reptilian hosts. The size of Ichthyornis was that of a pigeon.

Preceding these birds in time comes the *Archæopteryx* of the Upper Oolite deposits of Solenhofen, in Bavaria. Here the reptile characters increase in number as becomes the older nature of their possessor. A recently-procured specimen of this bird enabled a zoological authority to declare that it was certainly not wholly a bird, and as certainly not wholly reptile in its nature, but a strict link between these classes. For, firstly, it has the tail of a lizard, that is, the tail is long and jointed, and has no ploughshare bone, as in other birds (Fig. 5, B, *b*). Secondly, the bones of its palm were not joined together as in all other birds, whilst at least two of its fingers appear to have been provided with claws, a feature of exact reptilian nature. Then it likewise has been ascertained, by the discovery of the recent specimen already referred to, that this old bird of the Oolite possessed teeth. Judged fairly, then, Archaeopteryx is, at the very least, as much a reptile as a bird. Its shoulder and fore-limb (or wing) are decidedly those of a reptile, whilst its hind limbs are bird-like in nature. The facts that such a race of animals once existed, and that they lived at a period when, presumably, the bird-race was undergoing its evolution from the reptilian confines, may, in the eyes of any unprejudiced thinker, serve as clear evidences that the common origin of birds and reptiles is matter, not of speculation, but of scientific demonstration.

I have shown, thus, cursorily, the evidences supporting the contention that if, standing within the bird-class, we look for reptilian features within its limits, we are not disappointed in our search. But on the reptilian side of things, there are also evidences to be found of the community of type from which the birds and reptiles of to-day have sprung. It takes but a slight acquaintance with zoology to discover that the curious lizard, *Hatteria* (or *Sphenodon*), of New Zealand, as befits the curious history of its native country, brings us face to face with characters of abnormal kind, from the reptilian view of matters at least. For this lizard has ribs which are decidedly those of bird type, and, moreover, it has the same hollow-ended vertebræ seen in the fossil bird Ichthyornis. In other points of its structure as well, Hatteria seems to represent a primitive type of reptile, presumably indicating that stage in the evolution of the two classes wherein certain characters of the bird had already begun to be developed in the common ancestors of these groups.

The flying reptiles (*Pterodactyls*) (Fig. 2) of the Lias, Oolite, and Chalk, teach us that as the pure reptile thus acquired powers of flight, the development of flight in the bird-stock, which was evolved from the reptile, or conjointly with it, need cause us no surprise.

The Pterodactyls possessed the outermost finger (seen in the illustration) enormously elongated, and adapted to form the chief support of a wing-membrane which extended along the sides of the body and between the hind limbs and tail also, as shown in B, Fig. 2. It may be added that these reptiles had a keel on the breastbone like most living and extinct birds, and whilst in some species, the teeth were developed, in others the jaws appear to have been toothless, and to have been sheathed in horn like those of birds. But these reptiles are not "links." They stand, not between birds and reptiles, but at the end of their own side-branch of the great tree of animal life. Still, from the reptile-side, it may lastly be shown that the "found links" connecting them with birds—it may be, of course, in different lines from those indicated by Archaeopteryx and its neighbours—already find a place in the geological museum. In those curious reptiles, of which *Compsognathus* (Fig. 3) is the best known example, the characters of birds and reptiles were united in a literally surprising degree. Imagine a reptile possessing a swan-like neck, with toothed and bird-like jaws: suppose, further, that this animal had very rudimentary front limbs, and that it walked on its two hind limbs like a bird, and we may conceive that this Compsognathus, had it been feathered, would have at least appeared to resemble a bird much more nearly than a reptile. But a still stronger piece of evidence in favour of its bird-relationship awaits the naturalist when he discovers that the hind limbs of these curious reptiles are, in respect of structure, midway and between those of birds and reptiles. If we examine the hind limb of a bird (Fig. 4, A), we notice that the upper half of the ankle (*As. Ca.*) unites with the shin-bone, or leg (T); and as the lower half of the ankle joins the instep (I), the ankle joint thus exists in the middle of the ankle-bones, and the lower ankle and instep-bones form a single bone (*m*) by their union. In Fig. 5 the hind limb of the bird is also seen, *e* being the single bone formed by the union of ankle and instep-bones. In the reptile's limb (C), the ankle-joint, as a rule, opens between the divided ankle-bones; but the instep-bones (C: i., ii., iii., iv.) are not united either to one another, or to the neighbouring ankle-bones. Now it is extremely interesting to discover that the hind-limb of Compsognathus and its allies (B) is exactly intermediate between birds and reptiles. Here, the leg bones resemble those of birds in shape. The chief ankle bone (*As.*) exactly corresponds with that of a bird; and, as in birds, this bone becomes united to the lower end of the leg. But, lastly, as if to show the intermediate nature of the limb, the instep-bone (I—iv) remain free, and the leg of Compsognathus is thus practically half-way between that of the bird and reptile. Thus, as in birds, the upper part of the ankle is united to the leg; but, unlike birds and like reptiles, the Compsognathus had the lower part of the ankle free, and not united with the instep. In a word, the hind limb of this old reptile resembles the condition of the limb in the chick before hatching, and it may thus represent that stage in the evolution of the bird-type wherein the type of limb common to the primitive stock was being gradually modified into the more consolidated limb of the bird.

Thus to-day, there exists a series of forms, detached and isolated, perhaps, but still eloquent enough in their declaration of the existence in past epochs of animals which belong to no one class as defined by us to-day, and which—

further stand intermediate between existent classes of living beings. The existence of these "links," to argue backwards, is inexplicable, save on the theory of evolution, or on that of the production of "freaks" by nature: and this last idea, I apprehend, is put out of court, by every consideration worthy the name of a scientific thought.

NOTES ON ROWING.

By an Old Club Captain.

THE race is over, and the despised crews of "inefficient mediocrities" have somehow managed to take their boats over the course from Putney to Mortlake (on a good, but not wonderfully good, tide), in the very good times of 20 min. 12 sec. and 20 min. 32 sec. for winners and losers respectively. I was able to watch both crews under singularly favourable conditions as they rowed past the White Hart, at Mortlake. With an excellent field-glass by Browning, I had each crew in succession about half a minute in view, as distinctly as if they were at an oar's length from me. For a minute or two before and after this, I was able to see both crews together, foreshortened, so that the nature of the swing, and the time of stroke and recovery could not only be well seen, but readily compared. [I had before only seen the crews in practice, and always at the beginning of their spins. Oxford, in particular, I had seen under very favourable conditions.] At Mortlake on Saturday, there was a difference naturally arising from the circumstance that Oxford were rowing out to the finish a race already won, while Cambridge—though they could not be so sure of the hopelessness of their position as the Oxford men were sure of victory—were nevertheless very obviously rowing a losing race. There was not seen that liveliness on the feather, after rather sluggish disengagement of the oar, which had been characteristic of the Cambridge style. On the other hand, the Oxford men showed the good features of their style very markedly. The swoop down of the oars upon the water was splendid at this stage of the race (I am told that earlier there was occasionally some little trace of flurry). As for the way in which the work was done—a question which I left not quite decided in my last—I am as certain, after my observations last Saturday, as I am of my own existence, that nearly all the arm work was done in conjunction with body and leg work, and not, as some persistently assert, afterwards. I am equally certain that in the Cambridge boat the arm work followed the sway back of the body. The sluggish look of the Cambridge style was in singular contrast to the sharp slash of the Oxford oars through the water; and this sluggish look was entirely due to the inert condition of the arms during the first part of the stroke. The slow disengagement of the oar by the Cambridge, again, was in decided contrast with the clean, quick disengagement by Oxford. On the other hand, I cannot say that the swing forward by Cambridge was anything like so sharp as I had expected to find it, or as it appeared in practice. But this was only natural in a crew which had been rowing so hard a stern race. Moreover, the Cambridge style is, as every one knows who has ever tried it, exceedingly wearing in a hard race; and the lightning feather, which is its one theoretical advantage (more than counterbalanced by disadvantages), is very apt to be exchanged towards the end of a long race for a much less lively movement. This circumstance, which I have noticed myself in rowing races (I suppose every reader of these lines has already come to the conclusion that the writer is a Cantab) is well described by Mat Bradwood, in a passage which deserves to be quoted at full length, so apt and instructive is it. "Every day of practice on the Cam," he says, "you hear the coaches of the different racing-boats giving their crews certain directions, some absurd, and nearly all from some accidental reason useless. The chief of these is to 'keep it long,' and if you object to the results of this teaching, you are told that 'length' is the great requisite of good rowing, and that 'Oxford, sir, always beat us, because they are longer than we are.' Now this is true and yet untrue. At Cambridge, 'length' is acquired by making the men 'finish the stroke,' that is by making them swing well back beyond the perpendicular. Of course the oar remains longer in the water, but we maintain that the extra time it is kept there by the backward motion of the body is time lost. The 'swinging back' throws a tremendous strain on the abdominal muscles, the weakest rowing muscles in the body; very soon the men feel this strain, become exhausted and unable to 'get forward,' and, finally, lose time and swing and 'go all to pieces.' Length obtained by going backwards is of no possible use. A crew ought to be coached to go as far *forward* as they can, to finish the stroke by bringing their elbows past their sides, and their hands well in to their bodies, and then complaints about 'wind' and 'last' will be fewer."

It may be asked why, if the repeated victories and the nature of the victories of Oxford result from an inherent superiority of the Oxford over the Cambridge style, Cambridge does not adopt the Oxford style? It is well known that Cambridge club captains try to introduce what they believe to be the style of Oxford. The passage above quoted indicates very accurately the feeling of Cambridge men on this point. But a radically wrong idea is entertained at Cambridge as to what the Oxford style really is. Oxford men are apt, as I have already mentioned, to assert (and doubtless they believe, though erroneously, as close observation and theory alike show) that they do not use the arms till the body is nearly upright. Thus, Mr. Skey, F.R.C.S., in writing on the movement of the muscles and the body in rowing, gives as the result of inquiry which he had made, the following entirely incorrect account of Oxford rowing:—"The prominent and distinctive feature of the Oxford system consists, I believe, in this that the action of the glutei (the great muscles of the buttock), in drawing the trunk backwards to something beyond the vertical, is nearly exhausted before the agents of flexion of the forearm commence their work. The Oxford authorities consider that they row with their trunk, while others more prominently row with their arms. In truth" (here his anatomical knowledge sets Mr. Skey right) "the muscular system of both trunk and arms is indispensable in all cases, the only distinction being that in the case of Oxford oarsmen the greater part of the retraction of the trunk, by the action of the glutei is accomplished with rigid unbent arms, while in other cases the retractors of the shoulders, and the flexors of the forearm are somewhat more in unison, or rather, they share the time occupied by the former action." Mr. Skey is "unwilling to express a positive opinion as to the relative excellence of the two styles in rowing," but is inclined to think that some advantage is obtainable from the two actions being rendered consecutive, inasmuch as the superior power of the retractors of the trunk, on which the great effort in rowing depends, should be exerted singly, without the physical action of the system being hampered by two actions at the same moment of time." As this was written in October, 1869, when Oxford had been for nine successive years successful against Cam-

[illegible] had beaten in hollow fashion the [illegible] Harvard, it was only natural that Mr. [illegible] inclined to think the Oxford style [illegible] better, and [illegible] it unquestionably [illegible] [illegible] desirable is that which was, and [illegible] [illegible] Cambridge captains, and observation [illegible] Oxford does not follow that style, but [illegible] the arms to work from the beginning. How any [illegible] could ever be entertained on this point by those who [illegible] how much farther back than Oxford the Cambridge [illegible] have always swung, is difficult to understand. For, if the arms are straight when the body reaches the vertical position, the body must, of course, swing further back while the arms do their work, as yet scarce begun; but, if the arms have been at work while the body is swinging to uprightness, the arms have little left to do, and therefore the body need swing back but a very little. As Oxford men unquestionably do *not* swing far back, while Cambridge men do,—even men who know nothing practically of rowing can see *that*,—it should need no demonstration that Oxford men do more of their arm work than Cambridge men during the beginning of their stroke. "And I myself, with my very own eyes have seen," as the old chronicler wrote, that Oxford men, and London men too, and watermen always, row with body and arms, and legs too, from the very beginning of the stroke.

Meanwhile, at Cambridge they quietly accept the faith that Oxford men swing back with rigid, unbent arms (and they see this in tub practice and paddling, where it is the correct thing to do); so they keep to the rule, unconscious or forgetful that it is the old rule, good only for the old style of boats; and so they get beaten, unless they have overwhelming superiority of strength, when they win by half a length or a length, where Oxford, with the same superiority of strength, would have won by half a dozen or a dozen lengths.

I write confidently, but I write when there is good prospect that the matter will be tested in a way there can be no mistaking (I would stake a good deal on the result if I were a betting man). Mr. Muybridge's method of instantaneous photography, which has shown how a horse gallops and how an athlete turns a somersault, will very readily show how an Oxford or a Cambridge oarsman rows, and is very likely soon to be applied to the work. But there is a practical test which Cambridge captains might very easily try, and perhaps apply the result in a way very pleasing to themselves and their crews. Let a crew, each member of which rows well the Cambridge dragging swing-back, go over a measured distance, say half a mile, at their best speed in that style, and then let them—though, perhaps, a little tired—go back to the starting-place and go over the course again with a changed style. Let stroke and each man of the crew agree for awhile to give up their cherished drag and lightning feather; let the arms be sturdily called into action, in due subordination, of course, to body and in due alliance with the legs, from the very beginning of the stroke, so that when the body comes upright, the arms have nearly done their work. Let not the stroke be hurried, but a steady (not sluggish) recovery, precede the simultaneous grip at the beginning. I will undertake to say that, even at the first trial (absolutely important though practice is in this style, where everything depends on the work being done at the same time), even at the first trial, I say, the result will be such as to show unmistakably how much more effective, and also how much easier, this style is than the style inculcated forty years ago for boats as different from those of our time as a barge from a pleasure boat, or a pleasure boat (with a party of ten under her awning) from the old clinker-built racing craft.

THE ELECTRICAL EXHIBITION AT THE CRYSTAL PALACE.

EIGHTH NOTICE.

It is not difficult to understand that if a certain amount of electric energy is required to render a carbon filament 1 in. long incandescent, ten times that energy will be required (presuming there is no other resistance in the circuit) to similarly heat a filament 10 in. long. If, however, we have sufficient energy to heat 10 in. of carbon, it matters little whether that carbon is in one piece or in ten, or in any number of pieces making up the same total length. Here, then, is the first principle in the so-called division of the electric light. The great object to be achieved is really a *distribution* of the light, necessitated by the great diminution of luminosity as we recede from the source of light. The exact falling-off is "inversely as the square of the distance," that is to say, if we have a luminosity equal to twenty candles at a distance of 1 ft., the intensity at 2 ft. will be—

$$2^2 : 1^2 :: 20 : x$$
$$\text{or } 4 : 1 :: 20 : 5$$

5-candle power will, therefore, be the luminosity at 2 ft. distance. This explains how it is that a large light is so expensive, or lacks economy so much, when compared with a number of small lights placed at the proper distance from each other to illuminate the same area.

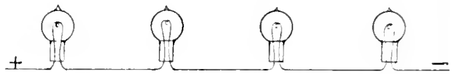


Fig. 1.

Fig. 1 illustrates the method of connecting the lamps, known as "joining in series"—that is to say, the whole of the current passes through each lamp in succession.

Fig. 2 illustrates another method of joining up, and is known as the "multiple arc," or "quantity" arrangement. The current comes in from the machine at +, and returns by the other wire marked –. In this case the current, instead of passing through each lamp in succession and having to overcome the added resistance of, say four lamps, is assisted by their conductivity, and divides itself between them, the resistance being thereby considerably reduced, just as a flow of water which requires a certain pressure to push it through, say, four yards of pipe, requires one-fourth of that force to drive it through one yard. If we were to place four pipes, one yard long, side by side, the required pressure would be again quartered. Actually, therefore (assuming each lamp to offer 50 ohms' resistance), joining in series would give 200 ohms for four lamps, while in multiple arc the resistance would be reduced to 12·5 ohms.

But suppose again that in the single pipe the quantity of water was only sufficient to fill one pipe, we should have to quadruple the supply of water in order to fill four pipes placed side by side. Accordingly, the current required for the arrangement described is of less intensity, but of greater volume, to produce equal degrees of luminosity.

Fig. 3 is another arrangement known as "mixed," that is, a combination of series and quantity. The diagram may be left to explain itself.

We should have liked this week to give, in continuation of our remarks on incandescent lighting, a few mathematical details concerning the arrangement of the lamps and generators. These details, however, will be best comprehended after we have finished our description, in

another series of articles, of the fundamental principles of electric generators.

The incandescent light is, beyond doubt, the light of the future, and we have no hesitation in saying that before long we believe it will be proved as much cheaper as it is brighter, purer, and healthier than gas. At the same time we must not lose sight of the fact that it ensures perfect immunity from all those possible catastrophes inherent to gas. An incandescent lamp, even when broken, is perfectly harmless, and it has been declared that it could be broken in the centre of a barrel of gunpowder without the slightest danger. The reason of this is that the moment the glass is broken, or even cracked, air rushes in to occupy the previously vacuous globe, and coming in contact with the carbon filament, oxidation instantaneously takes place, the filament being thus a continuous conductor no longer. The only danger is when the covering of the wires gets

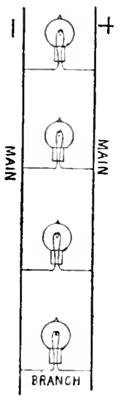


Fig. 2.

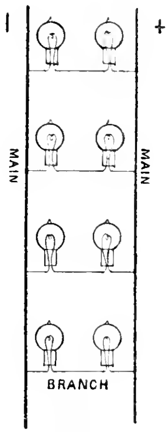


Fig. 3.

damaged, or in any other way allows the current to pass from one wire to the other without going through the lamps; then great heat would be developed, sufficient, perhaps, to set fire to the building. This danger, however, is easily guarded against by inserting a piece of easily fusible metal, such as lead, in the circuit, which, on getting hot, melts and breaks the circuit, the only inconvenience being the extinction of all the lights in that section, until the piece of lead is replaced. And what is this inconvenience compared to what would result from a gas explosion?

Anent the various systems now before the public, there is not much to choose between them, but for brilliancy and durability the Swan certainly leads the way. The Electrolier in the Furniture Court is simply beautiful, and contrasts in the delicate outline of its design with the gorgeous display of Mr. Edison. Nor must we forget Messrs. Elkingtons' show-room, which is very tastily adorned by the Swan light. The Maxim and Lane-Fox systems are both very good, and try hard to outdo each other in their application to delicate glass-work.

Explosive Paint.—It is stated that the Admiralty have directed that the whole of the xerotine siccative in store at the various dockyards should be immediately destroyed.

THE GREAT PYRAMID.

By the Editor.

This week my remarks upon the Pyramid must be brief, for the present number (on account of the approach of Easter week) appears under certain disadvantages. I take the opportunity of noting sundry objections to my views, which have been suggested by certain readers.

In the first place, many seem quite unaware of the difficulty of orienting a building like the Great Pyramid with the degree of accuracy with which that building actually has been oriented. One gravely asks whether (as Narrien long since suggested) a plumb-line, so hung as to be brought into line with the Pole Star, would not have served as well as the great descending passage. Observe how all the real difficulties of the problem are overlooked in this ingenious solution. We want to get a long line—a line at least 200 yards long—in a north and south position. We must fix its two ends, and as the pole-star is not available as a point along the line, we set our plumb-line at the northern end of the line, and our observing tube or hole, or whatever it may be (only it is not a telescope, for we are Egyptians of the time of Cheops, and have none), at the other. The pole-star being at an altitude of 26½ degrees, the plumb-line should be nearly 100 yards long, to be seen (near the top), coincident with the pole-star, from a station 200 yards away. That is a tolerably long plumb-line. Then its upper part (thus to be seen *without telescopic aid at night*) would be about 260 yards away. The observer's eye-sight would have to be tolerably keen.

I am also asked whether a dishful of water would not serve quite as well as a great mass of water, at the corner where the descending and ascending passages meet, to give the reflected rays from a star. It would, and so would a thimbleful—just as a thread of cotton would serve as well as a half-inch rope for the plumb-line just considered. But just in proportion as the water surface was diminished would the difficulty of seeing a star by reflected rays be increased. The builders had, doubtless, good reason for making the descending passage about 4 ft. wide and as many high. It at any rate enabled them to see the pole-star readily, just as the wide field of view of a comet-finder enables the astronomer to bring a celestial object very easily into view. Whatever reason they had for thus securing a tolerably large field of view, they would have precisely the same reason for retaining it undiminished when they used the reflected instead of the direct rays, in observing a star. Now for this purpose nothing short of the whole breadth of the descending and ascending passages, would suffice—in other words, no dishful or thimbleful of water would have served their purpose.

Then the *Saturday Review* asks why the descending passage should be repeated in the other pyramids when the orientation had already been secured in the Great Pyramid—manifestly quite ignorant of the fact that it would be far more difficult to take the orientation for one pyramid from another, than to do it independently. It also asks whether the slant descending passages were not obviously meant for the sliding-down of the King's sarcophagus. Sliding the sarcophagus down that it might afterwards be hauled up the ascending passage! or if not, what was the ascending passage for? and why was it of the same cross section as the descending passage? If the sarcophagus alone had been in question, we may be certain that the pyramid engineers would never have arranged for sliding it down from the level of the entrance to the descending passage, to the place where the ascending passage begins, in order afterwards to raise it by the ascending

passage. If they meant to go down to the underground chamber they would not have raised it at all, but let it down from the level of the pyramid's base. But, to say truth, moving the sarcophagus was a mere nothing compared with the lifting of the great solid blocks which formed the pyramid's mass. The engineers who moved these great blocks to their places would not have wanted slant passages at the right friction slope, and all the rest of it, by which to take the sarcophagus to its place; nor would they have provided for unnecessary descents or ascents either, but have taken the sarcophagus from the outside to its proper level, and sent it along a long level passage.

The *Saturday Review* says further; but, what *can* it matter what the *Saturday Review* says on subjects such as these?

A correspondent, Mr. J. F. Hodgetts, touches on the association which I mentioned as existing between the Jewish Sabbath (our Saturday) and Saturn; labouring, manifestly, under the impression that the point at issue was the identity of the Roman god Saturn with the Scandinavian deity assigned to Saturday. But of course he has entirely misapprehended me. It is not the god Saturn, but the planet Saturn, which is associated with Saturday. How Mr. Hodgetts can reconcile the clear statement of Dion Cassius with his belief that the days of the week were not associated with the planets until the twelfth century, passes my comprehension. Dion Cassius distinctly attributes the invention of the week to the Egyptians, and as he wrote a thousand years before the time named, there must be something wrong in Mr. Hodgett's dates. In the ancient Brahminical astronomy the days are associated with the same planets as among the Egyptians. See Mr. Colebrooke's papers in the "Asiatic Researches." Among more familiar discussions of this matter may be cited Bailly's "Astronomie Indienne et Orientale," and Bohlen's "Das Alte Indien."

Dion Cassius refers to the connection between musical intervals and the planets, showing that probably the old Egyptian lore which Pythagoras of Samos brought to Greece, included the association between the planets and the days of the week: that, in fact, all three subjects were connected—planets, musical intervals, and the days of the week. Longfellow thus poetically renders the views of Egyptian astrologers on these, with them, mystical matters:—

> Like the astrologers of eld,
> In that great vision I beheld
> Greater and deeper mysteries.
> I saw, with its celestial keys,
> Its chords of air, its frets of fire,
> The Samian's great Æolian lyre,
> Rising through all its sevenfold bars,
> From earth unto the fixed stars.
> And through the dewy atmosphere,
> Not only could I see but hear
> Its wondrous and harmonious strings
> In sweet vibration, sphere by sphere;
> From Dian's circle light and near,
> Onward to vaster and wider rings,
> Where, chanting through his beard of snows
> Majestic, mournful Saturn goes,
> And down the sunless realms of space
> Reverberates the thunder of his bass.

ERRATA.—In the letter-press accompanying chart of the path of Mars, second column, line 10, for "south to north" read "north to south," and line 12, for "north to south," read "south to north." Line 15 for "plane of its orbit," read "plane of the earth's orbit." The mistakes could scarcely deceive the careful reader, but it is as well that they should be corrected.

PHOTOGRAPHY FOR AMATEURS.

By A. Brothers, F.R.A.S.

PART III.

Before leaving the historical part of the subject, it may be interesting to refer to the partnership which was entered into between Niepce and Daguerre. The one appears to have rendered no assistance whatever to the other; and to Daguerre alone is due the credit of working out the process which was considered by the French Government of so much importance, that Daguerre was awarded a pension for life of £240 a-year, and to Isidore Niepce* £160 a year; the half to revert to their widows. Considering the great value of the discovery, the sum awarded was ridiculously small; but, although the French Government appear to have presented the discovery to the world, Daguerre patented the process in England, and must have realised a large sum by the sale of licenses.

For many years the two processes of Talbot and Daguerre were extensively used. The Talbotype, or Calotype, as it was indifferently called, was more suitable for landscapes and architectural subjects than for portraits, and the Daguerreotype was used almost universally for portraits, and may be said to have been without a rival until 1851, when Mr. F. Scott Archer presented his process to the public. Sir John Herschel, Mr. Bingham, M. Legray, and others, had tried various means for utilising glass on which to produce the negative image; but to Mr. Archer, assisted by Dr. Diamond, is due the honour of inventing a process which produced the most perfect results. Mr. Archer was a sculptor; he died a few years after the publication of his process, and as it then became known he was far from being in prosperous circumstances, his generosity in seeking no pecuniary advantage in the sale of his process, or by securing it by patent, will be fully appreciated. Mr. Archer did not invent the substance known as *collodion*, which consists of gun cotton dissolved in ether and alcohol; but he made it available for photography. By adding an iodide or bromide, or both, to the collodion, and then, when the film had *set*, by immersion in a solution of nitrate of silver, the conversion of the iodide or bromide of ammonia (or other similar salt) into an *iodide* or *bromide of silver*, the collodion film is rendered very sensitive to light. The collodion process is in universal use, and for thirty years may be said to have had no rival. Latterly, collodion has, to some extent, been superseded by gelatine. We shall see wherein the processes differ in a future paper. We have now given the main facts relating to the history of photography. If space had permitted, we might have entered into the subject with more detail. The merest glance has been given, and it has been a matter of some difficulty to select the main facts. A volume could be filled with the interesting details, while we have attempted to tell the story in a page or two.

The art of photography, as at present practised, is of great simplicity; but, easy as it is, some experience is necessary, and many failures may be expected before the amateur can look with satisfaction on his work. His first attempt *may be* a perfect success, and so perhaps may his second be, but his third attempt may be as great a failure, and from want of experience he will be altogether unable to account for his want of success. Much may often be learnt from failures, and for the satisfaction of the beginner he may be informed that failures frequently occur in the hands of those who have practised photography for many years. No rule can be laid down for accounting for

* Nicéphore Niepce died before the pension was granted, but the partnership was continued with his son Isidore.

failures—the causes are so various, that they require in each case to be traced to their source, and not infrequently the cause will not be discovered. The amateur, therefore, must not be discouraged by failures.

Advice will naturally be sought as to the outfit required: and the reply to this is that if expense be no object, the best apparatus procurable should be purchased at the outset. Our advice is, determine what you wish to do, and then go to a dealer in photographic materials and apparatus, and he will supply a list of what is necessary. If pictures not larger than 5 × 4 will satisfy the ambition of the beginner, the expense of the outfit will not be great—£5 will supply camera, lens, and chemicals sufficient to start him in his new and, as he will soon find, fascinating hobby. Perhaps the most useful size for the camera for the tyro is one for taking pictures 7 × 5, and if a lens of the "rapid rectilinear" form be adopted, portraits as well as landscapes may be taken with it, and it will not be necessary to have a special portrait lens for that class of work, for the amateur will soon find that portraiture is not the least pleasing of the uses to which he may put his apparatus.

It is assumed that all the necessary apparatus and chemicals have been obtained, and we must now describe how they are to be used: and it is assumed, also, that the amateur will commence with the wet collodion process. Having become expert with this, he will have comparatively little difficulty in practising the newer and quicker process with gelatine dry plates, which will be described later. First of all, it is necessary that the glass used should be of good quality and perfectly clean. Patent plate-glass is, of course, to be preferred: but any glass of good quality will answer. The glass must be perfectly clean; and, to effect this, whiting or Tripoli powder may be made into a thin paste with alcohol and water—say equal parts. After the glass has been washed in clean water, a small quantity of the Tripoli paste may be dropped on to the glass, then rubbed over *both sides* of the glass, and then rinsed off under a tap of running water. The cloths used for drying the plate should not be used for any other purpose, and should be washed without soap. When dry, the plates should be polished with a clean leather, kept strictly for the purpose. If breathed on, it will be seen at once whether the glass is perfectly clean. Before coating the plate with collodion, all dust should be brushed off with a large, soft brush, kept for the purpose.

BRAIN TROUBLES.

Irritability.

Among the most characteristic signs of mental weariness, irritability may be mentioned. We use the word rather in its technical than in its ordinary sense. Nervous irritation may be indicated quite as much by gloom and melancholy, as by temper or impatience. When we find ourselves disposed to take unreasonably gloomy or unreasonably fretful views of our affairs, to be troubled or vexed (that is, made sorry or angry) by trifling matters, we may be assured that there is something wrong with us. The mischief may be bodily, or it may arise from external causes; but usually—at any rate with those who exercise the mind more actively than the body—the cause of the change is mental. It is not always easy to distinguish between these various forms of irritability. Those who are affected by the east wind can ascertain, when they find themselves out of sorts, whether the wind is easterly or not; but [illegible] probable that the mere liabilit[illegible] be thus affected is a sign of nervous weakness, which may result from mental overwork.* And there are some meteorological causes of irritability not so easily inquired into as the influence of an easterly wind. (Has it been commonly noticed, or is the experience exceptional in the writer's case, that when the mind has been heavily taxed, blusterous weather produces the effects usually attributed to easterly winds?)

Again: some of the forms of irritability due to bodily mischief are not easily distinguished from those due to mental overwork. Thus, a case is related of a young man noted for his gentleness, who, forming one evening a member of a brilliant party (his companions being of his own age), was quarrelsome and cross-grained, wrangling with, and in the end offending, everybody in the room. Two hours after he was seized with nephritic torments, caused by a calculus, which did not cease to trouble him till the next day. The writer can recall an even more striking case of the sort in his own experience. He had been struck by his own exceeding ill temper (which, utterly wrong-headed though it seemed, he felt quite unable to control), while visiting, at the request of several of the professors of Yale College, the laboratories and technological collections of that institution. He could in no way distinguish his irritability from that which he had learned to regard as the effect of over-work. But it continued (though he had had and availed himself of an opportunity for resting) for more than twenty-four hours. Soon after (for there was an interval during which the sense of ill-temper and despondency passed away), he was attacked by renal tortures, which, unlike those of the amiable young man of the previous story, lasted more than a week, and amply justified (in the writer's opinion) all the ill-temper he had displayed beforehand,—if at least the disorder of the nervous system before the attack could be measured by the intensity of the pains suffered during the attack.

Usually, however, an indefinable feeling of irritability and ill temper signifies that the mind has been overworked. So, also, does that state in which, to use a commonplace but convenient expression, everything seems to go wrong. In reality, we do everything wrong, though we may be unable to recognise any difference between our way of attending to those slight matters on which the pleasant progress of our work depends and our customary methods. We misplace this and upset that, tear, smear, blot, and so forth, not because the fates are for the time being against us, but because we are weary and overwrought (though we may not be conscious of it), and our hands and fingers are not under the usual control of the mind and will.

* Dr. Forbes Winslow describes a curious instance of morbid irritability of this kind. "A military man, suffering from severe mental dejection, was in the habit," he tells us, "of promenading backward and forward in a certain track, towards evening, on the rampart of the town in which he resided. When he walked forward, his face fronted the east, where the sky was hung with black, as was, alas! his poor soul. Then his grief pressed doubly and heavily upon him; he was hopeless and in deep despair. But when he turned with his countenance towards the west, where the setting sun left behind a golden stream of light, his happiness returned. Thus he walked backward and forward, with and without hope, alternating between joy and melancholy, ecstasy and grief, in obedience to the baleful and benign influence[s] of the eastern and western sky!" Alfieri says, in his "Memoirs," "I have observed, by applying to my intellect an excellent barometer, that I had greater or less genius or capacity for composition according to the greater or less weight of the atmosphere: a total stupidity during the solstitial and equinoctial winds; an infinitely less perspicacity in the evening than in the morning; and much more fancy, enthusiasm, and invention in midsummer than in the intervening months."

DOES THE LUMINOUS MIXTURE OF BLUE AND YELLOW MAKE GREEN LIGHT?

By Lieut.-Colonel W. A. Ross, late R.A.

Professor Helmholtz says in a lecture on "The Recent Progress of the Theory of Vision" (Second English edition, Longmans, 1881), "It is impossible to make a green out of blue and yellow light." He supports this strong statement by the following argument. "The simplest way of mixing* coloured light," is by the observer looking at a blue (or yellow) wafer placed on a table through a small flat piece of glass, so fixed as to also receive the reflected image of another yellow (or blue) wafer a little distance away. If *b* is the wafer from which rays are transmitted through the glass, and *g*, that from which rays are reflected, and they are properly placed, "it then appears as if there was a single wafer at *b*, with a colour produced by the mixture of the two real ones. In this experiment, the light from *b* which traverses the glass plate actually unites with that from *g* which is reflected from it, and the two combined pass on to the retina." In this way Prof. Helmholtz finds that "the union of blue and yellow rays of light produces white."

Similarly, Prof. Rood says ("Modern Chromatics," 1879, p. 109) "Brewster's theory of the existence of three fundamental kinds of light—red, yellow, and blue—is found in all except the most recent text-books on physics, and is almost universally believed by artists. Nevertheless, it will not be difficult to show that it is quite without foundation." Eliminating a rather silly observation, that the theory "cannot be true, because there is no such thing as colour, which is a mere sensation varying with the length of the wave producing it,"—on which basis of "argument" we might ask why Prof. Rood took the trouble to write a book about mixing colours, when "there are no such things"—the "rude" assertion is found to be supported by evidence derived from the same kind of experiment as that of Helmholtz (devised by Lambert) above quoted.

Now, I do not take upon myself for a moment to assert that the "theory" of blue and yellow lights together forming white may not be correct; all I respectfully maintain is, that facts, so far as I have been able to produce them, seem to point in the other, or Brewsterian, direction.

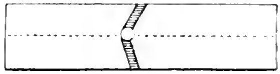
Fact 1. If we make a paste with a drop of distilled water, a little powdered *Atacamite* (or pure chloride of copper) and a certain proportion, which can be easily ascertained by experiment, of any sodium salt, and burn this paste on a platinum wire before a blowpipe, we obtain a beautiful deep green flame, or coloured light; the green colour being evidently due to the mixture of the blue light from the burning *Atacamite*, and the yellow light from the burning soda.

Fact 2. Bend about a quarter of an inch of thickish, clean, new platinum wire at a right-angle to the rest of the wire or shank, and hold it before the blowpipe, so that the yellow flame (afforded by all platinum wires in such a position until they become white hot) shall combine in a certain proportion with the blue, blowpipe-pyrocone, and you immediately obtain a distinct, though not very "strong," (from the minute quantities of the ingredients used) green light. I ascertained this fact some years ago from some platinum wire given to me by Messrs. Griffin & Co. to examine, as they supposed, from this "green flame," it contained copper; but I showed them that, not only their wire, but everybody's, could, by the above-mentioned manipulation, be made to yield a "green flame."

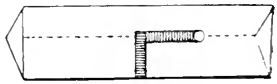
Fact 3. Chemists are aware that, by turning a gas—"Bunsen" very low, the blue pyrocone suddenly becomes green, just before the little explosion takes place which extinguishes it. The cause of this green colour is soon ascertained to be the burning of a certain proportion of "sodium" (contained in the air admitted) to the blue Bunsen-pyrocone; for, if we admit an excess of air to the "Bunsen" in full power, we find the blue "flame" almost completely yellowed. There are other ways of producing a green light from the mixture of blue and yellow lights by direct combustion: as, for instance, by introducing a rather moist piece of wood into a coal fire "burning blue," as a friend and I deliberately effected the other night at his house in Phillimore-gardens; but the above will suffice, so far, to disprove Helmholtz's assertion.

Fact 4. Let us now focus, in a semi-darkened room with open window, but venetian blind down, a sunbeam admitted through an upturned lath, by means of a largish lens (I used the object-glass of a lorgnette of 2 in. diameter, when I discovered the fact last October) upon the face of a glass prism held perpendicular to the floor. Instead of a spectrum on the wall, which the beam, without the interposition of the lens, occasions, we find the sunbeam continued within the prism, but of a brilliant grass green colour; although bubbles and other objects in the path of the green beam through the glass, reflect white light. This curious effect seems to me to be the result of the elimination of the least refrangible, or red rays, from the beam in its passage through the glass (for any-shaped, if moderately deep, piece of glass will do, but a mirror will not do), because, from their less elasticity when thus concentrated by the lens on the face of the refracting medium, they refuse to be bent like the other more refrangible rays (blue and yellow), and thus take up a different path from those; *i.e.*, are reflected and dispersed. The blue and yellow rays, left to pursue their path through the glass alone, combine to form green.

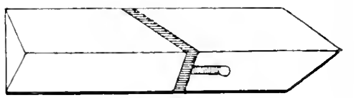
Your knowledge of mathematical optics will enable you to determine whether my ignorance of that subject is causing me to "talk nonsense" or not; but the above is the only explanation which occurred to me of this remarkable phenomenon when I observed it last October. Anyhow, the fact is, I believe, quite a new fact in optics; and, as the same phenomenon may occur whenever solar rays are collected by a lens for delivery upon a prism, as in most spectroscopes, although not with sufficient intensity to make it visible, as in the case of focussing, it would be very interesting to ascertain, by some optical analyzing process, whether the fact may not have affected spectroscopic results as now received?



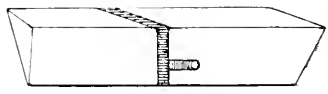
Viewed from Back.



From Right Front.



From Top and Right Front.



Viewed from Top.

From Front.

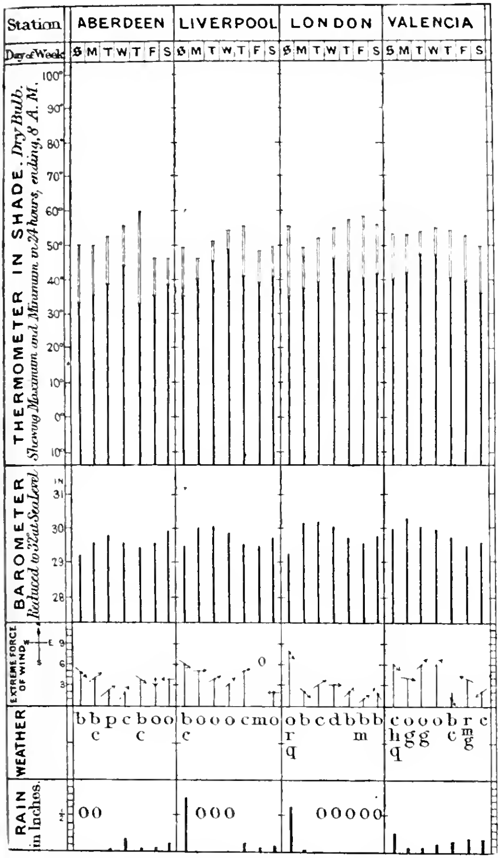
In these rough figures, the small white circle represents the focussed sunbeam impinging upon the face of a glass prism in various positions. The shaded lines show the path of the beam, now changed to a green colour, within the prism. The dotted lines are intended to represent those sides of the prism which are seen *through* it.

A common glass globular paper-weight is an excellent medium for the exhibition of this phenomenon.

* Query. Does this process really "mix" lights? I should like to see this proved.

WEATHER DIAGRAM.

FOR WEEK ENDING SATURDAY, APRIL 1.



Weather.—*Beaufort Scale* is, b. blue sky; c. detached clouds; d. drizzling rain; f. fog; g. dark, gloomy; h. hail; l. lightning; m. misty (hazy); o. overcast; p. passing showers; q. squally; r. rain; s. snow; t. thunder; u. ugly, threatening; v. visibility, unusual transparency; w. dew.

AMALGAMS.

Opinion is still divided with regard to the nature of amalgams, some considering them to be isomorphous mechanical mixtures, others true chemical compounds. The former view derives support from those cases in which amalgamation is associated with an absorption of heat, as in the solution of a salt or in dilution of a solution; the latter is supported by the fact that many amalgams are formed with a strong development of heat. A contribution to the subject has been lately made by Herren Merz and Weith, in the Berlin Chemical Society. These chemists have investigated whether, with regular heating, amalgams part with their mercury continuously or in distinct gradations.

The experiments consisted in placing the amalgam in a porcelain dish within a glass tube, contracted below, and inclosed in a second tube, having a bulb at its lower end. This bulb of the outer tube contained the substance of the vapour-bath (sulphur, mercury, or diphenylamine). To guard the amalgam from air, a lively current of an indifferent gas was passed through the interior tube while the experiment lasted. The amalgams used, which were always directly produced by known methods, contained on an average 60 to 80 per cent. of mercury. This heating was continued, wherever possible, until after several hours no decrease of weight (or hardly any) was perceptible. There were examined gold, silver, copper, lead, tin, bismuth, zinc, cadmium, sodium, and potassium amalgams. The results for the first eight are very briefly communicated, those for the last two, whose easy oxidability required special precautions, more fully. In the case of these alkali amalgams, the authors also sought to determine the melting points, but, for certain reasons, very accurate results were not reached. In general the melting points of the amalgams rise at first very quickly with the proportion of alkali metal, then gradually fall. It was thus observed that, when mercury is heated under paraffin to 250°, and then some sodium is added in portions, the whole mass solidifies with four to five per cent. of sodium; but with further addition of some percentages the mass fuses completely.

The results of their investigation are summed up by the authors as follows:—A survey of the results described shows, for a series of amalgams, that even with moderate heating they do not furnish determinate compounds.

The amalgams of gold, silver, copper, bismuth, lead, tin, zinc, and cadmium lose their mercury entirely, or nearly so even at or under the boiling temperature of mercury. Where no mercury remained, the cause is to be sought rather in a mechanical exclusion than in a chemical action. But, on the other hand, the easy decomposability of these amalgams evidently offers no proof that there are no chemical compounds in them.

For the rest, if we consider the great variability of amalgams, together with the fact that, in squeezing the so-called mercury solutions of metals, these latter do not remain behind, but certain mercury compounds, the view acquires the greatest probability, that at least very many amalgams may be, indeed, molecular combinations, but in fixed relations.

Most pronounced does chemism appear to be in the amalgams of potassium and sodium. They lose their mercury extremely slowly, even at the boiling point of sulphur, as also in a gas current, and so in circumstances highly favourable to removal of mere mixed substances. The remarkable relations, too, as regards the melting point, seem to speak for the presence of true chemical compounds. Probably these amalgams, at a comparatively low, as well as at a high temperature, consist of different compounds, none of which, however, have a durable existence, and therefore recurrent, fixed relations of composition are not to be met with. Alkali-metal amalgams of fixed composition would probably be obtained on production of larger quantities of amalgam; perhaps also by heating considerably above the boiling temperature of mercury.—*Scientific American.*

Reasoning Power in an Indian Crow.—Some years ago I sat with my partner in the verandah of our office at tiffin. Our regular guest, a broken-legged crow, sat on the window sill. In the bread basket was a piece of crust dried by the land wind as hard as a brick bat. I threw it to the crow who picked at it many times, but found it much too hard for his beak. He then evidently sat himself to find a remedy for so sad a state of things. He looked at it with his head on one side, then he took another view of it from the other side, and seemed fairly puzzled. All at once a bright thought struck him, he seized the crust, flew with it to the washhand basin in the corner, shook the morsel well for a minute in the water, flew back to his old place on the sill and gobled up the now soft crust triumphantly. If that did not betoken reason I do not know what could.—Kah Kah.

Sussex Archæological Society.—An interesting addition has just been made to the already large collection of antiquities in the possession of the society, deposited in Lewes Castle. It consists of a cinerary urn, probably of the British-Romano period, about nine inches in height. The vessel is of sun-dried clay, and about seven inches in diameter at its widest part, the mouth being about five inches. It was discovered by some labourers engaged in flint digging on Mr. Homewood's farm, at Jevington, a few days ago. They were working at the foot of Jevington-hill, and came upon several urns embedded in a quantity of loose flints, lying about two feet below the surface of the down land. There were no tumuli or other outward indications that the spot had been used as a burying-place. Unfortunately, the greater part of these relics were destroyed by the picks of the labourers before the nature of the discovery became apparent. One, however, remained intact, and this fact was communicated to the hon. secs. of the society, who at once organised an expedition to the spot. The visit was made on Wednesday, and the "find" carried off in triumph. Those present were the Rev. W. Powell, the Rev. P. de Putron, Mr. R. Crosskey, Mr. J. C. Lucas, and Mr. Griffith.

SILVERED GLASS TELESCOPES.

HAVING had a [illegible] silvered glass telescope made [illegible] Mr. [illegible] With [illegible], my experience is longer than [illegible] in this country. The instrument was used for many [illegible] of London, and for the last thirteen [illegible] the silvering lasts well for five or [illegible] years [illegible] 20th of the light is lost by portion that [illegible] occasional rubbings with a dry soft [illegible]. The silvering [illegible] on no account be touched unless it is [illegible] dry. An [illegible] of tarnish will usually vanish by exposure to the sun on a warm day. I never found real tarnish, [illegible] to give trouble, even in London. The mouth of the telescope tube is covered when the instrument is not in use, but no cover has been put over the mirror itself. As compared with a refractor of the same size, the standard power is equal in good weather; the light, of course, is less. My first observatory was composed of glazed sashes; the second of canvas, stretched on a wooden frame, strengthened with iron. In both, the internal temperature differs little from the external when the doors are open. This is of great importance. I have known several vexatious failures of good instruments in observatories with thick walls. Reflectors are more easily affected in this way than refractors. The Newtonian pattern, and moderate length of tube, enables [illegible] observations to be made in convenient attitudes, which is far from being the case with refractors. The observer should have a stool to stand upon, so as to bring the eye to the level of the eye-piece when the telescope points to the zenith; and another to sit upon, about as high as a chair, for a 6-in. or 8-in. instrument, with the top on a sliding tube, that can be fixed at different heights by a crosspiece. The construction is: one square tube inside another; a series of slots on two opposite sides of the inner one, and one slot on each corresponding side of the outer one, for the cross-piece to run through. HENRY J. SLACK.

HISTORY OF INEBRIETY AS A DISEASE.—Under this title, a communication appears in the *Detroit Medical Review*, from the pen of Dr. Crothers, of Hartford, Conn. He points out that inebriety was recognised as a disease long before insanity was thought to be other than [illegible] madness, or a possession of the devil. This disease was [illegible] at in an early age of the world, and is by no means a modern idea. On an old papyrus found in one of the tombs of Egypt dating back to a very ancient period, was a very significant passage referring to an inebriate who had failed to keep sober. Many of the sculptures of Thebes and Egypt exhibit inebriates in the act of receiving physical treatment from their slaves, such as purgatives, [illegible], or applications to the head and spine. Herodotus, five centuries before the Christian era, wrote "that drunkenness showed that both the body and soul were sick." Diodorus and Plutarch assert "that drink madness is an affection of the body which hath destroyed many kings and noble people." Many of the Greek philosophers recognised the physical character of inebriety, and the hereditary influence or tendencies which were transmitted to the next generation. Laws were enacted forbidding women to use wine, and young boys were restricted. In the first century of the Christian era, St. John Chrysostom urged that inebriety was a disease like dyspepsia, and illustrated his meaning by many quaint reasonings. This was the first clear distinctive recognisation of the disease which had been hinted at long before. In the next century Appian, the Roman [illegible], referred to the irresponsible character of inebriates, and the necessity of treating them as sick men. Many of the early and later writers of Roman civilisation contain references to drunkenness as a [illegible] disorder, not controllable beyond a certain point, which [illegible] in veritable madness. Little reference was made to this [illegible] until the thirteenth century, when one of the Kings of Spain enacted laws fully recognising inebriety as a disease lessening the punishment of crime committed when under the influence of spirits. In the sixteenth century the penal codes of France and many of the German States contained enactments which recognised the disease character of inebriety. All punishment for crime committed during this state varied according to the condition of the prisoner at the time. In 1747 Condillac, a French philosopher, wrote expressing clear views of the disease of inebriety, also that the State should recognise and provide means for its treatment. He asserted that the impulse to drink was, like insanity, an affection of the brain which could not be reached by law or religion. Dr. Benjamin Rush, of Philadelphia, in 1790, set forth the same theory, supported by a long train of reasoning. To him belongs the honour of first elaborating this subject and outlining what has been accepted half a century after.—*Medical Press and Circular.*

PREHISTORIC RESEARCH IN RUSSIA.

AT the meeting of the Imperial Geographical Society of St. Petersburg, Nov. 16, 1881, an account was given of the expedition of M. Malakhof, whose anthropological investigations, which were terminated in October last, had for their principal object the examination of the caves of prehistoric man discovered by him in 1880, and the search for traces of primitive cultivation in the Southern Oural, a region previously unexplored from this point of view. In his journey he examined the districts of Urjum and Velabuga, in the government of Viatka, a large portion of that of Perm, and the Troitsk district in Orenburg. In the first-named district he discovered, near the village of Verobieva, prehistoric remains, including a number of objects in metal and bone, and fragments of clay vases. The bones were those of the deer, bear, wild boar, beaver, horse, &c. In the Governments of Perm and Orenburg, M. Malakhof explored a number of caverns without success, but he was more fortunate in his researches on the shores of the Aiatsky, Shiguirchi, and Vurinsky lakes, and the rivers Muss and Isseta where he made a large collection of arms of polished silex and objects in bone, clay, bronze, and iron. These results lead to the conclusion that in the Oural region, the passage from the stone to the metal age took place in a wholly independent manner owing to the abundance of the metals. In one tumulus he found a skeleton with heavy copper ornaments, and in other excavations he met with a considerable number of bronze idols, amulets, and articles of gold. M. Malakhof has also copied the red hieroglyphic inscriptions found on rocks along various rivers. This find of pure copper articles in Siberia, in addition to those recently in Switzerland, renders it probable that there may have been a pure "copper age" before that of bronze, as thought by some anthropologists.

A MEMBER OF THE SOCIETY OF BIBLIOLOGICAL ARCHÆOLOGY.

SPEECH AMONGST FOWLS.—As Lieutenant-Colonel Souzel and some correspondents still keep the subject of animals and their doings to the fore, I send the following:—Some years ago I saw two young cockerels fighting in an out-of-the-way corner of a large poultry-yard. A hen, probably the anxious mother of one of the young heroes, came up, and, by voice and action, did her best to put an end to the encounter, but without success. She then went away, and presently returned, bringing with her a fine cock. The new-comer made straight for the combatants, who were again hard at it, administered a few vigorous pecks, and walked off with his loving spouse, leaving the youngsters very crestfallen and as peaceable as quakers. The cock's appearance on the scene was not accidental, the hen having gone to the other side of the yard, about thirty yards off, to find him, and having come back beside him, almost arm-in-arm with him (!). This, coupled with the fact that, on his arrival, he seemed to know exactly what to do, seems to point to some fairly well-developed means of communication between fowls, though, unfortunately, I either could not see or did not notice, what actually took place at the meeting. The incident itself is ludicrously human, and is, in fact, an Æsop's fable in real life for little boys just outgrowing the nursery. ROSEMONT GEARY.

INTELLIGENCE IN SWANS.—The following extract from Yarrell's History of Birds may interest your readers. Mr. Yarrell writes:—"I am indebted to the kindness of Lord Braybrooke for the following account of a female swan on the river at Bishop's Stortford. This swan (*Cygnus olor*) was eighteen or nineteen years old, had brought up many broods, and was highly valued by the neighbours. She exhibited, some eight or nine years past, one of the most remarkable instances of the powers of instinct that was ever recorded. She was sitting on four or five eggs, and was observed to be very busy in collecting weeds, grasses, &c., to raise her nest; a farming-man was ordered to take down half a load of haulm, with which she most industriously raised her nest and the eggs 2½ ft.; that very night there came down a tremendous fall of rain, which flooded all the malt-shops and did great damage. *Man* made no preparation, the *bird* did. Instinct prevailed over reason. Her eggs were above, and only just above, the water."—(Brit. Birds, III., p. 207, 208. Second Edition.) Two phenomena present themselves here. (1) The prescience in the bird of impending rain; (2) the means adopted by the bird to provide for the security of her nest and eggs. The first question may be, perhaps, mainly independent of any direct conscious mental emotion, and may be due, as suggested by Yarrell, to the susceptibility of the feathery covering of the bird to atmospheric electrical changes, and what we call *instinct* may have foretold impending rain; but I do not see how we can interpret the swan's act in providing for the safety of her nest, otherwise than by attributing it to direct *reasoning* power.—W. HOUGHTON.

MORNING WORK.

PERHAPS, on the whole, moderately early rising is now a commoner practice in cities than it was forty years ago. It seems strange that the habit of lying in bed hours after the sun is up should ever have obtained a hold on the multitude of brain-workers, as undoubtedly it had in times past. Hour for hour, the intellectual work done in the early morning, when the atmosphere is as yet unpoisoned by the breath of myriads of actively moving creatures, must be, and, as a matter of experience, is incomparably better than that done at night. The habit of writing and reading late in the day and far into the night, "for the sake of quiet," is one of the most mischievous to which a man of mind can addict himself. When the body is jaded, the spirit may seem to be at rest, and not so easily distracted by the surroundings which we think less obtrusive than in the day; but this *seeming* is a snare. When the body is weary, the brain, which is an integral part of the body, and the mind, which is simply brain function, are weary too. If we persist in working one part of the system because some other part is too tired to trouble us, that cannot be wise management of self. The feeling of tranquility which comes over the busy and active man about 10·30 or 11 o'clock ought not to be regarded as an incentive to work. It is, in fact, the effect of a lowering of vitality consequent on the exhaustion of the physical sense. Nature wants and calls for physiological rest. Instead of complying with her reasonable demand, the night-worker hails the "feeling" of mental quiescence, mistakes it for clearness and acuteness, and whips the jaded organism with the will until it goes on working. What is the result? Immediately, the accomplishment of a task fairly well, but not half so well as if it had been performed with the vigour of a refreshed brain working in health from proper sleep. Remotely, or later on, comes the penalty to be paid for unnatural exertion: that is, energy wrung from exhausted or weary nerve centres under pressure. This penalty takes the form of "nervousness," perhaps sleeplessness, almost certainly some loss or depreciation of function in one or more of the great organs concerned in nutrition. To relieve these maladies—springing from this unsuspected cause—the brain-worker very likely has recourse to the use of stimulants, possibly alcoholic, or it may be simply tea or coffee. The sequel need not be followed. Night work during student life and in after years is the fruitful cause of much unexplained, though by no means inexplicable, suffering, for which it is difficult, if not impossible, to find a remedy. Surely morning is *the* time for work, when the whole body is rested, the brain relieved from its tension, and mind power at its best.—*Lancet.*

A PANTHER IN VERMONT.

"F. H. H.," in the *Scientific American*, writes as follows:—It seems to me not improper that some mention should be made in your columns of the remarkable specimen of puma (*Felis concolor* L.) which was recently killed in the town of Barnard, Vermont. We are not surprised at the stories related by our forefathers of hunting wolves, bears, panthers, and other large animals on spots long since thickly settled by man, nor at the strange experiences of the woodsman when his axe was first heard to ring in the primeval forest. It is not an uncommon thing, indeed, now, for such animals as deer, catamounts, or bears to be shot or trapped in many towns on the northern border of New England; but when a full-grown puma, one of the most savage of wild animals on our continent, is taken prowling about the outskirts of a town, in a State which is settled to such an extent as Vermont, we are enabled to realise the condition of the wilderness as it once was, and the nature of those animals with which it was denizened.

The circumstances of this remarkable hunt are as follows:—

Some boys, who lived in Barnard, went out after partridges on Thanksgiving Day, Nov. 24, 1881. They soon discovered the fresh tracks of some large animal, and on following a short distance crossed their own path. Being frightened at this circumstance, and also from catching a glimpse of the animal, they hastened back to the house of a neighbour, who soon accompanied them, armed with a shot-gun, together with his son, who carried a rifle. They presently sighted their game, which they chased to a thicket, where it was dislodged several times, but finally shot. On dragging the animal out, what was at first thought to be a bear proved to be a female panther of the largest size, measuring 7½ feet from tip to tip, and weighing nearly 200 pounds. It would seem strange at first that the animal was not more savage, that he did not charge his pursuers, and kill them at once. This may, however, be partially accounted for from the fact, as afterwards appeared, that it had made its supper on two sheep in Pomfret only the night before."

This is the second or third of the species killed in the State since the beginning of the century, and in all probability it will be the last. The animal was in fine condition, being in its new fur, and showing no signs of having been previously trapped or wounded. The upper right canine was truncated at about the middle, but this might have been done in a skirmish when the puma was young. In general, the colour of the upper parts was tawny-yellow, with a darker wash of the same along the dorsal line, on the tip of the tail, the ears, and face. The whole animal presents in a striking and exaggerated manner the form and features of the ordinary domestic cat. The tail is straight and larger in diameter at the base, the neck short, the ears erect and pocketed. The dentition is precisely similar, the canines being conical, and rising an inch or more from the jaws. The paws are 7 in. wide when the fingers are spread, and conceal a very formidable set of claws.

This panther is supposed to have made the town and vicinity where it was taken, its home for seven or eight years, and on several occasions has been seen or heard from. One hundred and thirty sheep have probably fallen victims to its rapacious maw as the town records would indicate.

JELLY FISH.

A YEAR ago the Duke of Argyll made a famous *mot*, comparing the Ministry to jelly-fish, "who fancy they swim, while they only float." As the learned author of the "Reign of Law" has made so great an error, I fancy it must be general. Jelly-fish (*marjouilles*) cannot, indeed, dart about like fish proper, but they can swim, I should say, about two miles an hour. Their slowness is compensated by a faculty which enables them to know, if near a lee shore, that they must swim against the wind—otherwise they will be stranded, and die. But this faculty does not serve them so far as to announce an *opposite* shore. Thus, after a N.E. gale here, the shore is strewn with hundreds of jelly-fish, often 2 ft. in diameter. These were all, when the gale began, off the coast of Noismontier (to S.W.) ten miles away. They began to swim away from that lee shore, and after several hours' intelligent exertion, are lost at last (poor finite things!) on the *windward* coast of the bay. The fishermen say that a gale here kills, in this way, nearly all the *marjouilles*, on one coast or the other, and that for a long time hardly any are in the sea at all; whereas on an open coast they almost all effect their escape. They are not afraid of shallow water, if calm; I have often watched them, and touched them with hand or foot in bathing. They are slightly electric, leaving a sting or tingling on the skin.

It is said the glow-worm is always female, *i.e.*, the (winged) male has no light. This is not so. I have seen them crawling on the floor in the dark—a spot of light like the point of a match—and on bringing a light have found a male glow-worm. Some worms (centipedes) emit a most brilliant light when crushed.

HALLYARDS.

INTELLIGENCE IN BIRDS.

I HAVE not said anything about the crows, which are a feature of Yezo, and one which the colonists would willingly dispense with. There are millions of them, and in many places they break the silence of the silent land with a Babel of noisy discords. They are everywhere, and have attained a degree of most unpardonable impertinence, mingled with a cunning and sagacity which almost put them on a level with man in some circumstances. Five of them were so impudent as to alight on two of my horses, and so be ferried across the Yurapugawa. In the inn-garden at Mori I saw a dog eating a piece of carrion in the presence of several of these covetous birds. They evidently said a good deal to each other on the subject, and now and then one or two of them tried to pull the meat away from him, which he resented. At last a big strong crow succeeded in tearing off a piece, with which he returned to the pine where the others were congregated, and after much earnest speech they all surrounded the dog, and the leading bird dexterously dropped the small piece of meat within reach of his mouth, when he immediately snapped at it, letting go the big piece unwisely for a second, on which two of the crows flew away with it to the pine, and with much fluttering and hilarity they all ate, or rather gorged it, the deceived dog looking vacant and bewildered for a moment, after which he sat under the tree and barked at them inanely. A gentleman told me that he saw a dog holding a piece of meat in like manner in the presence of three crows, which also vainly tried to tear it from him, and after a consultation they separated, two going as near as they dared to the meat, while the third gave the tail a bite sharp enough to make dog turn round with a squeal, on which the other villains seized the meat, and the three fed triumphantly upon it on the top of a wall. In many places they are so aggressive as to destroy

crops unless they are protected by netting. They assemble on the sore backs of horses and pick them into holes, and are mischievous in many ways. They are very late in going to roost, and are early astir in the morning, and are so bold that they often came "with many a stately flirt and flutter" into the verandah where I was sitting. I never watched an assemblage of them for any length of time without being convinced that there was a Nestor among them to lead their movements. Along the seashore they are very amusing, for they "take the air" in the evening, seated on sandbanks facing the wind, with their mouths open. They are threatening to devour the settlers, and a crusade is just now being waged against them, but their name is Legion. ("Unbeaten Tracks in Japan," vol. ii., p. 149.)—From *Nature*.

FAIRY "FOLK LORE" OF SHETLAND.

LIKE all of the class to which the majority of them belong, the Shetlanders are strongly imbued with a belief in the supernatural. They seem half ashamed to confess it, but the fact is soon discovered by those who have any intercourse with them. Many of their superstitions are of Scandinavian origin, and have been handed down from their Norse ancestors.

The principal character in all their goblin tales is the drow, or fairy, a being supposed to have great power and malignity, and capable of doing great harm both to men and cattle. The drows are believed to inhabit knolls or hillocks, and when a person meets them, if he has not a Bible in his pocket, he must draw a circle round about him and say, "In God's name, keep back." In paralysis the drows are said to have taken away the affected limb, and to have put a log of wood in its place, the entire absence of sensation in the diseased member being adduced as proof of the assertion. Consumption is said to be owing to their taking away the heart, and this complaint is thought to be cured by dropping melted lead into water till a triangular piece is obtained, which is hung round the patient's neck, and placed as near as possible to where his heart ought to be.

Within the last few years witches, or wise women, have been consulted in regard to cases of sickness, by men apparently possessed of good sound sense, and some of them even elders of the Kirk. The advice given is a sort of charm, similar to those imputed to the witches of the seventeenth century, to which there are many allusions in the various Presbytery records of the period.

Touching for the king's evil is still a devout article of faith in Shetland; but in order to be effectual it must be gone about with a certain degree of ceremony. A silver coin of Charles II., one of a small number expressly manipulated by the merry monarch for the purpose, and bestowed on some favourite from whom they found their way into these islands, is first laid upon the afflicted person's tongue, and then carefully passed over the affected part, in the case of a male, by the third or seventh daughter; in that of a female by the third or seventh son, of a family belonging to one father and mother. In going through this process great precaution must be used lest the person operated upon should obtain a sight of the coin, for then the spell would be broken. We have heard it repeatedly affirmed that those who have been treated in this manner, and according to proper form, have invariably been cured, and great regret expressed that the only remaining coin in the island invested with this healing virtue is lost, and cannot be recovered.

There are also many superstitions connected with the sea. When they are about to set out to the haaf, they think it unlucky to meet a minister, to call anything by its right name, to make a false step, or to do anything else that may be ominous of trouble and disaster. Should any of these accidents occur, they will turn back and refuse to venture, although the weather be propitious, the sea calm, and everything else favourable for the expedition. When at sea the minister is called the "upstander," or the "lodar," the manse is denominated the "lodar's still hod," and the kirk the "banie house;" every part of the boat's furniture in like manner receiving a change of appellation.

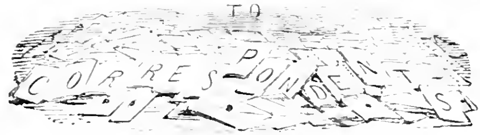
The belief in omens and portents is carried in Shetland to a greater extent than one would imagine. Talking to a man one day, we happened to mention the fact of our having seen a lamb which had been dropped very early in the season. To our surprise, he suddenly became very grave, but said nothing. The reason for this change in his demeanour turned out to be an impression that early-dropped lambs were "no canny," and were ominous of evil. "The folk at Hargaetter," he said, after a thoughtful silence, "had a lamb dropped on New Year's Day, and their son was killed by falling over the banks the same year."

The strange reluctance to save a drowning man, from the wild and absurd notion that to do so would be to incur the certainty of receiving some wrong or injury at his hands, and which is so well introduced into Sir Walter Scott's novel of "The Pirate," if it ever was common in Shetland, is now everywhere extinct, and there is not a man in the islands who would hesitate to do all in his power to aid a fellow creature if he saw him in peril from the waves. It is to be hoped that all such baneful superstitions will soon disappear before the advance of education.

A. W., in "The Antiquarian Magazine."

DIPHTHERIA COMMUNICATED BY CATS.—Dr. William Bunce, of Oberlin, O., sends a report of the following cases to the *New York Medical Record*, March 4:—On May 1, 1881, he was called to see a boy four years of age, of German parentage, and one of six children; he was found to have diphtheria. On the following day the youngest daughter, two years of age, presented symptoms of the same disease, and on the next day the father and two more children were attacked. After this date all the other members of the family, except the eldest boy, contracted the disease. A thorough examination of the house elicited no source of contagion, but in the barn a cat was found having the characteristic lesions of diphtheria. On inquiry, he ascertained that this cat during its period of sickness had been played with by the children. On August 20, 1881, he saw, with his son, Dr. W. C. Bunce, a lady, eighteen years of age, who had diphtheria of a very severe type, which terminated fatally on the third day. In a short time the disease developed in the mother and remaining two daughters. A half-grown cat in the room was found to have well-marked diphtheritic membrane in the throat; it was also ascertained that its mother and four other kittens had been in the same condition. The girls had endeavoured to cure the cats by removing the deposit, in this way exposing themselves to the contagious influence of the disease. After the recovery of these cases, and the removal of the diseased animals, the spread of the disease ceased. He thinks it fair, therefore, to conclude that the diseased condition of the cats was the cause of the diphtheritic manifestations in the cases reported. Mention is made of these cases as they are of importance in the consideration of comparative medicine.

THE WELSH W.—We have a fixed and settled conviction that the English language is the very best language in existence, and that all foreign tongues whatsoever are mere lingoes, wholly unworthy of our sublime consideration. Now there is no doubt a great deal of sound truth in this view; for even French and German philologists have been known to hold that English, because of its relative simplicity and logical development, its freedom from the childish fetters of gender and inflection, will ultimately become the common medium of intercourse for the whole world. But our firm and profound belief in the absolute superiority of our own tongue has always made us very disdainful of other people's. There is a genuine substratum of reality in the old joke about that typical John Bull who wouldn't learn French to talk to the mossoos, but thought the mossoos might learn English if they wanted to talk to him. This universal English feeling, however, seems to reach its culminating point when the foreign language with which we have to deal is Welsh. Most "Saxons" have a congenital horror and dread of the Cymric tongue, which they absurdly declare to be full of consonants and absolutely unpronounceable. As a matter of fact, Welsh is far softer and more vocal than our own harsh Teutonic speech, for it lies about half-way between English and Italian, so far as the relative predominance of vowels or consonants is concerned; and lest my reader should view this paradoxical statement with suspicion, taking me for a Welshman in disguise, I hasten to add that I am not in any way connected with Wales, and that I shared all the common Saxon prejudices on this matter myself until I began to learn a smattering of Welsh for philological purposes. Almost all the terror and mystery of those awesome combinations of letters which are wont so greatly to frighten us is removed in a moment, as soon as people have discovered the simple fact that *w* is a vowel, and not a consonant, its phonetic value being merely that of our own *oo*. Cwm and Drws look very terrible indeed until one knows that they are pronounced like Coombe and Druce; while the fearsomeness of Llwch disappears entirely as soon as we recognise that it is nothing more than the Scotch Loch in an unfamiliar guise. Yet, in spite of the perfect transparency and regularity of Cymric phonetic spelling, ten thousand English tourists continue every year to talk about those jaw-breaking long Welsh names, which are utterly unpronounceable by English lips merely because they have never taken the trouble to get up the most elementary rules of the language, as they would get up a little German before going up the Rhine, or a little Italian before trying a winter at Rome or Florence.—From "Some English Place Names" in the *Cornhill Magazine* for November.



Letters to the Editor.

[The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with their writers. All communications should be as short as possible, consistently with full and clear statements of the writer's meaning.]

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(I.) Letters to have a chance of appearing must be concise; they must be drawn up in the form adopted for letters here, so that they may go untouched to the printers; private communications, therefore, as well as queries, or replies to queries (intended to appear as such) should be written on separate leaves.

(II.) Queries and replies should be even more concise than letters; and drawn up in the form in which they are here presented, with brackets for number in case of queries, and the proper query number (bracketed) in case of replies.

(III.) Letters, queries, and replies which (either because too long, or unsuitable, or dealing with matters which others have discussed, or for any other reason) cannot find place here, will either be briefly referred to in answers to correspondents, or acknowledged in a column reserved for the purpose.

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

"God's Orthodoxy is Truth."—*Charles Kingsley.*

Our Correspondence Columns.

ELECTRIC TELEGRAPH.

[370]—It is pleasing to learn (letter 367, p. 479) that Sömmering's telegraphic apparatus is still in existence, but it must not be forgotten that the idea of the electric transmission of intelligence was not a novel one in Sömmering's day. It is to Charles Marshall, a Scotsman, that we owe the invention of the electric telegraph. He, in 1753, suggested that by erecting a series of wires, one for each letter of the alphabet, and attaching to the end of each wire a ball, near which was suspended a piece of paper free to move towards the ball, and inscribed with a letter of the alphabet, and sending a charge of electricity through first one wire and then another, the pieces of paper at the other ends might be caused to move, and so to indicate the letters of the message which it was desired to transmit. In 1774 Marshall's scheme was realised by Le Sage of Geneva. ALFRED W. SOWARD.

GHOSTS.

[371]—The following details regarding the "War Office Ghost" may be of interest to T. D. In September, 1857, Captain G—— W——, of the 6th Dragoon Guards, left England to join his regiment in India, leaving his wife at Cambridge. On the night between the 14th and 15th November, 1857, she dreamed that she saw her husband looking very ill, and she thereupon awoke, much agitated. When she looked up she saw the same figure standing by her bedside. He appeared in uniform, and seemed to be in great pain. Mrs. W—— at first thought that she must be still asleep, but, by rubbing her eyes, and by listening to the breathing of a child beside her, she became convinced that what she had seen was no dream. In December, 1857, a telegram from the seat of war appeared in the morning papers stating that Captain W—— had been killed before Lucknow on the *fifteenth* of November. This date was further confirmed by the War Office certificate, which was obtained by the family solicitor. Mrs. W——, however, maintained that her husband had died on the *fourteenth*, and not on the *fifteenth* as stated. While the solicitor's mind remained in uncertainty regarding the real date of the death, a curious incident occurred which seemed to confirm Mrs. W——'s opinion. The solicitor mentioned the case to a lady friend, who all her life had had perception of apparitions. She immediately, turning to her husband, said, "That must have been the same apparition that I saw on the evening we were speaking about India." The receipt for an account paid on the same day enabled them to fix on the *fourteenth* as the date. The solicitor was so much impressed by this that he applied to the War Office to find out whether there had not been some mistake about the date. The officials stated that there could be no mistake, as the death was referred to in two despatches from Sir Colin Campbell, and in both the date was given as the *fifteenth*. In March, 1858, a letter arrived from a brother officer, giving an account of Captain W——'s death. This officer, who had been riding beside Captain W—— when he was killed, stated that the death occurred on the *fourteenth* of November. Finally, about a year after the death, the War Office altered the date to the *fourteenth*. JOHN GORDON.

MOON CRATERS.

[372]—*A propos* of the notice at p. 439 of imitations of moon craters, I was greatly struck only a few days since by the very close resemblance to such craters in the impression produced on a smooth surface of sand by a drop of water falling on it from a height of about five or six feet. This is what happened in a greenhouse on the moisture of condensation dropping into a pot sown with seed and thickly covered with sand.

Perhaps, as a small contribution to KNOWLEDGE, this observation may be acceptable. J. POWER HICKS.

COLOURS AT NIGHT.

[373]—Can you tell me what is the explanation of the fact that if a strip of cloth coloured either white, black, or blue be hung up on a pitch-dark night in a place where no lights are visible, and against the sky as a background, it becomes swallowed up, as it were, in the darkness, and is invisible; but that if its colour be red, it stands out as a dark patch on the black sky? I have frequently observed this to be the case, so that I am sure of my fact.

WINTER.

GOLDEN SANDS.

[374]—On the Western coast of India there is a river. To the north of its mouth on the sea beach, during the south-west monsoon, appear many patches of black sand (grains of magnetic iron ore). These patches average five or six square yards area, and are about half-an-inch thick, and lie on the surface of the ordinary yellow sand of the beach. The gold-washing natives carefully scrape off this black sand, during the rains, as fast as it appears, and make heaps of it on the higher beach beyond the reach of the sea at high tides. When the rains are over they wash these heaps for gold, using first a small wooden cradle and then a shallow gun-metal dish, such as is commonly used by natives for their rice, and called a "Kinny." When the black sand is almost all washed away they use quicksilver, make an amalgam, squeeze the excess of mercury out through a piece of washleather, and get rid of the rest by heat, leaving from two to six annas (threepence to eightpence) worth of gold for their day's trouble. My trouble is not how they get the gold from the sand—that is easy enough, I have done it myself a hundred times—but how did the gold find its way down the river and on to the surface of the beach? and why always in company of the black sand?

Not one particle of gold is ever found in the yellow sand. There is no adhesion or attraction between the black sand and the gold, for the latter is as easily washed from the former as from yellow sand when mixed for experiment. I have tried it often. Why did not the gold-dust find its way to the bottom of the river, as it did at once to the bottom of the pan? That is a PUZZLER.

JUPITER IN CASSIOPEIA.

[375]—With reference to your editorial remark respecting my communication on "Jupiter in Cassiopeia" (correspondence column, p. 478), I beg to say the context of the refreshing passage in Schiller's "Wallenstein," naturally suggests the interpretation I put on the words of the poet, and every German reader who knows the force of *dahin* and has, besides, even an elementary knowledge of astronomy only, would at once understand it to mean "yonder." If Schiller had wished to make Wallenstein say, "that he saw Jupiter *in* Cassiopeia," he would certainly have used *darin*, or some similar expression. May I add that few poets have so carefully and accurately worked out the details of their productions as Schiller has done. In his brilliant tragedy of "Wallenstein," he has shown that he had fully mastered the "subject of astrology," and it is quite natural that he should have at the same time occupied himself with the science of astronomy, if he had not done so before.

C. A. BUCHHEIM, PH.D.

[illegible]

376.—W[illegible] the writer has not [illegible] Knowledge [illegible] W. M. W[illegible] in the places, wh[illegible] [illegible] currency has [illegible] and [illegible]. Has he considered [illegible] without a [illegible] of bread all this time? M[illegible]. Of course we must wait another [illegible] to him, to you, Mr. Editor.

C. J. Brown.

STARCH IN POTATOES.

[3]77.—In answer to [illegible]'s last question (329), it may be [illegible] found that the ratios between the starch and nitrogenous matter of potatoes are not always the same, and that in those potatoes which are the last floury the ratio of nitrogen to [illegible] is the highest by reason of decrease in starch. The cause seems to rest with the manure, for the same variety, under like conditions of position, soil, and climate, is greatly influenced in composition by the manure applied.

E. W. P.

HAIR TURNING WHITE.

378.—Perhaps the following facts may be of sufficient interest to warrant their insertion in Knowledge. James Trafford, a coach-smith, in the employment of Francis Mulliner, coachbuilder, of Liverpool and London, about two-and-a-half years ago burned himself accidentally at the root of the middle finger of his right hand. The injury was caused by a bit of red-hot iron flying into his hand. He was obliged to cease work at once, and continued to be incapable for about three months, during which time I watched the consequences of his accident as they made their appearance. He poulticed his hand, and in about a week the swelling broke, upon which matter began to exude. In a week after the breaking of the wound, and while the matter was exuding, his hair began to turn white upon the right side of his head, and a fortnight subsequently he presented the appearance of a man with the hair, moustache, and beard, on the right side of his head, snow white, and on the left side jet black. The process continued, the matter was still exuding. Five weeks after his accident, every hair on his head and face was perfectly white. At this time, he was only about forty-two years old. Ten weeks after his accident he was so bald that not a single hair was to be found on his head. A week later his hand healed, and the matter ceased to run. Another week and his hair re-appeared, and grew so fast that in about a month his head and face were again covered with hair. Six months after his accident, his head and face presented their usual appearance—jet black hair, moustache, and beard. I obtained his consent to send this account to another journal, but omitted doing so. It has, therefore, never before been published.

H. Smith.

THE LANK YANK.

379.—Reading on page 457 the remarks of "Canadensis" in this connection, I would add that, having visited New York City in the days of old, besides being collided with many Yanks upon the Australian gold-diggings, it struck me that the leanness of the American is due to the abominable practice of chewing honeydew, accompanied by the constant ejection of the saliva so necessary for the due assimilation, secretion, and digestion of the dietetical delicacies incident to their *Déjeûners à la Fourchette*, &c. Thus was I led to speculate upon the question, "How many of the leaner sort of Americans would suffice to cool the condenser of a 10 H.P. engine?"

J. J. A., Liverpool.

THE PERFECT WAY IN DIET.

380.—May I correct two misprints among the names of advocates of reform in diet cited by me? For Halley read Haller, the well-known German physiologist of last century; and for Daumen read Daumer, the learned Professor of Munich, popularly known as the (for some time) guardian of Gaspar Hauser, the wild boy of the wood. Also, for Boyle read Bayle, the eulogiser of Gassendi.

It is not pretended that all the authorities quoted by me in your last number, or the very many other admirable writers omitted in my necessarily brief notice, were, in practice, anti-kreophagist. Until some forty years ago there was no kind of Association, no rallying centre for those deeper thinkers, or more humane minds, who, nevertheless, instinctively, as it were, shrank from the cruel barbarism of the slaughter-house. No writer (not actually an abstainent from flesh-meats) has more often protested against the [illegible] practice of butchering for food than Voltaire (See, *inter alia*, "Essai sur les Mœurs," &c., his romance of "La Princesse de Babylone," Dict. Phil., art. "Viandes," &c.). It is entirely reasonable to assume that, were he now living, he would be practically, no less than theoretically, a humane dietist. And so of the other prophets of reformed dietetics who lived before the present era of associated reformers.

Howard Williams, M.A.

Queries.

349.—Quotation.—In what literary work shall I find "Little things on little wings bear little souls to heaven"?—F.

350.—Jet.—What is the composition of jet, and how may it have been formed in the rocks? Has it ever been chemically analysed, and with what result?—Whitby.

[351]—Glycol.—Can any reader give the following:—1. Test for glycerine. 2. A practical means of separating glycerine from the residue from palm oil, after the latter has been heated to, say, 320°, and agitated with H_2SO_4?—A. T. Ross.

352.—Will one of your readers be kind enough to tell me the best method to procure deutoxide of nitrogen from nitric acid?—H. Nelbo.(?)

Replies to Queries.

[276]—Photography.—For "quality," read "quantity;" and for "prosulphite," read "hyposulphite" of soda.—A. Brothers.

[330]—Climbing Plants.—If W. P. B. will look at Darwin's "Climbing Plants" he will see that the twining, though influenced (*i.e.*, retarded or accelerated), is not caused by the sun. In a list there given of twining plants, fourteen follow the sun (hop), twenty-seven move against the sun (convolvulus). A single revolution is often performed in from six to three hours, or less; not 24.—S. C. Wood.

[336]—In Memoriam.—We have received many replies to this query. Nearly all agree that Longfellow is the poet referred to, naming his poem, "The Ladder of St. Augustine." This reply is given by S. Oliver, K., Clare, S. C. Wood, T. Sulman, J. Stewart, H. J. Fry, Chas. H. Cotton, T. H. M., E. D. G., A. P. Thomas, J. S., H. A. H., T. B. E. C. R. suggests that either Longfellow or St. Augustine was intended (but how could St. Augustine be described as he "who sings to one clear harp in divers tones"?); Gog Magog names Longfellow, but speaks of his poem, "The Balm of Life"; W. C. says Shakespeare, but quotes no passage; Bernerd Batigan also names Shakespeare, *passim*, but especially in the sonnet beginning "Poor soul, the centre of my sinful earth" (in which we can trace very resemblance to the idea referred to by Tennyson); C. Fen says King David; W. G. Finch thinks that reference is made to the Son of David; T. M. Blackie, F.S.A., thinks Arthur Hallam was meant. To myself it seems altogether clear that Tennyson meant a poet living when he wrote, and that that poet was Longfellow.

369.—Telephone.—From ¼oz. to ½oz. of No. 36 silk-covered wire is required for bobbins. I will try to forward description of transmitter by next week.—G. E. V.

[In answer to G. T. W. M., page 459 of Knowledge, March 24, 1882, there is a book by Dean Alford, "Queen's English," published by Bell & Son, at 5s.—T. G. T.

Replying to query on "Colliery Spoil Banks," p. 477, there is no doubt that the ignition is "spontaneous"—a term rather vaguely applied. All *ordinary* combustion is simply a combination of the combustible substance with the oxygen of the air. Such combustion is going on wherever ordinary vegetable matter is exposed to air and moisture. The leaves that fall from the trees in autumn burn during the winter; so do the dead weeds, grass, &c. In their burning, they raise the temperature of the soil to an extent that is quite measurable when they are abundant. In crossing forests during winter-time, I have on several occasions been nearly ankle-deep in vegetable mud, while the ground outside the wood was frozen hard. This was mainly due to the slow combustion of the fallen leaves. The "spontaneous combustion" of haystacks, of heaps of cotton-waste, and the heat of hotbeds is of similar origin. I have seen a railway siding on fire at Coed Talon, in Flintshire. It was ballasted with cannel-coal slack, which combined with oxygen in this manner. The spoil banks of collieries are made

up of the rock that is taken out in sinking the pits and driving the roads, and most of the rocks—"the coal measures," as the miner calls them—contain vegetable matter, the most characteristic being the "linstay" or "linsey" rocks, so named by the collier on account of their resemblance to the striped "linstay" or "linsey" of which his wife makes her petticoats. This rock consists of alternate bands of black vegetable matter and of sandstone. The friable shales of the coal measures are similarly interstratified. It is this vegetable matter (in some cases increased by coal screenings) that fires on the pit bank. The tendency to such combustion is increased by the presence of iron pyrites—a compound of iron and sulphur, both of which are combustibles and do burn, or oxidise, readily when exposed to air and moisture. Iron pyrites is especially liable to such spontaneous combustion, and is thereby converted from its gold-like insoluble form into the readily-soluble "green vitriol," or sulphate of iron. This explains the sulphurous fumes to which "One who wants knowledge" alludes.—W. Mattieu Williams.

Answers to Correspondents.

**** All communications for the Editor requiring early attention should reach the Office on or before the Saturday preceding the current issue of* Knowledge, *the increasing circulation of which compels us to go to press early in the week.*

Hints to Correspondents.—1. *No questions asking for scientific information can be answered through the post.* 2. *Letters sent to the Editor for correspondents cannot be forwarded; nor can the names or addresses of correspondents be given in answer to private inquiries.* 3. *No queries or replies savouring of the nature of advertisements can be inserted.* 4. *Letters, queries, and replies are inserted, unless contrary to Rule 3, free of charge.* 5. *Correspondents should write on one side only of the paper, and put drawings on a separate leaf.* 6. *Each letter, query, or reply should have a title, and in replying to letters or queries, reference should be made to the number of letter or query, the page on which it appears, and its title.*

Lady Stewart. Regret very much that an advertiser in our columns has been so dilatory (I trust that is the limit of his offence). Your letter has been forwarded to the publishers, and due measures will be taken.—Gladiator. Fencing hardly a scientific subject. As it chances to be a favourite exercise of mine, I will give my own ideas about your difficulty. You ought, considering the name you adopt, to be able readily to meet the *un, deux*, and *un, deux, trois*, which your opponent is so fond of using, in the ordinary way. If I were you, I would not give in till I had acquired the knack. But, if you cannot, try what Mrs. Glasse would call "another way." Follow him with the round parade. This will bring him up in carte, instead of carte over the arm, and all he can do is to try carte over the arm again, which you can treat in the same way, following up his blade. Remember, though, that this parade is not in itself a defence. A good fencer will corkscrew in upon you, if you forget this. The parade simply changes the direction of your guard. Thus if you meet a thrust in carte over the arm with the round parade, you are engaged in carte and must defend accordingly, that is towards the left, not towards the right, as in the simple parade for that thrust. So if you oppose the parade to *un, deux*, &c., you are safe enough during your opponent's extension, but as the lunge is delivered the parade merges into defence in carte. The advantage of the round parade is that you know exactly where your opponent's blade will be when the thrust is delivered.—Priam. You may be surprised to learn that many of the marks of parenthesis you counted so carefully were added *after the papers were in type*, because, on careful consideration, they seemed more suitable than commas. Macaulay (whose name you misspell Macauly) used to boast that he seldom used the semicolon (not the parenthesis.) Whately, who severely rebuked the improper use of the parenthesis, used it more freely than most writers. In fact, every logical writer must use them, to write clearly and correctly. If it were not in very bad taste to comment on faults of style, I would point out that your letter would be all the better (at any rate, much more easily understood) if you had used the parenthesis more freely. Take, for instance, this sentence:—"It has been on my mind ever since the first number of Knowledge to call attention (in a friendly way) to a small evil, on the part of the editor himself, and (in degree) of some contributors, of the constant use of the parenthesis in the articles." Here, I have added parentheses where they are obviously not merely necessary, but essential to the grammatical accuracy of the sentence. Without them, you might mean "a friendly way to a small evil," and "in degree of some contributors." Your first sentence would further be improved by the addition of a dash after "evil" and "contributors," though, even then, "a small evil of the constant use" would be hopelessly ungrammatical. In fine, my dear sir, every sentence of your letter suggests the repetition in your case of a small quotation I have already had to use in reference to correspondents who, having evidently had small experience themselves, undertake to teach me how to do what has been the business of my life. "Teach not a parent's parent," I would beg of you, "to extract the embryo juices of an egg by suction;" the good old lady can the feat enact quite irrespective of your kind instruction.—Arthur Buchheim. Do you make no distinction between the ordinary use of the term logarithm and its use in the theory of functions? Or, supposing you do draw such a distinction, do you suppose our querist referred to the latter use of the term? What you so carefully explain to me is as familiar as the interpretation of imaginary expressions (of the imaginary cube roots of unity, for instance). Of course, log $(-a)$ has its meaning, just as $\sqrt{-a}$ has; but equally of course that meaning is outside the ordinary use of the function, precisely as $\sqrt{-a}$ means something which cannot be defined in ordinary arithmetical or algebraical terms.—R. W. R. Yes, that is a fair enough account of apparent planetary motions.—J. Smith. Have never heard that telescopic work injured the eyes appreciably. Galileo was certainly not blinded by ordinary telescopic observation. Milton died blind, but that does not prove that making poetry hurts the eyes.—William G. C. Yes, "the subject has taken up space in Knowledge,"—scarcely a reason for continuing the discussion.—H. Judge, T. G., E. C. R., A Constant Reader of "Knowledge," J. Harvey, Spoopendyke, and others. Queries vague, trivial, or for other reasons unsuitable. In future, questions relating to books on special subjects cannot be admitted, though regular contributors on such subjects may indicate books they consider suitable. Advantage has been taken of our Query column to ask questions and to answer them in such a way as to advertise books.—Ouprts. The query escaped our attention or it would not have been inserted. Of course, "Clubs are trumps" is the only correct expression.—M. Soc. Bib. Arch. Third part shall appear very soon.—Jos. Gainswood. The appearance in question has no scientific interpretation, though several well-meaning (but rather foolish) attempts have been made to find one. The story seems to have had its origin in astrological fancies.—Excelsior. Thanks. Corrections made.—Cambrian. Yes; all satellites were probably formed that way, and their greater or less distances would indicate greater or less antiquity.—Clare. Most readers know about Dean Alford's book on the Queen's English. The other matter is very aptly described by you as "no business" of a certain correspondent's.—A. C. Day. Those combination systems have always failed in the working.—Hint. Very likely I may. "I do not say" I won't: "but *Time*, my Christian friend."—Alice Bodington. Thanks, but you will have noticed that Mr. Clodd is attending to Palaeolithic Man.

BIOLOGY.

H. St. Maynard asks for the name of any good work on "Ants." He should consult Sir John Lubbock's "Scientific Lectures" (Macmillan), and also the "Transactions of the Linnaean Society," for much original information respecting the habits of these insects. The price of Lubbock's "Scientific Lectures" will be ascertained on application to the publishers, Bedford-street, Covent-garden.—A. Pontwood asks how Huxley divides the two orders of birds (*Ratitæ* and *Carinatæ*). This is a sample of a question, the answer to which would simply represent a chapter of the most technical details of comparative anatomy. I advise "A. Pontwood" to consult Huxley's "Vertebrata:" but I may add that the *Ratitæ* (ostriches, &c.) are classified by the structure of the wing and by the nature of the haunch-bones. The *Carinate* birds are divided into sub-orders, primarily by the nature of the vomer-bone of the skull, and by its relations to other bones (*e.g.*, the maxillo-palatines). Thus, the *Dromæognathæ* (Tinamous) have the vomer, &c., resembling the emus. The *Schizognathæ* have "the lateral maxillo-palatines ununited in the middle line, separated from the vomer by a fissure, and with the vomer pointed in front"—and so on. Unless "A. Pontwood" is prepared to enter upon a study of comparative anatomy, he need not attempt to understand the details concerning which he writes.—A. J. C. W.—Diet of Tortoises. Vegetable matters, especially lettuces and the like; a little milk occasionally.—Aquarium. See Mr. Gosse's book on the "Aquarium" (Van Voorst).—Newts. These amphibia should be kept (if water newts) in clear water, with growing plant-life, and with a dry resting-place admitting of their leaving the water occasionally. Food, worms chiefly. They are common in ponds in the country, or may be found beneath stones near water. See Bell's "British Reptiles."—J. Hamson. 1. The human skeleton you speak of found in the cave at Mentone, and spoken of by Rivière (Paris, 1873), was found in March, 1872. The cave is called La Barma du Cavillon. No metal nor any polished flint was found associated with it. The skull was dolichocephalic; and the whole surroundings (as well as the teeth) indicated a savage life. The skeleton is of *Palæolithic* age

The [illegible] rad[illegible] [illegible] accorded [illegible] the [illegible] 2. The [illegible] Dr. Schaaffhausen [illegible] that it was too imperfect to [illegible] the facial angle, he assumed that it belonged [illegible] developement. Mr. Busk says [illegible] individuals of European race, and [illegible] gists and anatomists agree. This skull is of [illegible] Huxley says that "the capacity of the interior [illegible] has not been ascertained ("Man's Place in Nature," page 128). Both Neanderthal and Engis skulls belong to the Post-glacial period. The former is certainly the lowest type of human skull known. 3. Mr. Fiske's "postulate" (KNOWLEDGE, [illegible] No. 11) simply recapitulates an underlying assumption of the whole statement of the case for evolution. It is a needful postulate for the person who is ignorant of, or opposed to, evolution. For one like Mr. Hamson, who admits the truth of evolution, it is simply an additional proposition.

ELECTRICAL.

WM. FLEMING. Good examination certificates have some little weight with electrical engineering firms when selecting their [illegible], but the industry is very young, and practical knowledge is, therefore, generally looked for. Try the examinations of the City and Guilds Technical Institute, Gresham College, E.C. The secretary will send particulars on speedy application. You may study with advantage Sprague's "Electricity," Culley's "Telegraphy" (the latest edition—seventh), and Urquhart's "Electric Light."—G. H. HARRIS. It is not so unusual as you imagine to see a magnetic needle in which both ends are of the same polarity. If you carry your experiment a little further, you will find the other or opposite pole in the middle of the needle, there being in this case two neutral zones. The centre pole will, of course, be equal in strength to the sum of the two end poles.—JAMES GRUNDY. 1. The manganic dioxide and carbon in the Leclanché cell should be about equal in quantity. Covering in the porous pot with pitch or marine glue keeps the contents in position, but be careful to leave a vent-hole for the escape of any gases that may be evolved in the cell. Use crystallised sal ammoniac and water for the outer cell. 2. You can only determine the value of the current obtained by measurement. Have you the means of doing this? 3. What is the specimen of wire you enclosed? Is it aluminium? It is too dirty-looking for platinum, nor does it test like it. The resistance of about one foot of it is only 0·016 of an Ohm. One hundred Leclanché cells failed to raise a couple of inches to redness, but 140 cells make 3 feet red hot. 4. The electro-motive force of a Leclanché cell is about 1·6 Volts, the Grove being 1·8.—W. G. FINCH. Electrical tricycles are not the only things missing in the Crystal Palace Exhibition. To work a tricycle you would require a motor, with a battery, or its equivalent, to work it. I know of none before the public, but believe one is soon to be offered by a Manchester house.—A. E. SMITH. A series of articles on dynamo-electric machines is now appearing in this journal. I am not quite sure what you require. If you do not see it, write again.—S. B. Galvanic batteries of the Daniell, Leclanché, and bichromate types are used by the Postal Telegraph Department. The electric light *could* be produced by them, but neither efficiently nor economically. £5 or £6 spent in Bunsen or Grove cells would yield an electric light for three hours or so. H. A. B. "Lightning protector" is a better name than "lightning conductor." Its chief work is to *neutralise* the electricity of the overhanging cloud by discharging into it an equal quantity of the opposite kind, which is drawn or attracted from the earth by the electricity in the cloud. Should it happen that the protector cannot discharge quickly enough, then the cloud-charge strikes towards the earth; and as the protector is the nearest object, and offers the easiest means of passage, the electricity from the cloud passes through the protector to the earth. Consequently, the more *good* protectors you have, the safer will your house be, as each one added is another possible channel of escape.—J. R. WILSON. Read preceding answer. The proper height of the lightning protector will depend on the area it is intended to protect. Copper, in the band or rope form, with platinum points, is the best material. As, however, there is considerable diversity of opinion on the question of measurement, &c., the subject will be dealt with in an article in this journal a week or two hence, when it is hoped you will find all you require.—C. H. W. Dynamo-electric machines are of so recent a birth that it is hardly fair to look just yet for any great flood of literature upon the subject. Efforts will, however, be made to supply shortly the information required by those who are anxious to make machines for themselves.

Our Mathematical Column.

PROBABILITIES.

To the two examples in our last the following may be added:—

3. *How many trials [illegible] a full pack [illegible] even chance of cutting [illegible] card [illegible]?*

The chance of failing to cut the given card in a single trial is $\frac{51}{52}$, in two trials $\left(\frac{51}{52}\right)^2$, and so on. If n be the number of trials, which will give an even chance, we have:—

$$\left(\frac{51}{52}\right)^n = \frac{1}{2}, \text{ or } \left(\frac{52}{51}\right)^n = 2.$$

Hence, taking logarithms:—

$$n\,(\log. 52 - \log. 51) = \log. 2,$$

$$\text{or } 81331n = 3010300$$

$$\text{whence } n = \frac{3010300}{81331} = 35\cdot7$$

whence 36 trials will give slightly more than an even chance of drawing the named card.

4. *In how many trials will there be an even chance that dealer at whist will have all the trumps.*

The number of possible hands which dealer can have is the number of combinations of 12 cards which can be made out of 51, and this number it will be found on trial is 158,753,389,900. Hence the chance that he will *not* have all the trumps,

$$= \frac{158,753,389,899}{158,753,389,900}$$

and to give the number of trials, n, in which there will be an even chance that dealer will have all the trumps, we have the equation

$$\left(\frac{158,753,389,899}{158,753,389,900}\right)^n = \tfrac{1}{2}$$

$$\text{Whence, approximately, } \left(\frac{15,875}{15,876}\right)^{\frac{n}{10,000,000}} = \tfrac{1}{2}$$

$$\text{And taking logarithms, } \frac{n}{10,000,000}(\log. 15,876 - \log. 15,875) = \log. 2$$

$$\text{or } 273,555n = 30,103,000,000,000,000$$

[To get the difference 273,555 corresponding to logarithms to ten places of decimals subtract logarithm of 15,860 from logarithm of 15,890, and divide the difference by 30. This gives near approximation to the difference between logarithm of 15,876 and logarithm of 15,865. But to get nearer still, do the same with logarithms of 15,861 and 15,891; add the results thus obtained, which are respectively 27,357 and 27,353, and halve—giving 273,555.]

$$\text{or } n = \frac{30,103,000,000,000,000}{273,555} = 110,100,000,000$$

This, then, in round numbers, would be the required number of trials to give an even chance of a deal in which all the trumps would be in one hand.

PROBLEM 5. *When the number of possible events, all equally likely, is very great, to determine what ratio the number of trials necessary to give an equal chance of any given event bears to the total number of possible events.*

Let the total number of events be N, then the chance that any given event will not occur in a single trial is $\frac{N-1}{N}$; and if n be the number of events necessary to give an equal chance of the occurrence of that event, we have, as in the previous case,

$$\left(\frac{N}{N-1}\right)^n = 2$$

$$\therefore \quad n\left\{\log N - \log.(N-1)\right\} = \log. 2$$

$$\text{or } 2n\mu\left\{\frac{1}{2N-1} + \frac{1}{3(2N-1)^3} + \&c.\right\} = \cdot3010300, \text{ where } \mu \text{ is}$$

the *modulus* of the common system of logarithms, or the reciprocal of the Neperian logarithm of 10; whence, when N is very great so that $\frac{2n}{2N-1} = \frac{n}{N}$

$$\frac{n}{N} = \frac{\cdot3010300}{\mu} = \frac{\cdot3010300}{\cdot4342945} = 0\cdot6931472 \; (= \log._e 2)$$

If we apply this rule to the previous case we get a value very close indeed to that already obtained in another way.

Our Chess Column.

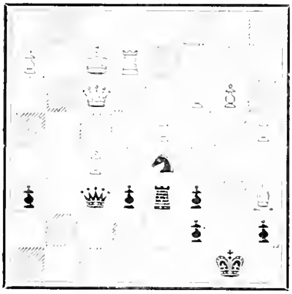
GAME BY CORRESPONDENCE.—(*Continued from p.* 485.)

Position after White's 29th move.

P to B5.

CHIEF EDITOR.

WHITE.



BLACK.

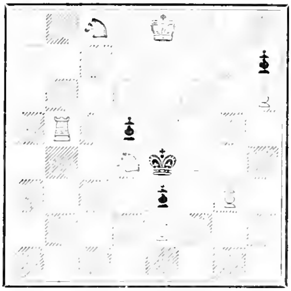
CHESS EDITOR.

	29. P takes P
30. R to Q5	30. P to B5
31. Q to R3	31. Q to Qsq
32. Q to R 5	32. Kt to B3
33. Q to K2	33. Q to KBsq
34. Q to B3	34. R to Q4
35. K to Bsq	35. R takes R

PROBLEM No. 25.

We think it best to republish this Problem in an amended form.

BLACK.



WHITE.

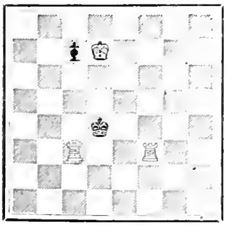
White to play, and mate in three moves.

PROBLEM No. 33.

"Easy and neat."

By C. H. BROCKELBANK.

BLACK.

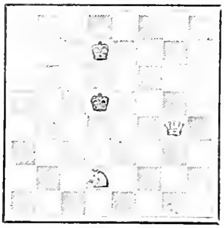


WHITE.

White to play and mate in three.

PROBLEM No. 34.

BLACK.



WHITE.

White to play and mate in three.

TWO KNIGHTS' DEFENCE.

WHITE.	BLACK.	WHITE.	BLACK.
DE RIVIÈRE.	MORPHY.	DE RIVIÈRE.	MORPHY.
1. P to K4	P to K4	11. Kt to R2	Kt to R2 (*c*)
2. Kt to KB3	Kt to QB3	12. Kt to Q2 (*d*)	P to KB4
3. B to B4	Kt to B3	13. Kt to Kt3	B to Q3
4. Kt to Kt 5	P to Q4	14. Castles	B takes Kt
5. P takes P	Kt to QR4	15. K takes B	P to B5 (*e*)
6. P to Q3 (*a*)	P to KR3	16. Q takes P	Kt to KKt4
7. Kt to K5	P to K5	17. Q to Q4 (*f*)	Kt to KB6 ch (*g*)
8. Q to KB3	Kt takes B	18. P takes Kt	Q to R5
9. P takes Kt	B to QB4	19. R to KRsq	B takes P
10. P to KR3 (*b*)	Castles	20. B to Q2	R to B3 and wins

Notes by Mr. J. Gunsberg in the *Chess Players' Chronicle.*

(*a*) A weak continuation of the Two Knights' Defence, and one which, in our opinion, gives Black the advantage. There are various ways of continuing besides P to Q3, but we are satisfied only with the result of one variation, viz., 6B to Kt5 ch 6P to B3, 7P takes P 7P takes P, 8B to K2, as enabling White to retain his advantage.

(*b*) Instead of this unsatisfactory move, it has been suggested by Mr. Proctor to play 10P to QB3; this provides a square of refuge for the Knight on Q4, and also threatens an advance of Pawns on the Queen side; but even this, in our opinion stronger move, does not quite equalise matters, which, through 6P to Q3, rest on a weak foundation.

(*c*) This is against modern "principles," as Mr. Potter would call it. The adopted way of continuing for Black is P to QKt4; he thereby tries to weaken the Queen's side, also relying upon subsequently playing B to QR3.

(*d*) We should have preferred 12B to K3.

(*e*) Taking advantage of the position in a forcible manner; had White not taken the Pawn, his game would, nevertheless, be in a precarious condition.

(*f*) Q to Q3 would have afforded a much better defence.

(*g*) This brings the game to a fine conclusion. White has no defence.

ANSWERS TO CORRESPONDENTS.

⁎⁎ Please address Chess-Editor.

Dummy. See answer in No. 20, p. 412.

John Fairweather.—Pawn on 8th is what you please to make it.

G. W. Thanks for good wishes. Have you begun your games? Solutions correct.

H. A. N.—27, 28, 29—solutions incorrect, see next number.

A. H. Empson *v.* G. Tow.

H. A. N., J. F. Washbrook, J. Griffiths, A. B. Palmer, John Fairweather.—We have corrected and republished this problem this week.

Correspondents whose opponents have not replied to their games can have fresh opponents on application.

SIR,—I see by the "Answers to Correspondents" that there are a good many dissatisfied spirits (unreasonable people!—ED.) amongst the subscribers to KNOWLEDGE. Some want more of one thing, and some more of another, and they all seem to find something that (in their opinion) might be done away with. Now, I, for one, should be sorry to see any of these radical changes, as I think KNOWLEDGE is well worth the money, even if taken only for one subject. I am quite satisfied that there are higher-priced papers with a far worse Chess column, and that, I must say, has most attractions for Yours truly, G. W.

DR. CHEYNE states of Sir Isaac Newton, that when he applied himself to the investigation of light and colour, to quicken his faculties and enable him to fix his attention, he confined himself all the time to a small quantity of bread with a little sack and water, without any regulation, except that he took a little whenever he felt his animal spirits flag. . . . The happy medium which Newton endeavoured to maintain, was just that which would preserve the blood in the fittest state for the purposes of the mind, while intently acting on the brain; and probably not a little of the splendid clearness of his demonstration may be attributed to the success with which he controlled all his bodily propensities, by the moderation which he invariably observed in the management of his stomach.—Extract from "The Use of the Body in Relation to the Mind." 1846. By GEORGE MOORE, M.D., Member of the Royal College of Physicians.

Our Whist Column.

[illegible]

[illegible] AC win [illegible] 3 [illegible]
2nd [illegible] BD [illegible] 3
[illegible] AC [illegible] 5

[illegible] AC [illegible] 1—3 [illegible] 1.
[illegible] 5 [illegible] 5 [illegible] 6.

[illegible] 6 points to the [illegible] with 4 tricks [illegible]

[illegible] English game with and [illegible]

1. [illegible] AC [illegible] tricks } AC win treble.
[illegible] 7 [illegible]
2. [illegible] 1 [illegible]
2 [illegible] 1 trick [illegible] honours, BD win single.
3. [illegible] AC [illegible] tricks.
2 [illegible] BD [illegible] 1 trick + 4 honours, BD win single.

[illegible] BD [illegible] (1 point) with 2 tricks only gained against AC's 19.

[illegible]

[illegible] BD [illegible] 2nd and 3rd game win 5 tricks. [illegible] with 10 tricks against AC's 19.

[illegible] BD win (8 points), 19—10—1 [illegible] is very unusual to lose the rubber and [illegible] win points.

[illegible] is not necessary to use many counters [illegible] = 5, [illegible] 6 [illegible] = 7, &c., &c., or any better way.

[illegible] to win the rubber and yet lose points, but this [illegible] to win the rubber with 2 tricks against 19, [illegible] 10 [illegible] 19. Dicky.

Dear [illegible] a letter from S. B., asking how "The Great [illegible] at Double Dummy," is to be solved. Will you give the problem next week? I thought two or three years ago I had solved the problem but I cannot at the moment recall how it is to be solved, if [illegible] fourth round, Y discards a Heart. For, then, [illegible] leads Heart, and B (dummy), after taking four tricks in Hearts, leads the long Heart, how is A to play? If he play small Diamond, [illegible] throws a Diamond and makes his King of Spades in the last [illegible]; if A play Spade Queen, Y throws a spade and makes his [illegible] Diamond in the last round. Of course it is all plain sailing [illegible] at fourth round, discards either a Diamond or a Spade. [illegible] this is S. B.'s difficulty, as he says he can see how twelve [illegible] but not the thirteenth. Editor.

PROBLEM IV.

[illegible] at Double Dummy, as given by Clay. [illegible] is to make every trick.

[illegible] The Hands. [illegible]

[illegible] K, Q, 3.
[illegible] 2.
Spades [illegible] A, Q.
Diamonds [illegible] A [illegible] 7 [illegible] 3.

Clubs—8, 6, 5.
Hearts—10, 9, 7, 6, 5.
Spades—K, 6.
Diamonds—Kn, 10, 8.

B

Y [illegible] Z

A

Z.
Clubs—Kn, 10, 9.
Hearts—8, 4.
Spades—9, 8, 7, 5, 4, 2.
Diamonds—K, 9.

[illegible] 7, 4, 2.
Hearts—A, K, Q, Kn.
Spades—Kn, 10, 3.
Diamonds—5, 2.

W. P. The [illegible] is used by Cavendish for the two cards above and below [illegible] already played. Thus, if ten of Clubs [illegible] second player has nine and King of Clubs, he [illegible] to [illegible] Four to the [illegible] the ten.

[illegible] Under what circumstances would you play what [illegible] the "intermediate call"? For instance, your partner [illegible] King [illegible] Queen of a suit, in which you hold nine, [illegible] and two, and you recommend throwing the nine and three [illegible] of the three and two in signalling. But might not that [illegible] in doubt whether one of the adversaries was not signalling [illegible] suppose the cards to fall thus, King, Ace, nine, seven, [illegible] your partner again gets the lead that the next round in [illegible] thus Queen, four, three, eight. Your partner sees [illegible] that you have signalled. But the two has not fallen and he [illegible] hold it himself, nor, obviously, does fourth player. Would [illegible] not naturally infer that second player held the two and had commenced a signal (though he had already had a lead, and had not [illegible] trumps)? It happens at times that a double signal is given [illegible] this way. How is your partner to know it has not happened [illegible] such a case as I have suggested?

Lord Lytton as a Whist Player.—Lord Lytton was very fond of whist, and he and I both belonged to the well-known Portland Club, in which were to be found many of the celebrated players of the day. He never showed the slightest disposition of a gambler. He played the game well, and without excitement or temper, and apparently his whole attention was concentrated upon it; but it was curious to see that at every interval that occurred in the rubbers he would rush off to a writing-table, and with equally concentrated attention proceed with some literary work, until called again to take his place at the whist-table. There was a member of the club, a very harmless, inoffensive man, of the name of Townend, for whom Lord Lytton entertained a mortal antipathy, and would never play whist whilst that gentleman was in the room. He firmly believed that he brought him bad luck. I was witness to what must be termed an odd coincidence. One afternoon, when Lord Lytton was playing, and had enjoyed an uninterrupted run of luck, it suddenly turned, upon which he exclaimed, "I am sure that Mr. Townend has come into the club." Some three minutes after, just time enough to ascend the stairs, in walked this unlucky personage. Lord Lytton, as soon as the rubber was over, left the table and did not renew the play.—From Sergeant Ballantine's "Experiences of a Barrister's Life."

Contents of Knowledge *No. 22.*

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PLAINLY WORDED—EXACTLY DESCRIBED

LONDON: FRIDAY, APRIL 14, 1882.

CONTENTS OF NO. 24.

⁂ Our Exchange and Sixpenny Sale Columns appear on Page VII.—in our advertising columns this week.

THE GLORIES OF THE STAR-LIT HEAVENS.

By R. A. PROCTOR.

ON a dark, clear night,
When all the stars shine,
And the immeasurable heavens
Break open to their highest,

the glories of the stellar depths seem revealed in their fullest splendour. Yet how small a portion is seen. "These are but a part of God's ways; they utter but a whisper of His Glory." If the eye could gain gradually in light-gathering power, until it attained something like the range of the great gauging telescopes of the Herschels, how utterly would what we see now seem lost in the inconceivable glories thus gradually unfolded. Even the revelations of the telescope, save as they appeal to the mind's eye, would be as nothing to the splendid scene revealed, when within the spaces which now show black between the familiar stars of our constellations, thousands of brilliant orbs would be revealed. The milky luminosity of the Galaxy would be seen aglow with millions of suns, its richer portions blazing so resplendently that no eye could bear to gaze long upon the wondrous display. But with every increase of power more and more myriads of stars would break into view, until at last the scene would be unbearable in its splendour. The eye would seek for darkness as for rest. The mind would ask for a scene less oppressive in the magnificence of its inner meaning; for even as seen, wonderful though the display would be, the glorious scene would scarce express the millionth part of its real nature, as recognised by a mind conscious that each point of light was a sun like ours, each sun the centre of a scheme of worlds such as that globe on which we "live and move and have our being."

Who shall pretend to picture a scene so glorious? If the electric light could be applied to illumine fifty million lamps over the surface of a black domed vault, and those lamps were here gathered in rich clustering groups, there strewn more sparsely, after the way in which the stars are spread over the vault of heaven, something like the grandeur of the scene which we have imagined would be realised—but no human hands could ever produce such an exhibition of celestial imagery. As for maps, it is obviously impossible by any maps which could be drawn, no matter what their scale or plan, to present anything even approaching to a correct picture of the heavenly host. There is no way even of showing their numerical wealth in a single picture.

Take, for example, the chart of 324,198 stars which I drew eight years ago, of which a portion is roughly presented in the illustrative map. Here the points and discs representing stars congregate together so closely, in places, that there is no room for more to be shown, and those which are shown are but inadequately presented. Yet what does this chart show of the heavenly host, regarded merely in their numerical aspect? The stars here charted are only those which can be seen, or rather only those which have been seen, with a telescope $2\frac{3}{4}$ inches in aperture, such a telescope as can be seen in every optician's window. I say only those which *have* been seen, because I know from my own observations that Argelander and his assistant observers, had they cared to turn their telescope to the heavens only on the darkest and clearest nights, could certainly have seen and charted half-a-million stars at least, instead of the 324,000 which they have actually included in their survey of the Northern heavens. In the Southern heavens at least as many could be seen. A million stars within the range of a telescope absolutely insignificant compared with the gauging telescopes of the Herschels, which again in light gathering power were feeble compared with the mighty Parsonstown reflector!

Utterly hopeless would it be to attempt to delineate the stellar host within the range of these noble instruments, when already we see the method of charting fail us for the work of the puny tube which Argelander employed. Yet how impressive is the scene roughly depicted in our chart! Each one of the points there shown represents a sun, and tells us therefore of a solar system, of a system in which such a globe as this earth would be but as a point, and regions exceeding in extent the mightiest kingdoms over which the monarchs of earth have ruled would be utterly as nothing.

When we pass onward from these glories to the vaster glories revealed by more powerful telescopes, we seem to lose ourselves in the contemplation of the mysteries of infinite space, infinite power, infinite wisdom. Yet it is not till we have learned to look on all that the telescope reveals as in its turn *nothing* compared with the real universe, that we have rightly learned the lessons which the heavens teach, so far, at least, as it lies within our feeble powers to study the awful teaching of the stars. The range of the puny instruments man can fashion is no measure, we may be well assured, of the universe as it is. The domain of telescopically visible space, compared with which the whole range of the visible universe of stars seems but a point, can be in turn but as a point compared with those infinite realms of star-strewn space which lie on every side of our universe, beyond the range,—millions of times farther than the extremest scope,—of the instruments by which man has extended the powers of visions given to him by the Almighty. The finite—for after all, infinite though it seems to us, the region of space through which we can extend our survey is but finite—can never bear any proportion to the infinite save that of infinite disproportion. All that we can see is as nothing compared with that which is; all we can know is as nothing: though our know-

ledge "grows from more to more," seemingly without limit. In time, we may say (as our gradually widening vision shows us the nothingness of what we have seen, of what we see, of what we can ever see), not, as Laplace said, *The Known is Little*, but The Known is Nothing; not *The Unknown is Immense*, but The Unknown is Infinite.

THE BEETLE'S VIEW OF LIFE.

By Grant Allen.

HERE on a yellow crocus, buried deep in the beautiful golden cup, I have found a little bronze-mailed beetle stealing the pollen as hard as he can eat it, and hugely enjoying his plentiful morning feast. I have picked him carefully out with a little bit of stick, and I have got him here now crawling about suspiciously upon my hand, and trying to find out what is the best way down from that unpleasantly warm and dangerously mobile valley, the hollow of my palm. I often wish I could discover how the world looks to that small creature here; and, perhaps, the question is not quite so unanswerable as it appears at first sight. When one remembers that brain and nervous system are on the whole a good index of mind, and that feelings and ideas depend upon the arrangement of the various sense-organs and their connected central parts, it will be clear that, after all, we may make a fair guess at what is passing in this little beetle's head, especially since his notions about things generally must in all probability be a good deal simpler and more directly dependent upon his sensations than our own.

Now, what, in the first place, are the beetle's senses? He can see, that we all know; and his sight is on the whole a good deal like our own. His eye can discriminate form, and that accurately, for in all flying creatures this sense is necessarily highly developed; it has been evolved and perfected side by side with their wings, or else they could never have learned to fly at all. They can doubtless distinguish colour, too; for we know positively from Sir John Lubbock's experiments that this is the case with bees, and there are good grounds for believing that the same thing is true of all flower-feeding insects as well, since all alike seem to be guided to the flowers by their brilliant hues. Sir John put drops of honey on slips of glass above bits of coloured paper; and when he had once taught a bee to feed from one slip, say the blue, he found that it would return straight to that slip, even when the relative places of the colours had been transposed. Now, almost all flowers which contain honey have also bright petals; and Mr. Darwin has shown that both honey and petals have been developed by the flowers for the sake of attracting insects, which carry their pollen from head to head, and so fertilise and impregnate the seeds. Moreover, the colours of the petals differ in different species, according to the kind of insects which they each wish to attract. Thus bee-flowers are usually blue or red; and Sir John Lubbock has proved that bees show a distinct preference for these colours, while beetle-flowers are often yellow, and small fly-flowers are generally white. Such facts, and others like them, show that the beetle has sensations of sight essentially identical with our own, and also that he has certain special tastes for certain special hues and blossoms.

It is much the same with the other senses. The beetle certainly hears sounds; and his hearing appears to be analogous to our own; for though he himself is not musical, yet many other insects are; and these produce special notes and melodies to charm the ladies of their kind. He can also taste, and is fond of sweet things, like most other animals, for the flowers which seek to allure him lay by a drop of honey for his use; and this liking for sugary juices is shared by almost all insects, from the flies which crowd around a barrel of treacle at a grocer's door, to the ants which suck the honey-dew from the little green aphides that they keep as we keep cows. Last of all, he can smell, for the flowers which depend on him for fertilization are usually perfumed, and both beetles and other insects are often attracted by scent, as all collectors well know; indeed, they frequently catch rare insects by enclosing one of their mates in a box, when the quick-scented and eager lovers soon sail up from leeward, evidently attracted by the distinguishing odour borne upon the breeze. Indeed, some butterflies have special scent-glands among the feathery scales on their wings, to make them more charming to their pretty spouses, just as so many of the higher animals have a peculiar musky perfume. I may mention that Mr. Darwin similarly sets down the brilliant colours and ornamental spots of butterflies, as well as the curious horns and excrescences of many beetles, to the long selective action of their fair lady-loves, who always choose the handsomest and strongest among their numerous rival suitors. It is to this same cause that we probably owe the bright iridescent hues and bossy headpiece of the little creature who has now just escaped from my hand by clumsily transferring himself to yonder tall blade of rank meadow-grass.

Thus, as far as his outward picture of the world goes, the beetle's ideas must really be very similar to our own. The universe of sights, sounds, smells, tastes, and touches through which he moves must present the same general effect as that which we ourselves experience in our intercourse with outer things. But when we come to consider the relations which the beetle establishes between these primordial sense-impressions, the little ideas and emotions which he elaborates out of them, we find signs that the difference is vast indeed. Though the material is the same, the product is as unlike as the letters of the alphabet are to the "Iliad" or "Paradise Lost." The elements of human thought are there, but the organising and co-ordinating power is wanting.

If you were to cut open the beetle's head, you would find in it a small knot or lump of nervous matter, roughly answering to our own brains. To this lump the various sense-organs send up bundles of nerves; and in it the impressions derived from the different senses are compared and arranged, so as to produce the common impulse upon which the beetle acts. But the size of this nervous knot is vastly smaller in proportion to the insect than the human brain is to the body of a man. Our brain consists of numberless cells, arranged and united in definite subordination to one another, and so disposed that every part of our nervous mechanism can be brought into relation with every other; while in many cases we are not concerned in our mental operations with actual sense-impressions at all, or even with memories of such impressions combined into the shape of ideal objects, but with wholly abstract conceptions, elaborated out of them by the action of the brain itself in its higher parts. The beetle, however, can do nothing analogous to this. Its mental life is wholly made up of direct impressions, and actions immediately dependent upon them. Memories it doubtless possesses in a slight degree, especially in the form of mere recognitions; but it is not probable that it can think of an object in its absence, or voluntarily recall it; while it certainly cannot reflect as we can about abstract ideas, or even about things which do not concern its immediately present needs and actions.

Indeed, the whole nervous system of the beetle is so loosely bound together—so little co-ordinated, as Mr.

Herbert Spencer puts it—that it can hardly be said to possess any distinct voluntary capacity, or any strongly-marked personality at all. In the case of man and the other higher animals, almost the whole nervous system is bound up with the brain, sending messages up to it, and receiving orders from it in return, so that a single great nervous centre governs all our movements, and ensures that uniformity of action without which the complicated activities of human life would be impossible. The only nerves (worth mentioning) in the human body which are not thus under the control of the brain, are those of the heart and other internal organs; and over these parts, as everybody knows, we have not any voluntary power. But all our limbs and muscles are moved in accordance with impulses sent down from the brain, so that, for example, when I have made up my mind to send a telegram to a friend, my legs take me duly to the telegraph office, my hand writes the proper message, and my tongue undertakes the necessary arrangements with the clerk. But in the insect's body there is no such regular subordination of all the parts composing the nervous system to a single central organ or head-office. The largest knot of nerve-matter, it is true, is generally to be found in the neighbourhood of the sense-organs, and it receives direct nerve-bundles from the eyes, antennæ, mouth, and other chief adjacent parts; but the wings and legs are moved by separate knots of nerve-cells, connected by a sort of spinal cord with the head, but capable of acting quite independently on their own account. Thus, if we cut off a wasp's head and stick it on a needle in front of some sugar and water, the mouth will greedily begin to eat the sweet syrup, apparently unconscious of the fact that it has lost its stomach, and that the food is quietly dropping out of the gullet at the other end as fast as it is swallowed. So, too, if we decapitate that queer Mediterranean insect, the Praying Mantis, the headless body will still stand catching flies with its outstretched arms, and fumbling about for its mouth when it has caught one, evidently much surprised to find that its head is unaccountably missing. In fact, whatever may be the case with man, the insect, at least, is really a conscious automaton. It sees or smells food, and it is at once impelled by its nervous constitution to eat it. It receives a sense-impression from the bright hue of a flower, and it is irresistibly attracted towards it, as the moth is to the candle. It has no power of deliberation, no ability even to move its own limbs in unaccustomed manners. Its whole life is governed for it by its fixed nervous constitution, and by the stimulations it receives from outside. And so, though the world probably appears much the same to the beetle as to us, the nature of its life is very different. It acts like a piece of clock-work mechanism, wound up to perform a certain number of fixed movements, and incapable of ever going beyond the narrow circle for which it is designed.

MR. MUYBRIDGE AND ROWING.

By the Editor.

The method which has been applied so successfully to determine all the successive stages of a horse's motion in galloping at the rate of a mile in less than two minutes, can be much more easily applied to determine all the successive stages of an oarsman or sculler's action—for even the swiftest racing-boat does not travel faster than a mile in five minutes on still water, and by taking her against stream the problem would be rendered even easier. If rowing men of the Oxford, Cambridge, and the various Thames clubs would invite Mr. Muybridge to this congenial work, the principles of good rowing style and the secret of successful oarsmanship could very readily be determined. Hanlan might, I have no doubt, be persuaded to row past the twelve cameras, and so hand down to posterity the perfection of his marvellous style. Mr. Muybridge would be willing, I know, to do his part—his expenses being guaranteed, of course. I do not know what the expenses would be, but to judge from all that I have learned, they would lie somewhere between two and three hundred pounds, for a week's experiments, with all necessary assistance in arranging for the sets of twelve instantaneous pictures. He would willingly supply contributors with copies (free of further expense) for study and comment. The value of such views at the present time, and their interest hereafter, when, perhaps, new methods of rowing may have come into vogue, and when, at any rate, oarsmen will like well to know how their predecessors rowed, can hardly be over-estimated. If the presidents of Oxford, Cambridge, London, and other clubs, care to see anything done in this direction, arrangements could be quickly made (at present Mr. Muybridge's apparatus is at New York, but it could be here in a fortnight), and the expenses readily subscribed. I should be very willing, for my own part, to put my name down for £50, if that sum would bring the amount up to the required total. If, however, there were good prospect of the amount being readily made up otherwise, I would content myself with such a subscription (five or ten guineas) as hundreds of boating men would, I am sure, be glad to offer for so invaluable a contribution to the scientific investigation of oarsmanship.

American Agriculture v. English.—Mr. Fowler, M.P., for Cambridge, recently gave to a Chicago *Tribune* reporter his impressions of the agricultural resources of the West, where he has spent some time in making observations. "What has interested me most," said he, "is the matter of transportation to England, in connection with the cost of production there, and the question is whether we can continue much longer to compete with America in the raising of wheat, or even to raise it at all and make it pay. The natural protection to English production, by reason of the cost of carriage, must be—nay, is—rapidly diminishing, and I rather expect, if we were to have a good harvest in Europe and America at the same time, you would have prices such as we have never expected." "The American farmer is producing and transporting wheat and corn so cheaply, then, that his English brother cannot compete with him?" "It is a good deal as a gentleman expressed it to me the other day, when he said: 'A man out here in Iowa is competing with the English farmer just as if he lived in Yorkshire.' That may be a strong way of putting it, but you must observe the great advantages which the American farmer has over the farmer on the other side. Iowa land, for instance, costs \$10 an acre, while in England it costs £50, £70, or £80 an acre, so that the Englishman is terribly handicapped at the start, for he has to pay interest on £50 to £70, while the Iowa man pays interest only on £2. Then, in addition to all that, the Iowa man has a better soil and a better climate. In short, with these advantages in favour of the American farmer, with the cost of transportation minimized as it is, so that our natural protection from that cause is rapidly diminishing, I have great doubts whether the cultivation of wheat will pay in England at all. I speak not so much of the present as of the future, for our crop this year has been a good one, while yours seems to have been just the other way. Your deficiency this year, as I have seen it stated, is 80,000,000 bushels—nearly as large as England's entire production in an ordinary season. But here is your vast expanse of territory developing every year. Then, again, you virtually raise wheat in this country by machinery. The extent of your wheat-raising territory is simply astounding, but your population, while large in the aggregate, is spread over these vast expanses, and your real market is elsewhere—across the water, over in England, where we find a contrary state of affairs: a comparatively small wheat-raising area, with millions of people to be fed. And I don't begrudge you your good fortune in the least. Your prosperity is ours, for, unless our people be cheaply fed, they cannot afford to work for reasonable wages, and unless we can manufacture at reasonable cost, we can no longer hope to supply the world with our manufactured products."

COLLISIONS AT SEA.

By the Editor.

It has been no unusual circumstance for two ships in open daylight, and in calm, clear weather, to be brought into collision through what landsmen might regard at first sight as absurd blundering, though seamen know that the indications which should have guided the manœuvres of either ship have been quite naturally misunderstood. In point of fact, the risks of collision at sea may be divided at present into two broad classes—the avoidable risks, and those which are unavoidable. Of the latter class of collisions it would be useless to speak; but of the former there is much to be said, and for their prevention much might well be done.

The first and most striking circumstance in the history of all such collisions as might in reality have been avoided, is the utter absence of any proper means of communication. Flag signalling is, of course, very often a ready and convenient method; but under certain conditions, of not unfrequent occurrence, it fails either wholly or in part. The wind may be insufficient to display the small signal flags, or may be so light and variable that they are not quickly or readily discerned. Again, the wind may blow in such a direction that the flags, though well displayed to viewers in other directions, are invisible to those for whom they are intended. Then anyone who examines the flags used in signalling will note that, although when the whole of each flag is squarely shown, one cannot possibly be mistaken for another, this is by no means the case when the flags are exhibited at some distance, in light winds, and under varying atmospheric conditions.

I have before me as I write pictures of a set of signals devised by Mr. A. Stewart Harrison, in which these objections are entirely obviated. In the first place, all the signals are of the same kind, and formed in a similar manner. In each signal there are two vertical rows of bright discs on a black ground, which may be set facing in any direction. (The discs at night can be illuminated either by reflected or transmitted light, as may be most convenient.) Each row may show any number of discs from 0 to 5, and as the eye can in a moment tell whether one, two, three, four, or five balls are exhibited in each row, any combination is at once recognised, and can be immediately replaced by the next, and the next, and so on, till the necessary message has been spelled out. Nothing could possibly be simpler or less likely to be misunderstood than this method of signalling. The discs could be discerned with the naked eye at a considerable distance in ordinary weather; and could be read off at two or three miles distance, in clear weather, with a telescope. The actual distance at which two discs of given dimensions could be separately discerned with a telescope is known already to astronomers from the experiments which have been made by Dawes, O. Struve, and others on artificial double stars; and it can readily be shown that with such dimensions as Mr. Harrison proposes for the discs, the range of distance above mentioned would be well within the powers of this method of signalling. It is not too much to say that if this method were adopted, a large proportion of the collisions which now occur would be rendered impossible, or possible only through utter negligence on the part of all concerned in directing the course of the two ships.

It is singular, however, that even those whose safety depends on the use of such methods seem unready to adopt any improvement in signalling at sea. After a method has been shown by experience to be quite inadequate, reliance continues to be placed upon it as though it had never failed. As an instance in point, consider the ships lights now in use. I remember, fourteen years or so ago, writing an article for the *Daily News* on a lamentable collision which had then recently occurred in consequence of the want of any means of recognising, in good time, a change in the course of one or other of two approaching ships. Nothing could, in a scientific sense, be less suited to the requirements of the case than the actual arrangement of a ship's lights. That a light on the port side should be of one colour, and a light on the starboard side of another, is well so far as it goes. But it does not tell another ship much. The approaching ship might change her course considerably, and yet show the same two colours, not greatly changed in apparent position. If only two lights are to be shown, these might be much more effectively placed than as at present. Suppose, for instance, a red light were carried in some well-chosen position amidships, and a green light near the bows, and at a lower level, both being so placed that they could be seen well from either the port or starboard side, or in front. Then an observer on an approaching ship would know on what course a ship carrying these lights was steering. If the green light were seen to the right, he would know that he was on her starboard beam; if to the left, that he was on her port beam. If the red light were directly above the green, he would know that she was bearing full upon him. If she changed her course, the two lights would be brought either nearer together or further apart. At present there is absolutely no indication of another ship's position (on a dark night when only her lights can be seen), unless she is steering on such a course that no precautions are required to avoid her. That is to say, if a ship, still at a moderate distance, is sailing in a course which will not bring her at all near another, an observer on this other ship will know what that course is, for he will see either a red light only or a green light only, according as her course is (in non-nautical terms) from right to left or from left to right. But if she is bearing nearly towards his ship, he will see both her lights, and nearly in the same relative position, whether her course would carry her past his ship (if it were at rest) on one side or on the other side. He has no means of knowing, therefore, to which side he should direct his course. There are the rules of the road at sea, of course, and if these were always, or could always be, strictly followed, the present arrangement of a ship's lights would serve well enough. But it is only necessary to read the reports of cases in our Admiralty Courts to learn that instances frequently occur where the rules of the road cannot be—or, at any rate, are not—followed. Even in broad daylight, and in clear weather, collisions have occurred when the vessels have been cognisant of each other's actual course and changes of course for a distance of two miles before collision actually took place, the observed manœuvres having been simply misinterpreted. How largely must the risk of collision be increased at night, in hazy weather, or under conditions otherwise unfavourable? Yet nearly all that is wanting to prevent such collisions absolutely, and all that is wanting to render them infrequent, is an arrangement by which not only the course, and any change of course, of each of two approaching ships may be quickly made known to the other, but also (as in Mr. Stewart Harrison's ingenious arrangement) by which each ship may quickly convey directions or warnings to the other. It is the interest of all who travel by sea, and the duty of all who care for the safety of our seamen, to urge that the simple measures required for preventing avoidable collisions should as soon as possible be carried out.

NIGHTS WITH A THREE-INCH TELESCOPE.

By "A Fellow of the Royal Astronomical Society."

BEFORE beginning our examination of the Constellation Virgo to-night, we will return to that of Hydra for the purpose of looking at a very wonderful object, omitted in our description on p. 376. The student will find it by fishing with a power of 100 or so about 2° (four diameters of the moon) to the south of μ Hydræ (map p. 474). It is No. 27 of Herschel's IVth Catalogue, and is one of the most remarkable planetary nebulæ in the heavens. Unlike nebulæ generally, this will bear considerable magnifying power. It will be seen as a pale-blue disc, looking just like the ghost of Jupiter. As Huggins has shown that it is gaseous, the sharpness of its outline is very curious.

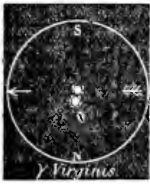


Fig. 26.

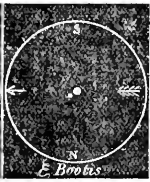


Fig. *a*.

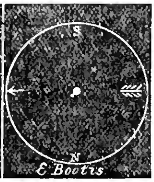


Fig. *b*.

Turning now to Virgo, we will begin with that most interesting star γ, which is shown in Fig. 26, as seen with a power of 160. When first observed by Herschel, in 1790, the components of this star were nearly 6″ apart, but were approaching each other; and in 1836 were so practically superposed as to appear single under the very highest power that Admiral Smyth could apply to them upon his 5·9 inch achromatic. Since that time they have been separating, and their distance at present amounts to about 5″, so that they form an easy pair in the instrument we are using. θ Virginis (map, p. 474) is a very pretty and interesting triple; the third star, which is nine times as far from the large one as its more obvious companion, will require a dark night and pretty sharp sight to see it well. There are very many beautiful and interesting pairs of stars in Virgo; but as they are mostly below the sixth magnitude they are not marked in the monthly maps in Knowledge, and no amount of description would enable the reader to identify them. Fortunately, simple sweeping in the marvellous region to which we are about to introduce the reader, will suffice to enable him to pick up many of the wonderful mass of nebulæ collected within the area roughly bounded by ϵ, δ, γ, η, and β Virginis, and β Leonis. If the student will arm his instrument with a power of about 40, and sweep slowly over that part of the sky contained within the curve drawn through the stars we have named (map p. 474), he cannot fail to be astonished and pleased at the wealth of nebulous objects, and the pretty fields of stars that he will encounter. One of these curious objects is shaped like a boy's kite. A few are resolvable into stars in some of the enormous telescopes now comparatively common. Others are unmistakably gaseous.

Above Virgo is situated Coma Berenices, easily recognisable in the sky by the coarse cluster of stars in its north-western portion. If the reader will draw an imaginary line from α through 36 on the map (p. 474), then at about three times as far to the right of 36 as 36 is to the right of α, and a little above such line, will be found 24 Comæ, a wide double star, but interesting from the beautiful contrast of orange and pale-purple presented by its components. Just above, and to the left of α Comæ (map p. 474), what will appear like a nebula will be found. It is 53 of Messier's Catalogue, and is really an immense mass of tiny stars; but it requires a much more powerful instrument than ours to show this. Other nebulæ, mostly faint, will be found among the cluster of stars to which we have previously referred.

Adjoining Coma Berenices above lies Canes Venatici, of which the chief star α, 12, or Cor Caroli—for it has all three designations—is a widish double. About one-third of the way between Cor Caroli and δ Leonis 2 Canum will be found; a close pair, with prettily contrasted colours. There are numerous other pairs in this constellation, but, for the so often reiterated reason, we can give no intelligible directions for finding them. In the case of more than one of the remarkable nebulæ, however, contained in it, we trust to be more successful in pointing out their whereabouts. 3° (6 diameters of the moon) to the south-west of γ Ursæ Majoris, the star at the end of the Great Bear's tail, will be found two rather dim nebulæ, nearly touching each other. This is Messier 51, the astonishing Spiral nebula, which, as seen in Lord Rosse's great telescope, has been pictured in so many works on astronomy. About midway between Arcturus and Cor Caroli, but rather nearer the former (map p. 474), will be found a bright nebula, Messier 3, which large telescopes resolve into a brilliant condensed cluster of minute stars. Some $2\frac{1}{2}°$ to the north-west of Cor Caroli, is a nebula, 94 Messier, which, though small, is sufficiently conspicuous in the class of instrument we are using. Other nebulæ in this constellation may be picked up by fishing, especially in the region between α Canum Venaticorum and ξ Ursæ Majoris.

[As but one figure came from our esteemed contributor, F.R.A.S., this week, we have ventured to add two doubles, viz., ξ Boötis, fig. *a*, and ϵ Bootis, fig. *b*. Both stars are shown in our monthly map, No. 22, for March 31.—Ed.]

THE AMATEUR ELECTRICIAN.

ELECTRICAL GENERATORS.

THE task we have set ourselves in this series of papers is not by any means an easy one. Our object is to help, by practical and easily-followed-out instructions, those who desire to become experimenters in this most interesting department of physical science. There is no "Royal road" to a thorough knowledge of the laws of electricity, any more than of any other science, and our readers must, therefore, please understand that while every effort will be made to render each branch of the subject dealt with as complete as possible, a "text-book" on electricity is beyond our scope. It is anticipated that we shall soon be at liberty to initiate a series of papers on the rudimentary demonstrations of electrical properties, but here the assumption must be allowed, that our readers know just a little of the science.

The present production of large quantities of electricity—larger than were ever conceived to be possible—necessitates some agency other than the galvanic battery. Indeed, the battery could never have yielded so bountiful a supply of electric force as is now being utilised in very many places—notably the Crystal Palace Electrical Exhibition. Why this is we will not attempt to explain now, although it may be found advisable to do so hereafter. It is not difficult to conceive that one of the features in the

galvanic battery is the transference or *motion* of particles of matter from one state of combination to another. Thus, in the Daniell cell, the sulphate of copper is converted into sulphuric acid by the means of hydrogen, which displaces the copper, the sulphuric acid being likewise converted into sulphate of zinc by particles or atoms of zinc taking the place of the hydrogen.

There are, however, other forms of motion which in their turn are capable of generating currents of electricity, motion not necessarily of minute particles, but of large masses of matter. If one of two masses moves or passes before the other, more particularly if magnetism or electricity is present in one of them, an electric current is generated. Let us imagine, in the first place, that we have a hollow helix or coil of copper wire, such as that contained on the leg of an electro-magnet, or such as we should get by carefully winding an insulated copper wire round a thin stick of wood. Now, suppose the two ends of the wire are connected to a galvanometer. Then on plunging a steel magnet into the coil, the needle of the galvanometer will be deflected, say, to the right, and will indicate the passage of an electric current. When the magnet is withdrawn, the needle will be again deflected, this time, however, to the other side, thereby indicating the passage of a current in the opposite direction to the previous one. Let us pursue this a step further. Our readers are doubtless aware that when a piece of soft iron is placed in the vicinity of a magnet, magnetism is immediately induced in it. If, then, we place a core of soft iron in our coil of wire, and bring a magnet near it, it becomes at once magnetised. The core being already inside the coil, magnetising it in that position is equivalent to plunging in a magnet *instantaneously*. A current of electricity is consequently produced, but, of course, of greater strength than when the magnet is plunged into the coil. Similarly, on removing the inducing magnet, the iron core becomes at once *demagnetised*. This is identical to withdrawing the magnet, and therefore a strong current is generated or induced, travelling in the *opposite direction* to the preceding one.

What have we done? We *moved* a magnet, and obtained a current of electricity. To a certain degree, the current produced is a measure of the force expended in moving the magnet, and it is interesting to notice that when the coil circuit is complete, it is more difficult to move the magnet than when the circuit is broken. This phenomenon will, however, be referred to more fully on a future occasion. The experiments above described illustrate the principles of what is known as the magneto-electric machine, such as those used for medical purposes, in which a coil of wire and its core of soft iron are made to approach and recede from a permanent magnet, which is fixed. We shall, in our next, give instructions for making a small machine, not one of those used by the medical faculty, but one of a much improved form, which will be capable of doing all that a galvanic battery will do.

The apparatus for performing the experiments described in this paper may be very cheaply constructed, and our readers are strongly recommended to try them, in order that they may better understand our subsequent papers on the subject. The coil may be made by winding five or six layers of No. 22 cotton-covered wire* on a paper tube, about half an inch in diameter, and three inches long. After the wire is wound on the tube, it would be as well to coat it with a little shellac varnish, made by dissolving shellac in methylated spirit. The magnet may be of the ordinary round form, such as are used in telephones, and should fit the tube easily. A piece of soft iron of similar dimensions will complete this portion of the apparatus. The galvanometer may also be easily made. Magnetise an ordinary sewing-needle by rubbing it a few times in one direction over one pole of the steel magnet, and then suspend it by a thin thread (or, better still, by a fibre of unspun silk), in an oblong coil, consisting of about ten turns of the No. 22 wire, just long enough to allow the needle to rotate freely.

* Messrs. Rickard, of Derby, are wire brokers of the highest repute in electrical circles. Their price for No. 22 is 1s. 10½d. per [illegible]

THE THREE COLD DAYS OF APRIL.

By the Editor.

FEW weather phenomena in this country are more remarkable, and seem at present less easily explained, than the so-called "borrowing days," as they are called, between the 10th and 14th of April, when usually the temperature falls considerably below that due to the time of year. The cold at this time is, at any rate, sufficiently marked—first, to have attracted long since general attention; and, secondly, to affect in a very obvious manner the average temperature for these days during the last eighty years. We find these three cold days of April, which before the change of style were the first three days of the month, thus described in doggrel lines in the north of England:—

> "March borrows from April
> Three days, and they are ill:
> The first of them is wan and weet,
> The second it is snaw and sleet.
> The third of them is a peel-a-bane,
> And freezes the wee bird's neb tae stane."

The following lines are given in the "Glossary of Scotch Words and Phrases":—

> "Said March to April,
> Gie me three hoggs upon yon hill;
> And in the space of days three
> I'll find a way to gar them die.
> The first a bitter blast did blaw,
> The second it was sleet and snaw,
> The third it cam sae fell a freeze
> The birds' nebs they stack to the trees;
> But when the days were past and gane
> The three pair hoggs cam hirplin hame."

This is manifestly an imperfect version of the lines in the poem called the "Complaynt of Scotland," where the reference to the borrowing of three April days is much clearer (in the above account March borrows hogs not days):—

> March said to Aperill
> I see three hogs upon a hill:
> But lend your first three days to me,
> And I'll be bound to gar them dee.
> The first it shall be wind and weet.
> The next it shall be snaw and sleet.
> The third it shall be sic a freeze,
> Shall gar the birds stick to the trees.
> But when the borrowed days were gane,
> The three silly hogs cam hirplin hame.

This is, I believe, the oldest version of the doggrel. It belongs to a time when the three cold days of April really were the first three days of April. The other was perhaps modified to correspond with the new style, according to which the cold days fall in the heart of the month, and cannot be very well imagined to be borrowed by March. It is worthy of observation how correctly common observation has indicated the true position of these cold days; for in the temperature curve derived from three quarters of a century of accurate observation at Greenwich, the depression corresponds exactly with the days which before the change of style were the 1st, 2nd, and 3rd of April.

THE ECLIPSE OF MAY 17.

WE propose to give next week a small map showing the course of the moon's shadow during this eclipse. The editor has appointed Mr. Richard A. Proctor his special correspondent in Egypt on this occasion; but whether Mr. Proctor's numerous engagements will allow him to accept the appointment is not as yet known.

THE NEW MOON IN APRIL.

THIS is the way in which our usually calm and reflective contemporary, the *Scientific American*, tells us that the moon will be in conjunction with four planets on the 18th and 19th inst.:—

The new moon of the 17th commences her course with a brilliant record. On the 18th, the day after her change, she pays her respects to three planets—Venus, Saturn, and Neptune—on the same evening. It is difficult to see the moon when a day old, for the crescent is but a slender thread, still it can be done. If the evening be exceptionally clear, the keen-eyed observer may behold the lovely picture, the moon passing about two degrees north of Venus and three degrees and a half north of Saturn. But the loveliest exhibition of the month will occur on the 19th, when the two days' old crescent will be in conjunction with Jupiter, and only forty minutes north of him. As the moon does not set until after 9 o'clock, there will be ample opportunity for seeing the show, if the clouds are kind.

VENUS IN APRIL, 1882.

THE *Scientific American* has, indeed, suddenly become quite poetic over the planets. Here, for instance, is its account of Venus in April, 1882:—"Venus is evening star, and the only one among the planets whose movements excite a marked interest during the month. She has now advanced far enough in her eastern course to be seen in the west soon after sunset, and to allow the observer to obtain a glimpse of the beauty to be revealed during her nearly ten months' course as evening star. She will soon be the brightest in radiance, the largest in size, the softest in colour, of the myriad golden points that glitter in the celestial archway. Neither is she to be considered alone in an æsthetic light. The Queen of the Stars has unwittingly a mission to perform, when, closing her career as evening star in December with the grand event of the transit, she furnishes the inhabitants of the planet that shines so brightly in her sky, one means for measuring the unapproachable, the much-desired solution of the problem—the earth's distance from the sun."

THE NEW COMET.

THIS comet, called "Comet Wells," is now passing from near Vega (Alpha Lyræ) into Cepheus, where its course will change, in May. We propose to give next week a map showing a portion of its path. The following reaches us from America (*Scientific American*):—

The elements of the orbit of the new comet are: Perihelion passage, June 15; perihelion place, 49 deg. 35 min.; longitude of node, 206 deg. 40 min.; inclination, 74 deg. 17 min.; perihelion distance, 10,000,000 miles; motion direct.

This comet appears to have no analogue in the past, as no comet is known with elements sufficiently resembling these to constitute reasonable belief in identity. The elements of the comet of 1097 somewhat resemble those of the present comet, but the perihelion distance of the former is computed to be seven times as great as that of the latter.

At present the comet is about 160,000,000 miles from the earth, and its distance from us will probably not be less than 80,000,000 at any time, though further calculations will be necessary to settle that point. It may be expected to make a fine display for a few days in the early part of June. The present extraordinary intensity of its light, which comes to us from the enormous distance of 160,000,000 miles, proves that it has plenty of material for future display, and it will probably show a long and nearly straight tail of enormous dimensions to our antipodes. How much it will give us is still problematical.

Professor Maspero is said to have succeeded in making satisfactory terms with the villagers whose dwellings and mosque encumber the temple of Luxor, his only difficulty being with Mustapha Aga, the local British Consul, whose demands are considered exorbitant. The temple is likely to yield results of the highest archæological interest. It was begun by Amenhotep III., carried on by Seti I., Rameses II., Horus, Sabaco, and Alexander Ægus; and the great pylons erected by Rameses II. are sculptured with battle scenes similar to those at Aboo-Simbel, and inscribed with a version of the heroic poem of Pentaur.

The Formation of Coal.—All attempts to explain satisfactorily the formation of coal have thus far proved unsuccessful, though it is generally understood that it is the product of the decomposition of vegetable matter. Just how that decomposition has been brought about chemically is a matter which chemists have not as yet been able to solve. The principal difficulty has been that it has been impossible to obtain a clear insight into the chemical constitution of coal. It has been thought hitherto, and this is still the popular belief, that coal is in the main pure carbon, mixed with varying quantities of bituminous substances. It has been generally believed that, as the product of the distillation of coal is principally carbon, it would be safe to conclude that free carbon actually does exist in coal. The fact that sugar, starch, &c., under similar circumstances leaves a residuum consisting of carbon has never been considered a proof that that element existed in these bodies in a free state. It is well-known that coals which may have the same percentage of carbon, hydrogen, and oxygen, do not by any means, in coking, yield the same products of distillation, and we have a complete analogy for this in the behaviour of cellulose and starch when subjected to distillation. Evidence points to the conclusion that coal is a mixture of many and complex compounds; and the difficulty, amounting almost to an impossibility, of separating these compounds has much to do in rendering a chemical solution of the questions involved in the formation of coal a very arduous task. The production of coal by artificial means is met by great obstacles, among which the absence of all knowledge concerning the conditions under which that process actually took place is the principal one. The question whether the vegetable matter to which our coal veins owe their origin was amassed by drifting, or was carbonised *in situ*, has been much debated, and there has been much discussion on the point whether it was obtained from water or from land plants. Dr. Muck, of Bochum, in a recent work to which we shall refer at greater length in the future, takes up the theory that algæ have mainly contributed to the formation of coal. It is urged that the remains of marine plants are rarely found in coal veins, and that shells, &c., are not often met with. Dr. Muck calls attention to the fact that marine plants decompose easily and completely, losing their form entirely; and that the disappearance of the calcareous remains of mollusks is readily explained by the formation of large quantities of carbonic acid gas during the process of carbonisation. In accepting the marine origin of coal, it is not necessary to resort to the assumption of immense pressure and high temperatures to explain decomposition and the total destruction of the structure of the original substance. Dr. Muck combats Fremy's bog theory at length. His views are well supported by recent investigations made by Herr P. F. Reinsch, who has examined 1,200 sections of coal, coming to the conclusion that that mineral substance has not been formed by the alteration of accumulated land-plants. Herr Reinsch claims to have discovered that coal consists of microscopical organic forms of a lower order of protoplasm; and though he carefully examined the cells and other remains of plants of a higher order, he computed that they have contributed only a fraction of the matter of the coal-veins, however numerous they may be in some instances.—*Scientific American.*

RAPID MOTIONS PHOTOGRAPHED.

TO a people like the English, interested by inherited tendencies to all active exercises, and especially in horsemanship, the experiments made by Mr Muybridge on the movements of galloping horses, running dogs, &c., ought to be full of interest, and his successful mastery of most difficult problems deserves their highest recognition. A few years ago the news that a photographer of San Francisco had succeeded in taking photographs of a galloping horse was received here with incredulity. Now we have the photographer himself among us, and the means of studying his pictures in such sort as to remove all possibility of doubt as to the reality of his triumphs over difficulties which had naturally enough been regarded as insuperable. That a horse rushing along at the rate of a mile in 1¾ minutes, and moving its limbs forward in part of each stride with nearly twice that velocity, should be seized by photographic art so as to show every limb well and clearly delineated, would have seemed wonderful indeed to the earlier professors of that art. Still more amazing is it to find ten or twelve distinct pictures taken during a single stride, the comparison of which enables the most rapid of all equine movements to be analysed as thoroughly as though the horse could be made to go through all the movements of the swiftest gallop at a funereal pace. Then, by combining these together in a much improved kind of zoetrope—the zoopraxiscope—the horse can be made to go through the action of galloping as perfectly as though he were actually galloping before the eyes of the observer. This Mr. Muybridge has done for the galloping and leaping horse, the trotting and walking horse, the dog running, leaping, and in the chase, cattle, wild bull, deer, athletes, gymnasts, and even for birds. Not only the characteristic movements of the different actions, but even those slight and scarce definable peculiarities which distinguish the movements of one athlete from another in performing the same feat, of one horse from another in moving at the same gait, and so forth, are perfectly recognisable in the combination of pictures which, separately seen, simply startle us by the new light which they throw on the real nature of these rapid motions.

Whether figures thus unfamiliar should replace the conventional forms by which artists have tried to represent swiftly moving horses, men, and other animals, is a point about which we apprehend that art and Mr. Muybridge may be some time at issue. But that the utterly incorrect conventional pictures should go by the board (whatever compromise should replace them) there can be very little doubt. It is well known to artists that wrong colours and incorrect shades have to be used to produce particular effects; and it may well be that positions never at any single moment assumed by an animal, may better suggest the idea of motion than the somewhat ungainly positions which are (*not assumed, be it noticed, but are*) passed through by swiftly-running animals. But it is quite certain that pictures so utterly and unnecessarily wrong as those which have hitherto done duty (save in a few exceptional cases) for moving animals, must be improved out of existence.

ELEPHANTS.

BEFORE the Jumbo mania has quite passed away, it may be well to place before the readers of KNOWLEDGE a few facts in connection with the distribution of elephants, past and present.

It is well known that the two existing species of elephant are found, one in Southern Asia, the other in Africa, and this represents their distribution ever since the dawn of history; but probably few of the multitude lately thronging the Zoological Gardens to see the gigantic African specimen lately shipped to America, knew, or cared to remember, that *perhaps* under their feet reposed the fossil remains of the much more gigantic ancestors of the great beast they came to admire, which once roamed wild through British forests; and that in the land to which he has been consigned, now quite destitute of wild elephants, abundant traces are found of animals of the same family long since extinct. The greater part of those found fossil in America belong to the gigantic Mastodon, which differed from the elephant in the form of its teeth, and in other peculiarities, although resembling it in general form. "The bones and teeth of the Mastodon," says Mantell, "have been found throughout the plains of North America, from north of Lake Erie, to as far south as Charleston in South Carolina. There were also Mastodons peculiar to Central and Southern America. The remains of other species have been discovered in the Crag of England, in France, Switzerland, Germany, Spain, and Italy, in Asia Minor, and in several parts of India." But in addition to the remains of Mastodon, both America and Europe can show geological records of a nearer relative of the elephant, in the fossil remains of the Great Mammoth, which are so abundant in Siberia, and which are found also in India. There is more than one species of elephant found fossil in England, but the Mammoth is the chief, and great interest attaches to it, because although doubtless it has been extinct for an immense period, its remains are found both in England and France, associated with the rough stone (Palæolithic) implements of early man. In the river gravels and caves of England, many fossil elephants have been found with the weapons and tools of the wild hunters by whom probably they had been slain; and all doubt as to the co-existence of man with this huge extinct animal is set at rest by the discovery, in one of the French caves, of a drawing of the Mammoth, made by the cave dwellers on a part of a tusk of the animal. A cast of this remarkable drawing may be seen in the British Museum, and by it we see that this extinct elephant was covered with long hair and wool, and had enormous tusks; the truthfulness of the representation having been proved by the discovery in Siberia, in 1799, of an entire carcase imbedded in ice, where it had lain for unknown ages.

The great interest of these discoveries lies in the fact of their being found in the cold regions of Northern Europe, Asia, and America, whilst at present elephants do not range farther north than 30° N. The woolly covering of the Mammoth doubtless enabled it to endure great cold, but it is certain that at present it could not find sustenance in those regions where it was formerly so abundant; therefore its presence denotes that a great change of climate has taken place in those regions, a fact confirmed by many other geological observations, proving that a warm temperate climate and abundant vegetation once existed within a few degrees of the North Pole.

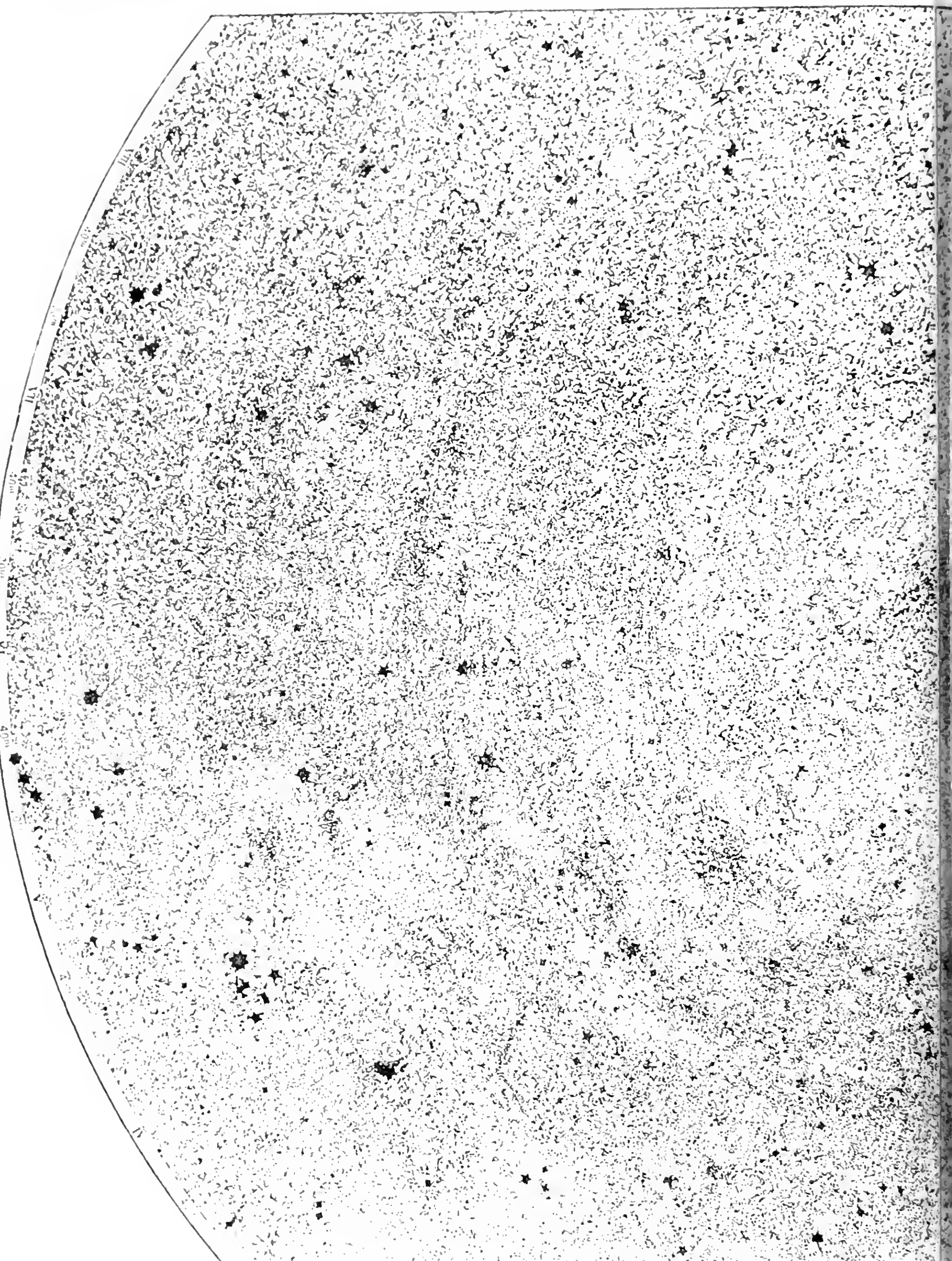
The question then arises as to the birth-place of these gigantic ancestors of the elephants of Asia and Africa. From the abundance of the fossil remains in the countries before-named, it seems probable that they originated in the north; nevertheless, it must be remembered that the bones of many species of elephant have been found among tertiary deposits both in India and Europe, and how they managed to spread themselves over the American continent and into Africa is a problem of general, as well as scientific interest. It is evident that they do not owe their distribution to human agency, for although man has been proved to have co-existed with the Mammoth, it is certain he did not then possess cranes and steam ships to convey huge beasts across the ocean; it therefore follows that at the time when Mammoths ranged freely over Britain, our present island was not divided from the continent by the waters of the Channel; this shows a considerable change in the distribution of land and water, but the change must have been still greater in the North, to enable the Mastodon and the Mammoth to pass between America and Asia. As to Africa, which does not appear to have been the original home of the elephant, it was undoubtedly at one time united to Europe by land in the Mediterranean, which allowed the passage, not only of elephants, but of other great beasts now exclusively African, as the rhinoceros and hippopotamus, which are also found fossil in England; but this connection had been broken long enough to allow of the total extinction of the Mammoth in Europe, and the rise of the new species of elephant to which Jumbo belongs, in Africa, long before the dawn of history.

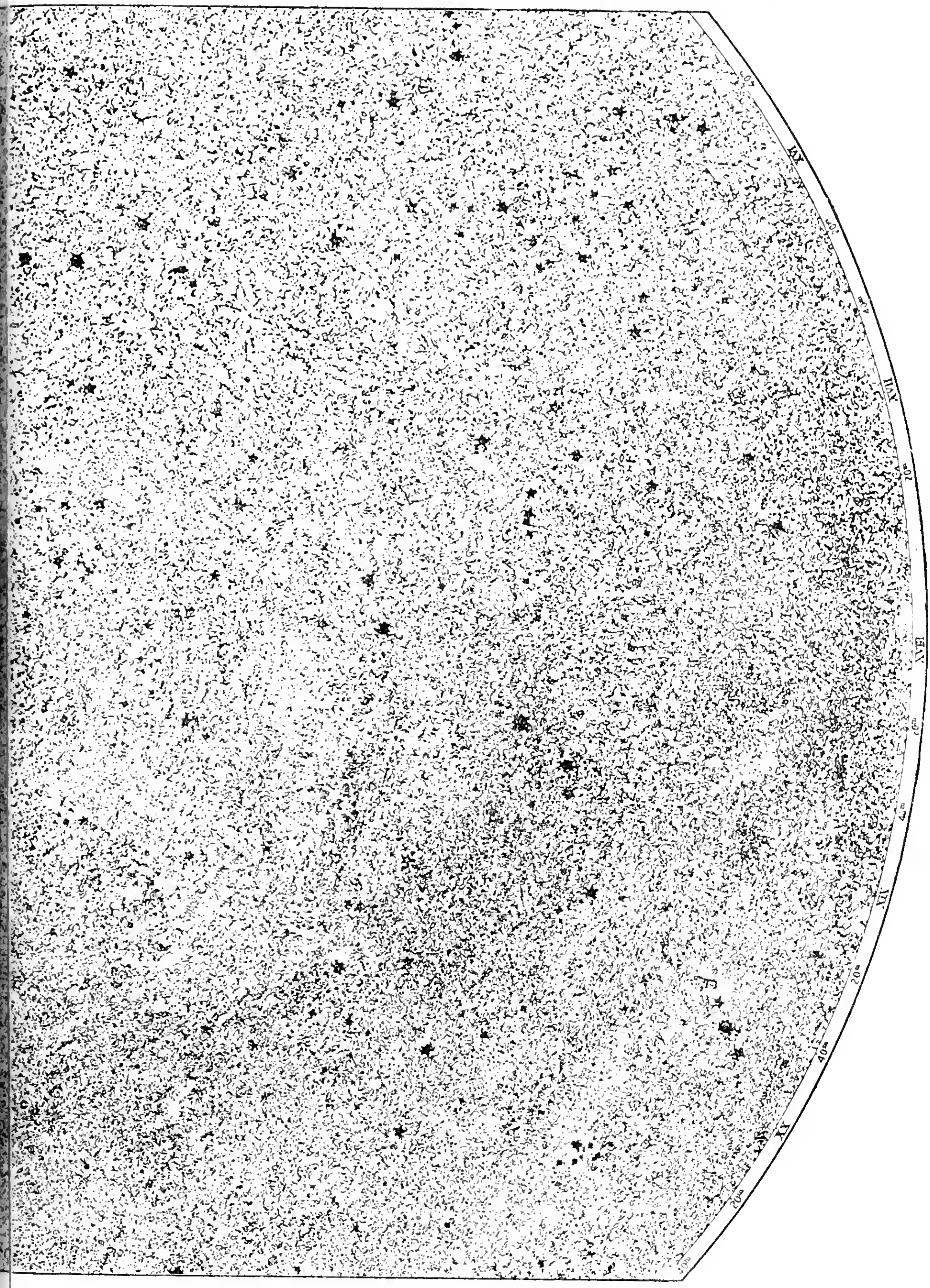
This subject has been ably treated by Mr. Wallace in his books on the "Geographical Distribution of Animals," and "Island Life," to which I would refer such readers of KNOWLEDGE as may desire more information on these most interesting questions; meanwhile, it is possible that the deportation of Jumbo to America may in the distant future restore the elephant to that great continent. We know that ancestors of the horse are to be found fossil in America, but the horse was unknown to man on that continent before the Spanish conquest, though now present in vast wild herds, so likewise it is possible that Jumbo's descendants may at some time range over the prairies as the Mastodon did of old; and naturalists of future centuries may, perhaps, trace to the favourite of the British Zoological Gardens the rise of a new species of elephant in America.

A. W. BUCKLAND.

[346]—ACCUMULATIVE SINKING FUND.—The following table, showing the number of yearly drawings, (each increasing by the interest on the bonds redeemed) required to pay off loan at par) will be found useful:—

Int. on Loan.	½ p. c.	1 p. c.	1½ p. c.	2 p. c.	2½ p. c.	3 p. c.	5 p. c.
3	65·83	46·90	37·47	31·00	26·67	23·45	15·90
4	56·02	41·04	33·43	28·01	24·36	21·60	14·99
4½	52·31	38·73	31·49	26·78	23·39	20·82	14·58
5	49·15	36·72	30·05	25·68	22·52	20·10	14·21
6	44·02	33·39	27·62	23·79	21·00	18·85	13·53
7	40·03	30·73	25·64	22·23	19·73	17·79	12·94
8	36·81	28·55	23·98	20·91	18·65	16·88	12·42
9	34·17	26·72	22·58	19·78	17·71	16·09	11·95
10	31·94	25·16	21·37	18·80	16·89	15·38	11·53

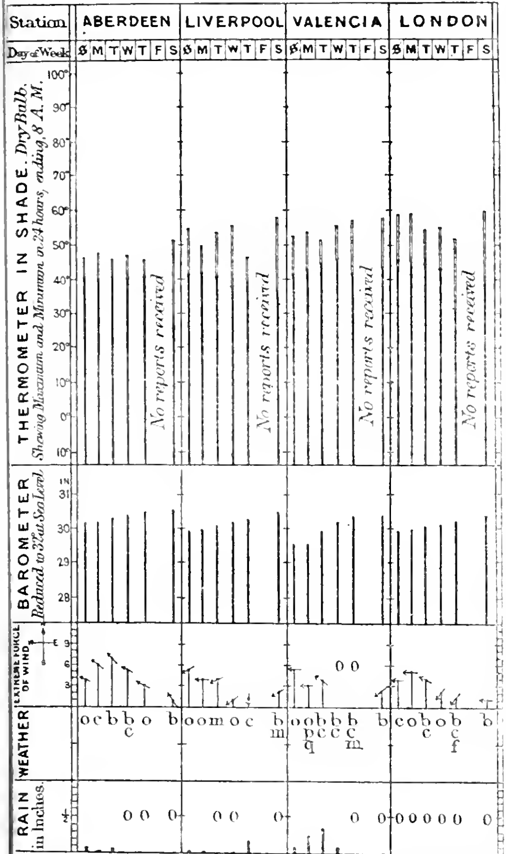




RAIN | WEATHER | BAROMETER | THERMOMETER IN SHADE

WEATHER DIAGRAM.

FOR WEEK ENDING SATURDAY, APRIL 8.



Weather.—*Beaufort Scale* is, b. blue sky; c. detached clouds; d. drizzling rain; f. fog; g. dark, gloomy; h. hail; l. lightning; m. misty (hazy); o. overcast; p. passing showers; q. squally; r. rain; s. snow; t. thunder; u. ugly, threatening; v. visibility, unusual transparency; w. dew.

CANALS ON THE PLANET MARS.

In a letter to the *Times*, Mr. Webb says:—It has long been known that the surface of the planet Mars is so mapped out into brighter and darker portions as to suggest the idea of continents and oceans, and the analogy thus implied with the arrangements of our own globe is strengthened by the existence of brilliant white patches, as of snow or ice, situate at or near the planet's poles of rotation, and varying in extent with its changing seasons, as well as by occasional differences in outline or colouring, which may well be explained by the supposition of a vaporous atmosphere.

In the autumn of 1877 and spring of 1878, when the planet was in a part of its orbit which presented its surface advantageously to view, a number of minute, straight, black or dusky bands were detected by Schiaparelli, traversing and subdividing the supposed continents in various directions. These have been called from their aspect "canals," though, of course, their scale entitles them rather to the appellation of straits, or very long, narrow arms of the sea. A few of these had been previously seen by various observers, but to the Italian astronomer belonged the credit of developing and delineating them as a system. At the ensuing return of the planet in 1879-80 they were again detected and drawn by him, with very little difference. But during the course of last January and February he has been so fortunate as to perceive the duplication of these dark streaks by the addition of parallel lines of similar character and length in no fewer than twenty instances, covering the equatorial region with a strange and mysterious network, to which there is nothing even remotely analogous on the earth, and which leads us at once to see how premature have been our conclusions in this respect, and how far we still are from any adequate conception of the real constitution even of our nearest neighbour but one in the solar system.

T. W. Webb.

[I have thirty or forty tracings of views of Mars taken several years ago by Mr. Dawes—"eagle-eyed Dawes" as he was aptly named—in which, though he used but an 8-inch telescope, some of the long narrow passages mentioned by Mr. Webb are shown. I mention this, because it may serve to corroborate what otherwise might seem improbable, the circumstance that Signor Schiaparelli should have seen with his comparatively small telescope what has escaped the attention of observers using such instruments as the Herschelian reflectors, the three-feet reflector made by Mr. Common, and the magnificent 26-inch refractor of Washington. Albeit until observers with such instruments as these have distinctly seen what Signor Schiaparelli has mapped, we must not too hastily assume that these are real features of Mars. Mr. Nathaniel Green, whose fine lithographs of Mars adorn a recent volume of the Memoirs of the Astronomical Society, considers that these narrow passages are due to an optical illusion which he has himself experienced.

Should it be proved that the network of dark streaks has a real existence, we should by no means be forced to believe that Mars is a planet unlike our earth, but we might, perhaps, infer that engineering works on a much greater scale than any which exist on our globe have been carried on upon the surface of Mars. The smaller force of Martian gravity would suggest that such works would be conducted much more easily there than here, as I have elsewhere shown. It would be rash, however, at present to speculate in this way.—Ed.]

WERE THE EGYPTIANS AWARE OF THE MOTION OF THE EARTH?

Having shown from their ancient hieroglyphical texts that the Egyptians understood the true motion of our planet, it now only remains for us to see whether this fact is corroborated by the accounts we have in classic authors of the opinions of the Egyptians on astronomical subjects. In discussing the matter from this point of view the greatest caution is necessary, because an ancient writer might assign to them an explanation of astronomical phenomena they never really held, from misapprehension, or supposing it is found in some philosopher's works who propounded a system allied to the Copernican, he might have falsely asserted his theory to have been derived from Egypt in order to gain for it a better hearing. M. Le Page Renouf, in his Hibbert Lectures, seems inclined to reject in many cases the assertions that certain Greek philosophers had been educated in Egypt, even doubting whether it was true that Pythagoras did so; but his views seem far too sweeping when compared with the universal testimony of the ancients, many of whose statements bear on the face of them evidence of truthful impartiality. For instance, Seneca says:—"Eudoxus first brought with him from Egypt into Greece a knowledge of the movements of the planets; nevertheless, he makes no mention of comets. Hence it follows that even the Egyptians, a people more curious than any other in all matters of astronomy, had occupied themselves but little with the study of these bodies. At a later period Conon, a most accurate observer, drew up a catalogue of the various eclipses of the sun observed by the Egyptians, but makes no mention of comets, which he would hardly have omitted if he had found any facts respecting them."

It would be an easy task to show that in all cases where the theories of the Greeks have approached most nearly to those we now know to be correct, they were expounded by men who are distinctly said to have studied in Egypt; but only a few of the most important of those which strictly appertain to our subject can be given.* Perhaps the most valuable remark of a Greek author is

* See Wilkinson "Ancient Egyptians," vol. II., 316, and I., 447. Also Humboldt, Cosmos II., 544, and 692, ed. Bohn. That Socrates *was* in Egypt is proved by a Greek inscription on an Egyptian temple, in which a Greek student says he was there "200 years after the divine Socrates."

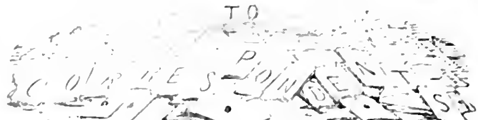
to be found in Diogenes Laertius's Lives of Philosophers, where, before describing them individually, he gives a short account of Egyptian doctrines derived from Manetho and the younger Hecatæus, of Abdera. In this, after saying they consider the true form of God to be unknown, and that the world had a beginning, and is perishable, it says of the earth, "It is in the shape of a ball. The stars are fire, and the moon is eclipsed when it crosses the shadow of the earth." That we have here no mistaken statement of Egyptian thought set forth is certain from what Diodorus says, for he emphatically tells us that "the Egyptians knew that the earth was round," and that the words are to be taken literally is evident from his naïve comment, that were this true the Nile could not rise from the other hemisphere to flow into this. Whether it is fact that some Greek philosophers gained their knowledge in Egypt; the proofs that Pythagoras did so are convincing, and although it may be doubted we have any writings of his own, yet it is universally admitted that his disciple Philolaus is a correct exponent of them. In addition to the common testimony that his Pythagorean astronomical system came from Egypt, as was shown by Bökh in 1819, Wilkinson proves that his theories of transmigration, emanation, and music were all derived from the Nile Valley. Now Plutarch says—"Whilst others consider the earth immovable, the Pythagorean Philolaus believed that it moved around the central fire in an oblique circle in the same manner as the moon and the sun, and we have the evidence of Diogenes, Laertius, and Theophrastus, that Hicetas of Syracuse believed the earth revolved in a circle around its own axis." Philolaus here falls into the error of considering the central fire round which the earth revolves to be distinct from the sun, a mistake held, as Aristotle tells us, by the Italian followers of Pythagoras, but he may have been the cause of the error by misinterpreting the Egyptian views; and Aristarchus of Samos, a brother Pythagorean, has explained them correctly when he says, as quoted by Archimedes, who, after alluding to his denying that the earth was immovable in the centre of the universe, informs us he considered the sun constituted this centre, and was immovable, *like the other stars*, whilst the earth *revolved around the sun*. This great thinker alludes to the twofold motion of the earth on its axis and round the sun elsewhere; and further, the Egyptians, either from having observed the passing of sun spots over the solar disk, or for some other reason, conjectured that the sun also revolves, for, according to Plutarch, "In the Egyptian Hermetic books, when the sacred names are mentioned, Hermes is said to be Apollo, and to represent the *rotatory* motion of the sun" (Bunsen's "Egypt," vol. i., page 417). In conclusion, it may be said, therefore, that we have, as far as our knowledge enables us to judge, every reason to believe that the Egyptians understood the movement of the earth in the heavens. We arrive at this from two courses of investigation—from what may be termed the purely Egyptian, viz., authentic texts penned by themselves, and from the information handed down to us by the Greeks, mostly in reference to the doctrines of the Pythagoreans, which were, by everyone qualified to know, looked upon as Egyptian, but sometimes these correct ideas of astronomy are directly referred to Egypt herself.

A Member of the Society of Biblical Archæology.

MR. MATTIEU WILLIAMS ON COD "SOUNDS" AND "SCIENTIFIC PRIVILEGE."

I SHALL leave "Old Fossil" to settle with Mr. Mattieu Williams the nature of the structure in the cod, concerning the proper name of which Mr. Williams appears to be specially perturbed. I might remind the disputants that *post-mortem* appearances, both in men and fishes, are often deceptive. One thing, however, is quite certain—namely, that no one undertaking the dissection of a cod with any degree of care, could ever mistake the "sound" or "swimming bladder" for the *dorsal aorta*, or main arterial blood-vessel of the fish. The swimming-bladder of the cod contains air, not blood; although there is a beautiful network of blood-vessels (*retia mirabilia*) closely connected with the air-bladder. That which concerns me in Mr. Williams's communication is my former contention that whatever fishermen call the "sound," the term is used in zoology as synonymous with the names "air-bladder" and "swimming-bladder." As I do not find in Mr. Williams's article a single word disproving this contention, I must leave the matter where Mr. Williams's common sense is content to let it rest. If he prefers, with the fishermen, to limit the name "sound" to another structure than the "air-bladder" he is very welcome to continue the practice. Zoologists are not likely to follow his lead.

A. Wilson.



Letters to the Editor.

[The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with their writers. All communications should be as short as possible, consistently with full and clear statements of the writer's meaning.]

All Editorial communications should be addressed to the Editor of KNOWLEDGE; *all Business communications to the Publishers, at the Office, 74, Great Queen-street, W.C.*

All Remittances, Cheques, and Post-Office Orders should be made payable to Messrs. Wyman & Sons.

**** All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.*

All Letters or Queries to the Editor which require attention in the current issue of KNOWLEDGE, *should reach the Publishing Office not later than the Saturday preceding the day of publication.*

(I.) Letters to have a chance of appearing must be concise; they must be drawn up in the form adopted for letters here, so that they may go untouched to the printers; private communications, therefore, as well as queries, or replies to queries (intended to appear as such) should be written on separate leaves.

(II.) Letters which (either because too long, or unsuitable, or dealing with matters which others have discussed, or for any other reason) cannot find place here, will either be briefly referred to in answers to correspondents, or acknowledged in a column reserved for the purpose.

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

"God's Orthodoxy is Truth."—*Charles Kingsley.*

Our Correspondence Columns.

THE POTATO.

[381]—I was pleased to see "Farmer's" contradiction in No. 20 to my article on the potato; for it shows me that English farmers are at last beginning to take some interest in agricultural research. May the day be not far distant when we shall be able to look up to our farmers as authorities on such matters, instead of having to look abroad to see how to feed our cattle, manure our land, &c., as we have to do at present.

I should, perhaps, have liked to have seen "Farmer's" letter a little more to the point; for as it now stands it is only "negative" in its contradictions, and I think I said in my article that "negative" results were of no use in science unless accompanied by "positive" (when obtainable), to bear out the investigator's or critic's assertions. For instance, "Farmer" should have told us the difference in weight between the Victorias from which the blossoms had been removed, and those on which the blossoms had been allowed to remain. Perhaps he did not think it worth while to give the weights, but they are really important in a matter of this kind.

"Farmer," I am sure, will excuse me for saying that I do not quite see the point of his second remarks; for he himself ends them by saying, "and has none of the sweetness peculiar to a *frosted* potato."

Thirdly, "Farmer" alludes to the compost; and as the "unmanured plot," &c., is connected with the same contradiction, I will answer the two by saying, that the "compost" was one of six or seven manures tried on the same kind of land (sandy, I believe), at the same experiment, while the "unmanured" was that plot which did not receive any manure; and so the deduction was fairly arrived at, I think. Has "Farmer" tried the effect of the different manures in this way? If he has, his results will be valuable. Further, I may say that in this case my remarks about "Potatoes grown," &c., do not exactly contradict the remarks about the "unmanured plots." "Farmer" says, "one assertion flatly contradicts the other, and both are contrary to his or any one else's experience." Does his experience, or that of his friends, lie evenly between the two points? He does not say!

I will now quote my authority for the various assertions I have made, and have much pleasure in doing so, especially if "Farmer" intends to pursue the subject in the paper I shall name, for I am sure another worker in the *Field* will be hailed with delight by all scientists abroad.

I will name his contradictions 1, 2, 3, 4, 5, to save space :—

1.	Bird. Cents.	1879.	631.	
2.	,,	1880.	472-474.	
3 & 4.	,,	1879.	106-108.	W. Paulson.
5.	,,	1880.	868-870.	W. H.

F. C. S.

TRICYCLES.

[382]—As a bicycle and tricycle rider of many years' standing, and one who has tried many kinds of tricycles, I would strongly warn your readers against taking Mr. Browning's advice, and getting a "creeping thing" in the way of a tricycle, with small wheels, under any circumstances. It is obvious that the smaller the wheels, the more must inequalities in the ground and stones inconvenience the rider, and this is an important point. Large wheels pass unjoltingly over an obstacle that would clash the teeth on a small 36-in. wheel.

In case of an emergency, I should not like to have my toes in a stirrup, as with the Monarch. It might be very awkward.

My settled opinion is, that a 50 or 52 in. Cheylesmore is the perfection of tricycles. The machine is beautifully finished, very light, and yet strong, and is also one of the fastest runners and easiest hill-climbers. Then comes the Rucker, the Premier, and perhaps the National. The Humber is fast, but I concur in Mr. Browning's remarks upon it in other respects.

It is a great mistake to have too heavy a machine, no matter the rider's weight, and strength can be secured without ponderosity.

I have just had a three days' ride to and along the salt coast and back on a Cheylesmore (about 190 miles), and rode every hill both up and down (although I crossed the South Downs), and experienced no fatigue whatever.

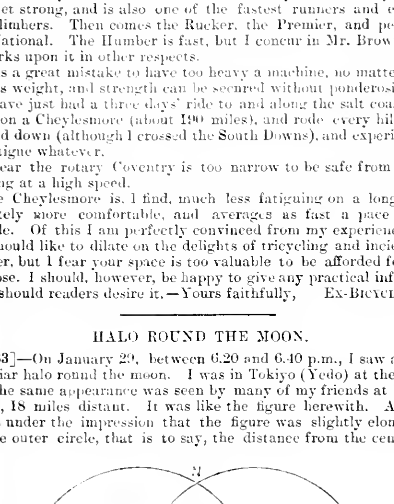
I fear the rotary Coventry is too narrow to be safe from overturning at a high speed.

The Cheylesmore is, I find, much less fatiguing on a long ride, infinitely more comfortable, and averages as fast a pace as a bicycle. Of this I am perfectly convinced from my experience.

I should like to dilate on the delights of tricycling and incidental matter, but I fear your space is too valuable to be afforded for the purpose. I should, however, be happy to give any practical information, should readers desire it.—Yours faithfully, EX-BICYCLIST.

HALO ROUND THE MOON.

[383]—On January 29, between 6.20 and 6.40 p.m., I saw a very peculiar halo round the moon. I was in Tokiyo (Yedo) at the time, and the same appearance was seen by many of my friends at Yokokama, 18 miles distant. It was like the figure herewith. At first I was under the impression that the figure was slightly elongated on the outer circle, that is to say, the distance from the centre of



the moon to the edge of the first halo was less than that from the edge of the first halo to the farthest point in the second; but on drawing it I am convinced that this was an optical illusion. The lower part of the second halo intersected the lower horn of the moon in a peculiar way, as given in the drawing. The moon was nearly half full.—Yours, &c.,

Yokohama, Japan, Feb. 13, 1882. H. PRYER.

PERSONAL ILLUSION.

[384]—I observe in your issue of March 18 that a correspondent, under the *nom-de-plume* of "Jumbo," mentions as an instance of the above the fact that to him "horizontal lines appear plainer than perpendicular" ones. I beg to suggest that this phenomenon more probably depends on a physical than a psychical cause. It is produced, in the great majority of cases, by a difference of refraction in the vertical and horizontal meridians of either the lens or cornea of the eye. The effect of this is to cause the one set of lines to be brought to a focus on the retina, while the ones at right angles to them are not. This condition can be corrected in most cases, to a great extent, if not entirely, by the use of cylindrical glasses. G. A. HERSCHELL, M.D. Lond.

[Answered by W. R. D., F. T. W., and several others.—ED.]

REPLIES TO QUERIES.

[318]—VEGETARIAN BOOKS and pamphlets are numerous, but perhaps G. A. S. would find what he wants in Dr. Nichol's "How to Live on Sixpence a-Day," price 6d. [Nichols & Co., 23, Oxford-street, London]. Of cheap cookery books there are, "How to Spend Sixpence," price 1d., "The Penny Vegetarian Cookery," and the "Food Reform Cookery Book," price 2d., to be had from the Secretary of the Vegetarian Society, 56, Peter-street, Manchester. I should be glad to answer any letters addressed 16, Meadow-street, Moss Side, Manchester, as a full answer to G. A. S.'s query would occupy more space than the editor could spare.—A FELLOW OF THE CHEMICAL SOCIETY.—[Many other replies received; but the above will suffice: and more also.—ED.]

[337]—SELF-ACTING BLOWPIPE.—Such is for sale in the better class of tool-shops. A vessel with a screw top holding spirit, has a small pipe leading from near the top of its interior through the bottom, where it is turned at a right angle. A spirit-lamp underneath heats the spirit in the vessel; the vapour given off is expelled through the pipe, and passing over the flame of the lamp is ignited. A long roaring flame is the result, but it is not so hot as a good blowpipe flame, as the combustion is only that of the spirit without admixture of air. The flame is a large one, and is quite unsuited for soldering, but I have used it for manipulating glass tubing, as it allows the use of both hands.—CLARE.

Answers to Correspondents.

**** All communications for the Editor requiring early attention should reach the Office on or before the Saturday preceding the current issue of* KNOWLEDGE, *the increasing circulation of which compels us to go to press early in the week.*

HINTS TO CORRESPONDENTS.—1. *No questions asking for scientific information can be answered through the post.* 2. *Letters sent to the Editor for correspondents cannot be forwarded; nor can the names or addresses of correspondents be given in answer to private inquiries.* 3. *Correspondents should write on one side only of the paper, and put drawings on a separate leaf.* 4. *Each letter should have a title, and in replying to a letter, reference should be made to its number, the page on which it appears, and its title.*

ASTRONOMY AND MATHEMATICS.

J. MURRAY. How severe the P. D. system must be, which will not allow Venus to be more than 5,000 miles in diameter! It is some comfort to know that, as "gravitation or retardation is a compound body, it is hard to say to a 1,000 miles off-handed." Yet you say, "Without a donght" (the worst spell of doubt I have yet experienced) Mars is only about 1,600 miles in diameter.—A. N. S. (1) Thanks for calling my attention to the undoubted mistakes occurring in the times noted p. 468. I took down the *Nautical Almanack* for 1881, instead of 1882. Fortunately the reasoning is in no sense affected by the mistake. (2) Astronomers find it convenient to take the sun's centre; that is the only reason I can think of.—D. MAXWELL. Your reasoning is incorrect, though plausible. You cannot so simply deal with the attraction of the segment of a sphere, as to assume that it acts as though the whole mass were at the centre of gravity. The sphere (either of uniform density, or composed of concentric shells, each of uniform density) is the only solid whose resultant attraction on a particle is the same as though its whole mass were collected at the centre of gravity. As a matter of fact, the force of gravity within a globe of uniform density diminishes as the distance from the centre. Note also, that if through a particle in such a globe, a spherical surface concentric with the surface of the sphere be supposed to pass, the portion of the globe outside that inner spherical surface produces no effect whatever on the particle. — M. BOYCE. Your friend was right. The sun is far north of east at 6 A.M., either mean or solar time. By solar time, he crosses at 6 A.M. and 6 P.M. the great circle passing through the east and west points and the pole of the heavens (a declination circle), *not* the great circle passing through the east and west points and the zenith.—A. S. T., M. H. C., ERIN-GO-BRAGH. Thanks.—NOBLEHILL. I beg to inform you that pressure would not vanish at the earth's centre. Pressure *there* is not the result of gravity *there*, but of gravity *elsewhere*, and therefore your reasoning has no weight. The matter above the earth's centre has, on the contrary, a great deal of

weight hence, very much pressure. R. Bailey Walker, F.S.S. Many thanks. F. H. J. H. You can magnify a flea to the size of a brewer's horse, or of Jumbo, by means of the oxyhydrogen lantern. —Sexex. Certainly, the velocity and distance of any two planets are connected by Kepler's law. Thus, take two planets A and B, let their periods be p and P, their distances d and D, their velocities v and V. By Kepler's law:—

$$d^3 : D^3 :: p^2 : P^2$$
$$\therefore \frac{d^2}{p^2} = \frac{D^2}{P^2} \cdot \frac{D}{d}$$
$$\text{But obviously } v : V :: \frac{d}{p} : \frac{D}{P}$$
$$\therefore v^2 : V^2 :: \frac{d^2}{p^2} : \frac{D^2}{P^2} :: \frac{D^2}{P^2} \cdot \frac{D}{d} : \frac{D^2}{P^2}$$
$$:: D : d$$

whence $v : V :: \sqrt{D} : \sqrt{d}$, or the velocities of planets are inversely as the square roots of the distances.—J. Lankshear. Yes, the darker part of the moon can often be seen. That part is lighted by the earth; for when the moon is new to us the earth is full to the moon.—C. E. Thorburn. Thanks.—J. T. D. How could the pole and centre coincide, however the earth might be flattened? If they coincided, the pancake earth would be of no thickness at all, which is absurd. Centrifugal force at equator accounts for only a part of the difference. For the attractions at equator and pole, independently of rotation, see Todhunter's "Statics." The moon question rather more complex than you seem to think.—J. B. Moffat. We have not "G. E. V.'s" address. F. D. It seems to me there is no confusion and no difficulty in the probabilities question.

ELECTRICAL.

Eccentric Chuck. 1. Swan's lamps are 5s. each, the other forms are 10s. to 12s. 6d. 2. About twenty Bunsen cells, in good condition, will light up a Swan lamp for three or four hours at a time. 3. Speaking at the Royal Institution a few weeks since, Mr. Swan said his lamps were reliable for 1,200 hours continuous burning; but I believe they are now safe for at least 2,000 hours, or about twelve months ordinary burning.—L. J. The Swan lamp (see preceding answer) may be obtained for 5s. of Swan's Electric Light Company (Limited), 13, Mosley-street, Newcastle-on-Tyne; 25s. was the price, until recently, and further reductions, at least, in the other systems may, perhaps, be expected.—Ashbridge-Robinson. A large-sized Pickford van might, perhaps, hold a sufficient number of Daniell Battery cells to maintain fifteen Swan or Edison lamps, but it is very doubtful. An induction coil is of no use. You would require an engine and a dynamo-electric machine.—A. W. B. To make a Grove's cell, get an earthenware jar of, say, a quart capacity, into which put an amalgamated zinc cylinder, and inside this a porous pot containing a sheet of platinum. In charging, put concentrated nitric acid in the porous pot, and sulphuric acid, diluted with ten times its volume of water, in the outer cell. Substituting a block of gas carbon (a waste product in gas-making) for the platinum converts the Grove into a Bunsen. Generally the Grove is made in the flat form, the Bunsen almost invariably being round. Is this lucid enough? You can buy either for about 5s. per cell, but can make them for about 3s. or 3s. 6d. 2. Your best plan is to write to the various companies, offering your services, and stating your qualifications.—E. C. H. A description of the Brush machine is already written, and only waits its turn to appear in Knowledge.—S. G. T. Really, I am puzzled how to answer your queries. However, I will try what I can do. In the first place, the engine does not produce electric sparks. Secondly, friction is of no use to produce the electric light; and, thirdly, galvanic batteries are not very much better, although a light can be obtained from them; nor, fourthly, are the "magnetic coils affected by the action of the engine" beyond being rapidly rotated before the poles of a magnet. Read our articles on "Electric Generators." "The Engineer," and "Engineering," for the past two or three years, contain the information you require about gas-engines.—F. A. S. We shall describe the Gramme machine "when space permits," but it is more difficult for amateurs to make than a modification of Siemens's machine, which we are describing in our columns for the "Amateur Electrician." Urquhart's "Electric Light" is, I think, the most recent work on the subject; but, although a good book, it is rather crude in some of its descriptions.

MISCELLANEOUS.

G. M. Gorham. The true theory of the rainbow would be rather too abstruse for our readers; albeit, if you had not lost the one you mention, we should be very willing to publish it.—M. T. H. Do you therefore reason that because our grandmothers were idiots in matters of dress, their granddaughters need not mind being merely foolish?—F. H. S. Your questions quite unsuitable. Our readers (and we too) care for none of these things.—Fons Bandusii. Do not know.—Gregory. Very likely there is a good deal of electricity about the sun. Any details?—P. H. Carpenter. We have already arranged with a geologist for the discussion of such subjects. *Our Query column is defunct, and so is our Reply column.* C. R. Bree, M.D. Yes, but unfortunately for your argument the egg is not an ellipsoid.—W. Mabel. We cannot take up the subject. If the trade cares to try it, let them do so; they have our full and free permission. They have not asked for it, but that is a detail.—J. Allen Brown. Quite so. Dr. Siemens's theory will, however, never be attacked seriously by science, for a very sufficient reason—it has no life, therefore needs no killing.—Anno Domini. "Oh, bosh! the worthy bishop said;" for which you will overhaul the "Bab Ballads"—perhaps.—G. W. B. Yes, but perhaps Mr. S——, of London, will advertise as much for himself. It is not our business to do so.—Bernard Batigan. See No. 24.—W. Roberts. Arabic names explained shortly. Know nothing showing Cromwell believed in astrology. He was superstitious in some things—strangely so for so strong a mind.—J. A. Bromley. Many thanks for the Dutch barometer. My being the "fountain of knowledge" does not help me to get the water in, but will try all three given methods.—Thomas Smith, Jun. Nobody questions that character is shown by the shape of the head; so it is shown by the chin, the nose, the cheek-bones. The so-called science of phrenology, as advanced by Gall, Spurzheim, and all their followers who really know what phrenology means, asserts that underneath the so-called bumps are the cerebral organs corresponding to the several qualities associated with those bumps. It is this that science rejects. One can tell character as well from nose, lips, chin, eyes, jaws, and so forth, as from cranial development, and nine-tenths among so-called phrenologists (I suspect all) really judge of character thus, and not from the bumps. Now I venture to assert that there is as much reason for asserting that there is an organ of Voluptuousness (Capital V., if you please, Monsiear le Compositor) within the lips, and of Resolution within the chin, because full lips indicate a volaptuous character and a prominent chin resolution, as for saying that there is an organ of Destructiveness in the part of the brain behind the ears, and an organ of Philoprogenitiveness just above the nape of the neck. But the fact of the matter really is that so-called phrenologists of our time know nothing whatever of what Gall and Spurzheim really taught.—J. I never met with a man of science yet who did not agree with what, as you tell me, Professor Boyd Dawkins said at Manchester. Sir W. Thomson's theory of the meteoric origin of life is a joke, nothing more; if he really maintained it, it would be a jest.—F. W. Halfpenny. Yes, there can be no doubt the Egyptian books contained nearly all that we find in the books you mention, and a good deal more; so did the Assyrian stone records: and many go about saying, in favour of the account you refer to, very much what Rogue Riderhood used to say for himself ("*Now* say I'm a liar.") But the subject is hardly suited for our columns. We do not want to know what has been taught, but what *is*.—W. J. Cant. We hope to publish the figures drawn by compound pendulum, but, hitherto, the photographic record is incomplete. The author of the paper you mention has very kindly sent some red tracings, but the red is aniline, and unsuitable for photographic effect. Your stamped and directed envelope might be used if we could do so without injustice to some 750 others.—G. H. Mortimer. We can no longer publish book titles. Your query unsuitable.—E. C. R. Can you not look up Mr. Judd's book yourself? (Similar questions—that is, questions relating to books—received from J. H., L. Brown, W. Hartwig, M. Conybeare, J. H. Ludwig, and multitudes of others.—Vacuum. Query unsuitable. We must consider the many, not the few, or mere units.—M. B. Alder. You think Grant Allen very wicked, and also very silly, because he says, "the daffodil has done so and so;" others (myself, for example) think his way of putting these things perfectly delightful. What can I do, except to quote the old saying, *De gustibus non est disputandum*? But, tell me, where does science stop and blasphemy begin? Is it blasphemous to say that child grows, or this tree thrives? If you can allow your mind to admit development on the small scale, can you not, by any possibility, admit it on the larger scale too? Can you not see that to Him "who works in and through all things," there can be no distinction (as with us) of large and small, long lasting and transitory? For my own part, when I hear fanatics raving against the general doctrine of evolution, I see in their ineptitude the strongest evidence for evolution. Their inability to see that the same Being who can arrange for the evolution of the fowl from the egg or the man from the embryon can also presumably arrange for the development of the race also from a race of lower

type, may be compared to the incapacity of the infant for that which is easy for the grown man, or for the inability of the lower races to effect what is well within the power of the higher. I say with you, "there is something loathsome about untruth." It would be exceedingly loathsome, therefore, for me to say I agree with you in aught save that general proposition NARCISSUS, LUNATIC. "No more on that head," as the bald man said.—FOREIGNER. Man is inquisitive, and wants to know what there is at the Pole. That is the only reason I can imagine for polar expeditions.—EASY. Thanks; but do not see our way to taking a part in the matter.—SCALPEL, C. H. C. We have been obliged to decline to insert *any* question asking for name or names of books on special subjects, though we question the *bona fides* of no individual querist. In three cases we have had replies to such queries in the same handwriting as the queries themselves. We are sorry that for the sake of a few dishonest querists, the great majority of honest ones suffer, but we cannot help it.—NICHOLAS WHITLEY. That may do very well for the V. I., but we would as soon insert a paper by Parallax asserting the earth's flatness, or any other absurdity.—J. B. T. Well: but that is just what inertia requires.—R. W. BRANTHWAITE. You say "the dental formula at p. 459 is entirely wrong and needs correction," and then you give the dental formula for man. Since No. 1 of KNOWLEDGE appeared, I have been told many things which I knew before, and I thought scarcely anything in that way could surprise me; but I frankly confess you beat me. I did not expect to have it carefully (and I must admit very lucidly and fully) explained to me that man does not ordinarily possess twelve incisors, or twenty-eight molars, or forty-four teeth all told. "It will go near to be thought so shortly." Where, in p. 459, is it stated that the dental formula there given is man's? ROB CRONIEL (? ?). We could scarcely find space for Cottier Life.—GERALD MASSEY. Thanks; but these verbal questions are not for us. The mistakes in the letter arose from its accidentally escaping correction. There are no such eight stars.—G. W. HART. Thanks; but Chemistry of Genesis would hardly suit us.—THOS. A. COTTON. We cannot say where that or any instruments are to be purchased, either here or privately. In fact, stamped and addressed envelopes, except for return of MSS., are sent to us in vain. Our rule on that subject has been very clearly stated.—C. A. R. The subject is a good one, and we hope to have such an article shortly.—G. F. HANAP. We cannot take the office of private tutor even to a section of our readers. Besides, the questions you ask are not even complete.—JAMES DEAS. Guillemin on Comets, translated by Glaisher. Can recommend no book on astrology.—UNITAS. Thanks.—COGITO. Mr. Grant Allen appears to presuppose some intelligence in his readers, when he speaks as if there were intelligence and volition in plants.—J. RUSSELL C. Cheese problem rather too ordinary.—ALPHA. The right answer, as I understand the question, is $42\frac{6}{7}$. A takes one-third before D, who takes two-ninths, retires. A then should now take nine-sevenths of one-third, or $42\frac{6}{7}$ per cent.—ARTHUR GEARING. You solve a problem not given in these pages. A rectangle formed of three squares is not the same thing as three squares arranged so as to form three-quarters of a square.—ZETA. Loomis's Astronomy has good refraction tables. Price, I think, 8s. 6d.—A. N. S. If that (referring to your probabilities question) needs proof, we had better prove that two straight lines cannot enclose a space.—H. C. STANDAGE. Thanks, but table of payments quite unsuitable.—COMITATUS. I suppose the front engine draws the front half of the train, and the hind engine pushes the hinder half.—J. PARKER FOWLER. If you were acquainted with the formulas of spherical trigonometry you could hardly ask the question; if you are not acquainted with it, we can hardly answer it. Why cannot we see through a brick wall? sounds like a conundrum. Light is not "lost in passing through the wall." It does not pass through, but is partly reflected, partly absorbed, at the surface.—JAMES HARRINGTON. I do not know Mr. John Hampden's address, but if I did, I could not forward your communication; it would be a breach of our rules. Let me advise you not to waste time and money on such nonsense.—ERIN-GO-BRAGH. Thanks; but the facts mostly familiar.—H. C. STANDAGE. Pigments not in our line.—S. M. COX. Thanks. About possible hands there is no difficulty so long as there is clear definition of what constitutes different hands.—B.M., F.R.C.S. You may depend upon it Sir Edmund Beckett knows all about the advantage of having the screw-driver properly ground. Telling *him* that a mechanical device he suggested would be troublesome, unworkmanlike, and unnecessary — would be like telling Hanlan that he had entirely erroneous ideas about rowing.—M. WYATT. Thanks; but as there is no practical value in rules for finding roots, we must reserve the space the subject would take for other and more pressing matter.—AN ENGINEER. We noticed the passage, but did not understand it as you have done. No one can for a moment suppose that the presence of air in a bird's bones could add to the bird's buoyancy; but it should not be necessary to explain to an engineer that the hollow structure of the bones is adapted to give as much strength with as little weight as possible. The point you explain is fully dealt with in a Fitful reply of my own at p. 60.

Letters Received.

BOBBY.—(You are cool enough). J. Hargreave, L. M. N., K. Sperritt, M. N., J. Hannay, F. Finleyson, R. Morris, S. T. P., K. Pavitt, M. R. Winstanley, Peter Parley, Post Meridian, F. Friendlison, Avernus, M. J. Merryweather, Semper Paratus, R. Rudolfsen, Mons Parturiens, S. Silliman, J. R. Rendell, M. Purvis, K. H. Payne, R. P. T., Rap Back (Why? R. A. P.), Per aspera ad ardua tendo (Many do), J. V. M., Holocaust.

Our Whist Column.

By "FIVE OF CLUBS."

PLAY SECOND HAND WHEN KNAVE IS LED.

KNAVE, as an original lead, is played only (see synopsis of leads, p. 310, No. 11), (1) from King, Queen, Knave, and two or more small ones; and (2) from Knave, ten, nine, with or without small ones; except in the unusual case that original leader holds four trumps and three three-card suits, when Knave may be led from Knave, ten, and another. If then second player holds either King or Queen, he knows the lead is not led from (1); while, if he holds either ten or nine, he knows it is not from (2); and, lastly, if he holds one of the first set, as well as one of the second set, he knows that the lead is a forced one, the leader having probably no four-card plain suit. Taking the two more common cases, of a Knave lead, second player, if he holds King or Queen only, above the Knave, showing that the lead is from Knave, ten, nine, and others, should play a small one, unless with the Queen he holds the ten, when he should play the Queen. But this last case is very unlikely to occur, as it would imply that Knave had been led from Knave and one or two small ones. If second player holds both King and Queen, he should cover Knave with Queen. If he holds Ace and Queen with or without small ones, he should play the Ace, knowing that leader does not hold the King, so that covering with Queen is useless, whether third player or partner hold the King. If, when Knave is led, second player has no card above it, he will, of course, play his lowest. If second player holds King, nine, or Queen, nine, the lead is probably a forced one—from Knave, ten, and a small one; it is, therefore, useless to put on the higher card, unless circumstances render it very desirable to gain the lead. But usually the small card would be played, leaving partner to take the trick if he can.

PLAY SECOND HAND WHEN TEN OR NINE IS LED.

Here the principles are much the same as in the previous case. Ten (see synopsis of leads) is only led from King, Queen, Knave, ten, and from King, Knave, ten, with or without others. Therefore, if second player holds the Queen, with nothing to show that the lead is forced (and ten is very unusual as a forced lead), he knows that the lead is from King, Knave, ten, and should play the Queen, or not, according as she is singly or doubly guarded. It is obviously useless to retain her if she is only singly guarded, for she must fall next round; and as obviously unwise to play her if she has two guards or more. If ten is led, and second player holds Knave and nine (a fourchette), of course Knave should be played. The lead, with these cards outside the leader's hand, is very unusual, being from ten and one or two small ones. Nine is only led from King, Knave, ten, nine, with or without others. If second player, then, holds Queen only singly guarded, he should play her; but if she is doubly or trebly guarded, he should play his lowest.

PLAY SECOND HAND WHEN SMALL CARD IS LED.

The rules for play second hand when a small card is led are nearly all included in the general instructions already given. When second player is long in the suit led, he plays somewhat as he would if the suit were his own, and he were leading, only that, first, in playing one of a sequence he always plays the lowest, and, secondly, as his partner is in a more favourable position, being fourth player instead of third, he leaves more open to him. For instance, in leading from Ace and four small ones, Ace is played; but if second player holds these cards he plays a small one, unless the game is in a critical state, and either one trick or a lead is much needed; or if first player is one of those who affects the lead from a singleton. So when an honour is led and you hold a

higher honour and no lower in the suit, you pass the trick, except you have strong reason for desiring a lead (or wish to stop a trump lead, but that case belongs to play second hand in trumps). When, however, you hold Ace, Queen, and others second hand, a small card being led, your play is unlike that which you would adopt in leading from such a hand. Thus, with Ace, Queen, Knave, with or without others, Ace is always led, but with these cards second in hand, you play the Knave (or lowest of Queen Knave sequence), from Ace, Queen, ten, Queen is to be played; for if third player covers you remain with the tenace, so that unless led through again, and Knave lies with third player, you are sure to make two tricks in the suit (apart from ruffing). From Ace, Queen, and one or two small ones, you play second hand a small one on a small card led. From Ace, Queen, and three small ones or more, you play Queen, if weak in trumps, smallest if strong.

When you are short in the suit led, you play second hand on somewhat the same principles as in the case of the forced lead *of a suit in which you have no reason to believe your partner strong*. You play, of course, the lowest instead of the highest of a sequence, and you leave more to your partner, as he is more favourably placed.

But if you hold only two cards in the suit led, you reverse, as second player, the rule which holds, almost without exception, in the unusual case of a forced lead from a two-card suit. Thus, if you hold King, or Queen, or Knave, and one small one, you play the small one, not the honour (in plain suits), unless you have some special reason for desiring to obtain the lead, in which case the King may be played, second hand from King one small one. Many skilful players of their own hand maintain, with some obstinacy, that Queen should be played second hand from Queen one small one, because the chance of her falling is so great, greater than that of King falling when you retain him second in hand. The chance of a singly guarded Queen making is always much less than that for a singly guarded King; but you do not increase it by playing the Queen in such a case. You know certainly that either King or Ace lies with third and fourth players. If third holds either, your Queen falls. It may be, third player puts on King, and your partner takes the trick with the Ace, in which case you have the satisfaction of knowing that you have thrown away your Queen. Of course, if your partner holds Ace, and third player does not hold a King, you gain by putting on the Queen. But this is practically the only case in your favour. The matter may be put thus: One of the following arrangements *must* hold; as it is certain, from the lead of a small one, that leader has not both Ace and King:—

	By playing Queen,
3rd holds Ace and King	... You throw her away
4th „ „	... You save a trick
3rd „ „ 4th holds King or not	... You throw her away
3rd holds King, 4th holds Ace	... You lose a trick
„ „ „ not Ace	... You throw her away
3rd weak, 4th holds Ace	... You gain a trick
„ „ „ King	.. You probably gain a trick.

There is, you observe, a balance in favour of the play of Queen second-hand turning out unfavourably.

Cavendish gives a reason for playing the small card, the force of which some whist players seem not to recognise. He says that, playing the Queen unnecessarily exposes your weakness, and enables the leader to finesse when the suit is returned. As the Queen is the proper card to play if you hold King, Queen, and a small one, you expose your weakness to the player who holds King. If this is the leader, he can finesse the ten if he holds it or his partner leads it, on the return of the suit, knowing you do not hold the Knave. He may even finesse the nine if it suits him; a finesse against only one card. If the third player holds the King, he puts it on, and your weakness is exposed to both adversaries, who can finesse under equally favourable conditions. Of course, if fourth player holds the King, you only disclose your weakness to him; but that is one favourable case to two unfavourable cases. Manifestly, the play of Queen second hand from Queen one small one is unsound.

Next week we shall give, with remarks, inferences on a game singularly well illustrating the weakness of the lead from a singleton, even when the object is to secure only the odd trick, so that there is no question of bringing in a long suit.

G. THOMPSON.—Solution of Problem 2 quite right.

SOLUTIONS OF PROBLEM 3, by G. Nidroy, Moleque, D. Maxwell, W. K. Flett, F. D. Mackenzie, Wm. R. F., J. Harvey, M. Purvis (or Purvis), J. Pocklington, P. R. M., correct. All others wrong. There is no way of making all the tricks unless the Queen and Knave of Spades in A's hand are discarded.

LECTAS.—Pole's "Whist" published by Longmans. Clay's by De la Rue & Co. Our latest edition of Cavendish is the twelfth.

SOLUTION OF PROBLEM 3.

	A		Y		B		Z	
1st Trick	S	Ace	S	2	S	7	S	K
2nd „	H	Kn.	H	7	H	3	H	2
3rd „	H	6	H	8	H	9	H	5
4th „	S	Kn.	H	K	H	Ace	D	4
5th „		Qn.	D	2	H	4	D	7
6th „	C	5	S	3	S	10	D	8
7th „		3	S	4	S	9	C	Kn.
8th „	C	2	S	5	S	8	C	Qn.
9th „	D	5	C	4	H	Qn.	C	K
10th „	D	6			H	10	D	9*
11th „	D	Kn.	C	7	D	3	D	10
12th „	D	Ace	C	8	C	6	D	K
13th „	D	Qn.	C	9	C	10	C	Ace

The lead of Spade Ace, and the discard of Queen and Knave, are the "points" of this problem.—FIVE OF CLUBS.

Our Chess Column.

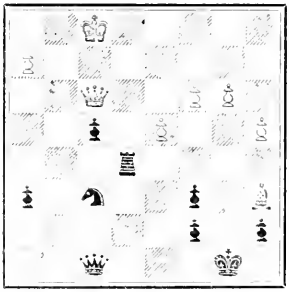
GAME BY CORRESPONDENCE.—(*Continued from p. 505.*)

Position after Black's 35th move.

R takes R.

CHIEF EDITOR.

WHITE.



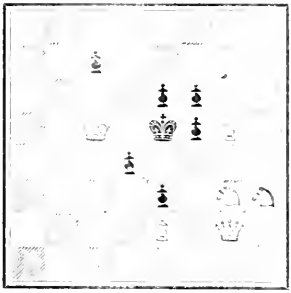
BLACK.

CHESS EDITOR.

36. Q takes QBP	36. R to KSch
37. K takes R	37. Q to Ksq ch
38. Q takes Q	38. Kt takes Q
39. K to B2	39. Kt to B3
40. K to B3	40. Kt to Q4
41. B to B1	41. Kt takes P
42. K takes P	42. P to QR4
43. K to B5	43. Kt to Kt 8
44. P to KR4	44. Kt to Q7
45. B to Q5	45. Kt to B8
46. K to Kt6	46. Kt to K6

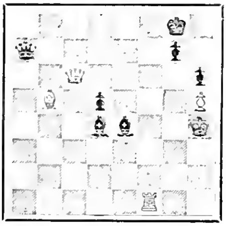
* If Z here plays his C Ace, B's C 10 is then best C, and he plays it before D 3, the result being the same.

PROBLEM No. 35.
By LEONARD P. REES.
BLACK.



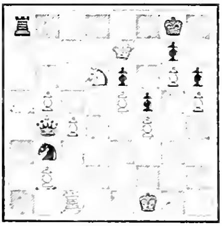
WHITE.
White to play, and mate in two moves.

PROBLEM No. 36. By ERCOLE DEL RIO.
BLACK.



WHITE.
White to play and mate in four moves.

PROBLEM No. 37. Example of "SMOTHERED MATE."
(Black received the odds of Queen's Rook.)
BLACK (Mons. Gretener).



WHITE (A. J. Maas)

White's last move was Q from KR4 to K7. Black then played Kt takes R. White now mates in five moves, beginning Q takes KPch. (If White checks at QB7, followed by Kt K8, Black can draw by perpetual check.

THE GIUOCO PIANO (*Continued from p. 442.*)

1. $\frac{\text{P to K4}}{\text{P to K4}}$ 2. $\frac{\text{Kt to KB3}}{\text{Kt to QB3}}$ 3. $\frac{\text{B to B4}}{\text{B to B4}}$ 4. $\frac{\text{P to B3}}{\text{Kt to B3}}$

Besides 5P to Q3 and 5P to Q4, which we have examined in our last article, White has four other moves at his disposal, viz., 5Kt to Kt5, 5 P to QKt4, 5 Q to Kt3, and 5 Castles. 5 Kt to Kt5 is the weakest of these continuations, but as it presents some danger to the inexperienced, we will examine it first :—

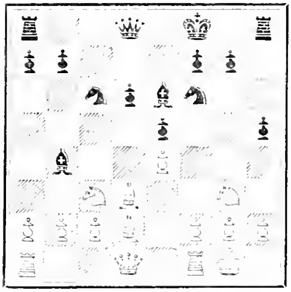
5. $\frac{\text{Kt to Kt5}}{\text{Castles.}}$ 6. $\frac{\text{P to Q3}}{\text{P to KR3}}$ 7. $\frac{\text{P to KR4}}{}$

This is a position which has occurred in thousands of games. The danger arises if Black should now capture the Knight with his Pawn, then White would win by having the command of his whole King Rook's file, e.g.,

7. $\frac{}{\text{P takes Kt}}$ 8. $\frac{\text{P takes P}}{\text{Kt to R2}}$ 9. $\frac{\text{Q to R5}}{\text{R to Ksq}}$ 10. $\frac{\text{Q takes BPch}}{\text{K to Rsq}}$

11. $\frac{\text{R takes Kt ch}}{\text{K takes R}}$ 12. $\frac{\text{Q to R5}}{\text{Mate.}}$ But, of course, Black would do well to play $\frac{}{\text{7 P to Q3,}}$ and he will soon obtain a good game. Should White play $\frac{\text{8 Q to B3}}{}$ then $\frac{}{\text{8 Q to K2,}}$ to be followed either by B to K3, or Kt to Kt5, in which latter case Black might possibly threaten to capture the Knight, provided the Queen cannot enter either on R5 or R3. This latter contingency will ultimately force the retreat of the White Knight from Kt5, again leaving Black with a superior position. Black might also play $\frac{}{\text{6 P to Q3}}$ at once, instead of $\frac{}{\text{6 P to R3,}}$ and he will also have a good defence. The danger of this manœuvre is by no means confined to this particular opening. As a very remarkable instance we here give the ending of a game which occurred in the Berlin International Tournament of 1881, between Herren Zukertort and Shallopp. The latter, as second player, adopted the Sicilian defence.

Position after White's 13th move (Castles).
BLACK.



WHITE.

White (Zukertort). Black (Shallopp).

Herr Shallopp proceeded with :—

	13. Kt to Kt5
14. P to KR3	14. Q to R5
15. P takes Kt	15. P takes P
16. R to Ksq	16. Kt to Q5

and White resigned, as after this move he had no defence. Black threatens Q to R7ch and Q to R8mate. If 17. QKt. to K2, with the object of interposing that Knight on Ktsq., to prevent the mate, then Black would reply with 17. Kt to B6ch. 18. P takes Kt. 18. P takes P, and again it is impossible to prevent the mate.

But in spite of this wonderful success, we have no hesitation in declaring this attack unsound, for had White delayed the capture of the Knight for a few moves only he might have played so as to provide protection for his King, which White thought was available after P takes Kt, overlooking Black's clever resource of Kt to Q5, which prevents the King escaping by way of K2. Thus, for instance, after

	13. Kt to Kt5
14. P. to KR3	14. Q. to R5

instead of taking the Knight at once he ought to have played

15. QKt to K2	15. B to QB4
16. P takes Kt.	16. P takes P
17. R to Ksq winning,	

as after 16. Q to R7ch, 17. K to Bsq, White now has a safe game, besides being a piece ahead.

ANSWERS TO CORRESPONDENTS.

*** *Please address Chess-Editor.*

Correct solutions received from Muzio, G. W., A. McDonnell, E. J. Winterwood.

C. P. and Henry Selby.—See revised problem.

E. A. Dillon.—Received with thanks.

Our Mathematical Column.

PROBABILITIES

By the Editor.

Let us next deal with some cases not altogether so simple as those hitherto considered.

Suppose that there is a lottery, in which there are more prizes than one, the prizes being unequal in value: how can we determine the value of a ticket?

Take a simple but definite case:

Suppose there are ten tickets, all equally likely to be drawn, and that there are three prizes, worth respectively £6, £3, *and* £1, *what is the value of a single ticket?*

The prizes are together worth £10. It follows that the ten tickets must together be worth the three prizes together, for any one buying all the tickets would get all the prizes and no more. Hence the ten tickets must together be worth £10, or (since they are all equal in value) must be worth £1, just the same price as when there is a single prize of £10.

And manifestly this is so in every case. It matters not how the prizes are distributed, the value of one among n tickets is always one nth part of the total value of the prizes.

It is probably this simplicity in lotteries of this kind, and the consequent obvious nature of the fraud when the total value of the prizes is less than the total amount received for tickets, which has caused those who have taken advantage of the weakness of human nature for gambling, to adopt various systems in which the swindling is as great, or greater, but is not quite so obvious. All the existing lottery systems, and all the gambling games carried on formerly at such places as Homburg and Baden, and now at San Marco, are so arranged that the luck may for a while run against the lottery holders or the "bankers" at roulette, rouge et noir, and the rest. The swindlers who thus encourage gambling can truly say that they take their chance of loss, and even of serious loss. They do lose at times, heavily; but in the long run they always come out right, the percentage of profit, estimated from mathematical considerations, is invariably attained.

Nay, these gambling rascals not only adopt systems by which they may occasionally lose, but they affect to allow privileges by which, as it seems to the inexperienced, they *must* lose. They allow that very system of wagering to which we referred some time since as one by which, to all appearance, one player must always win,—the system of doubling the stake after each loss until finally a win leaves a balance of gain as against several previous losses.

Let us take the simple case stated by us before, and see where the fallacy about sure gain lies:—

A *tosses a coin with* B, *staking* £1; *if he loses he stakes* £2; *if he loses he stakes* £4; *and so on, doubling each time until he wins, when he clears* £1. *And, as he must at length win, he can keep on adding pound to pound,* ad infinitum, *yet each separate wager* is *fair. Where is the fallacy?*

The fallacy resides in the supposition that A must at length win. He may go on doubling till he no longer possesses enough money to wager again on this doubling system, or till, having wagered more than he possessed, he is unable to pay. He is then ruined, and the process of adding pound to pound comes, perforce, to an end. In the long run, if B only has money enough as compared with A, this untoward event is bound to happen. If A and B have nearly equal capitals at starting the case is in some degree different: A may ruin B. But in the case of the bank at San Marco, or wherever else gambling may be pursued (of course mere coin-tossing is not the method but only illustrates the method), there are multitudes of small A's all risking their "small peculiar" against the possessions of one big (and very busy) B. They are inevitably absorbed separately if they are only possessed strongly enough by the gambling spirit.

Let us see what are the respective positions of A and B with regard to the prizes actually at stake in this case:—

At each venture A plays for £1, and we may consider that B stakes £1. If A's capital allows him to go on doubling ten times before he gives in, B plays for what A will have to pay him if he, A, is obliged to stop. The amount will be the sum of A's successive payments up to, and inclusive of, the tenth doubling, or

$$£1 + £2 + £4 + £8 + \ldots\ldots + £256 + £512 = £1,023.$$

The sum of the prizes is therefore £1,024; and there are 1,024 possible events, for there are 2 possible events at each tossing, and, therefore, 2^{10} possible events in 10 tossings. Therefore, at each venture (not at each tossing, but at each setting-off upon a series of tossings with constantly doubled stakes) B is practically in the position of one holding a ticket in a lottery of 1,024 tickets, each priced at £1, and a single prize of £1,024 (for note that we must not call the prize £1,023, any more than in the case of a fair lottery of 10 £1 tickets, we should call the prize £9, because that is all the winner really gains, £1 having been paid for his ticket). Or we may say that B is in the position of one who pays £1 for the chance of drawing one particular ball out of a bag of 1,024, £1,024 having to be paid him if he is successful. We know that in the long run—in a few millions of trials, for instance—he would draw successfully about one 1,024th the total number of trials. We know further that there would be times when he would be behind, and times when he would be ahead of this average. The times when he was ahead would be bad times for A. If B's capital enabled him to continue long enough he would be practically certain to ruin his opponent.

In their excessive desire to swindle the people, Governments which have encouraged lotteries have tried to devise the most attractive forms of wagering, submitting to mathematicians the due discussion of the probability problems involved. One of the most remarkable occasions of this kind on record is that which gave rise to what is called the Petersburg problem. It occurred to the Russian Government to start a lottery on the following plan:—

Each person who took part in it was to venture the same fixed sum of money £x, on the following conditions:—A coin is to be tossed until head appears; if head comes at the first toss the person is to receive £2; if at the second toss the person is to receive £4; if at the third toss, he is to receive £8; if at the fourth £16, and so on. The difficulty was to determine at what amount should x be fixed?

The answer given by mathematicians was not encouraging. We can form a tolerably clear notion of the sum we should care to risk on such a venture; and I suppose no one would be inclined to place that sum very high. If a lottery were actually established on such a principle as the above, and £10 or £12 were set as the value each person was to pay for his venture, very little business would be done at the price. But mathematicians asserted that if the value of x were set at *any sum, however large,* the "bank" would inevitably lose in the long run, supposing only that a sufficient number of ventures were made on such terms. For example, say the value assigned to x were £1,000, then, although it would be madness to risk such a sum on a single trial, yet if many millions of ventures were made, the bank would be immensely in arrears when a balance came to be drawn on the results of all the trials. The theoretical value of x is, in fact, *infinity*.

This curious paradox is described by Professor De Morgan as affording one of the most instructive lessons on the subject of probabilities.

I shall explain hereafter the reasoning by which the above seemingly paradoxical, but undoubtedly true, answer is obtained.

Problem.—Can the following be solved by elementary geometry? In a triangle given, $a + b$, $a + c$, and the angle A to construct the triangle?

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CONTENTS OF No. 25.

A STUDY OF MINUTE LIFE.

BY HENRY J. SLACK, F.G.S., F.R.M.S.

No. III.

AN old experiment that never fails to interest the observer is the revival of dry, seemingly dead, rotifers, by supplying them with water. The common rotifer (*rotifer vulgaris*) is a curious little creature, showing, as do nearly all its tribe, a very marked advance upon the infusoria described in former papers. When full-grown and stretched out, she—for it is the female that concerns us—is about $\frac{1}{24}$th of an inch long. The whole animal is very flexible. It can elongate itself, wholly or partially, by a process like sliding the tubes of a telescope, or can swell out laterally like a pear or a ball. From the head it can thrust forth cilia for swimming, and it can hold on by its tail-foot, and crawl like a caterpillar. For respiration and food-collecting, it puts out two groups of cilia, which, when in motion, have the appearance of revolving wheels, and hence it was called the "wheel animalcule." The so-called wheels produce strong currents in the water, and their whirlpool character is easily shown by putting in a little indigo or carmine. Rotifers are provided with elaborate organs, including a nervous system, and in the large Pitcher Rotifers (Brachions) a moderate magnification—say about fifty linear—is sufficient to show a brain mass in the head very conspicuously. An observer of the common rotifer, and of most others, is at once struck with the sight of energetic work going on in the creature's interior. The food particles are whirled down a gullet into an internal mouth, commonly called a gizzard, and this apparatus consists of several parts, two of which open and shut with a motion something like that which may be shown by putting two hands opposite each other, keeping the wrists in contact, and making the bent fingers alternately meet and separate. In the common rotifer, these biting parts are what Gosse calls *quadrantic*, being like two quarters of an apple, and furnished with crushing teeth to act upon the food before it passes into the stomach. This rotifer hatches its eggs internally, and the infant may be noticed inside its mother, working its mouth, or gizzard, before it leaves home and commences an independent life.

These remarks may be a sufficient prelude to the experiment of reviving these creatures from their torpid state. The first inquiry will be, how and where to find them. A small wisp of hay, covered with water in a gally-pot, and kept in a warm place, is pretty certain to exhibit some in the course of a few days, but lower forms of life will appear before them. The fine dust to be found in gutters on a dry day is very likely to contain some, and so is the dust that can be shaken from tufts of moss on roofs or trees. On a clear day in February, one such tuft, taken off the tiles, was shaken over a sheet of paper, and a minute pinch of its dust placed on a glass slide, in a drop of water, and covered with a piece of thin glass. A convenient slide for these purposes is made by cementing with shellac-glue on to an ordinary slide,* three thin slips of glass, about three-sixteenths of an inch wide, so as to form three sides of a small square. The slide thus prepared should be laid flat, a drop of water put in the middle of the square, and a covering-glass over it to rest upon the thin slips. This makes a shallow water-trough, and the cover is held sufficiently tight by the water it comes into contact with. The grain or two of dust obtained in the way mentioned exhibited no life of any kind for a few hours, but in the course of the day a couple of rotifers and one or two other things began to swim about. The water in these small troughs, being little exposed to the air, evaporates very slowly, and it is easy to keep up the supply by putting a drop with a camel's-hair pencil, so that it can run in. After looking for some time at the revivalists, the slide was put aside, and examined a few days afterwards.

It was quite dry again; the rotifers were discernible as little lumps, and to all appearances dead. A drop of water soon revived them, and this process may be repeated many times. The creatures stand the amount of drying they get under ordinary atmospheric conditions of evaporation, but they may be over-dried by heat, and then no moistening calls them to life again. The reports of various experimenters concerning the extent to which they may be dried without being killed do not agree. Dr. Carpenter, Lord Osborne, and others have found them uninjured by drying that made them quite brittle; while others, after reducing them to that condition, have not succeeded in restoring their animation. Probably the rate at which the drying occurs has much to do with the result. Slow drying is much less likely to cause any disruptive shrinkage, and it gives the little animals time to protect themselves from absolute dessication by a sort of mucus Mr. Davies has described.

When dried in the heat of a summer sun, and left as a dust particle in a gutter or elsewhere, their life work seems quite suspended, and in this they differ greatly from the bats and dormice in their so-called hybernation. These undergo no drying, though they lose fluid, and their vital work goes on, though at a slow rate. In one of Marshall Hall's experiments with a bat in the torpid state, its respiration consumed nearly three-and-a-half cubic inches of oxygen in sixty hours, and he remarked that in the dormouse and hedgehog the sense of hunger seemed to rouse them from hybernation, and that food conduced to a return of the lethargy. It is also found that a dormouse who is fat when the cold reduces him to torpidity, is lean when the winter is past, and the season for renewed activity arrives again. The mudfish passes into a state nearer that of the rotifer, when the hot sun of Africa bakes it in a mud-pie. The lethargy of hybernation and also that of heat and drought, enables animals to do without respiration to an extent that would be quite impossible in their

* Shellac dissolved in spirits of wine. Methylated will do if strong.

ordinary condition. Mr. Marshall Hall kept a lethargic hedgehog under water for 22½ minutes without injuring it, though 3 minutes' immersion killed another in the normal active state.

The rotifer when dried to a chip, and not killed, is much like a watch that has all its wheels sound, but the spring removed. All life processes require a high degree of mobility in the molecules of the living substance. Water supplies this condition, and those animals or plants that can survive desiccation differ from dead ones when they pass into the torpid state, in that their organic structure is not destroyed, and the chemical character of their albuminous matters is left in such a state that it can, by imbibing water, resume its ordinary activities. Life must not be regarded as some entity put into an organism, as wine is poured into a bottle, but as a complex series of actions and reactions which result in nutrition, growth, and reproduction. When we come to the higher phenomena of thought and feeling, science can only say that it has no explanation to offer as to the why and the how such manifestations are connected with molecular changes in brain and nerve.

THE GREAT PYRAMID.

By the Editor.

I COULD descant at great length on the value which the Great Pyramid, when in the condition represented at p. 315, and in the accompanying illustration repeated from p. 398, must have had for astronomical observation. I could show how much more exactly than by the use of any gnomon, the sun's annual course around the celestial sphere could be determined by observations made from the Great Gallery, by noting the shadow of the edges of the upper opening of the Gallery on the sides, the floor, and the upper surfaces of the ramps. The moon's monthly path and its changes could have been dealt with in the same effective way. The geocentric paths, and thence the true paths, of the planets could be determined very accurately by combining the use of tubes or ring-carrying rods with the direction lines determined from the Gallery's sides, floor, &c. The place of every visible star along the Zodiac (astrologically the most important part of the stellar heavens) could be most accurately determined. Had the Pyramid been left in that incomplete, but astronomically most perfect, form, the edifice might have remained for thousands of years the most important astronomical structure in the world. Nay, to this very day it would have retained its pre-eminence, provided, of course, that its advantages over other buildings had been duly supplemented by modern instrumental and optical improvements.

Unfortunately, the Great Pyramid was erected solely for selfish purposes. It was to be the tomb of Cheops, and whatever qualities it had for astronomical observation were to be devoted to his service only. The incalculable aid to the progress of astronomy which might have been obtained from this magnificent structure entered in no sort into its king-builder's plan. Centuries would have been required to reap even a tithe of the knowledge which might have been derived from Pyramid observations, and such observations were limited to a few years—twenty, thirty, forty, or fifty at the outside.

Now, while I am fully conscious that the astrological theory of the Great Pyramid is open to most obvious, and at the first sight most overwhelming objections, I venture to say not only that these are completely met by what is certainly known about the Pyramid; but that the astrological theory (combined, of course, with the tomb theory), is demonstrably the true explanation of all that had been mysterious in the Great Pyramid.

Take the chief points which have perplexed students of the Pyramids generally, and of the Great Pyramid in particular.

1. Granting the most inordinate affection for large sepulchral abodes, how can we account for the amazing amount of labour, money, and time bestowed on the Great Pyramid?

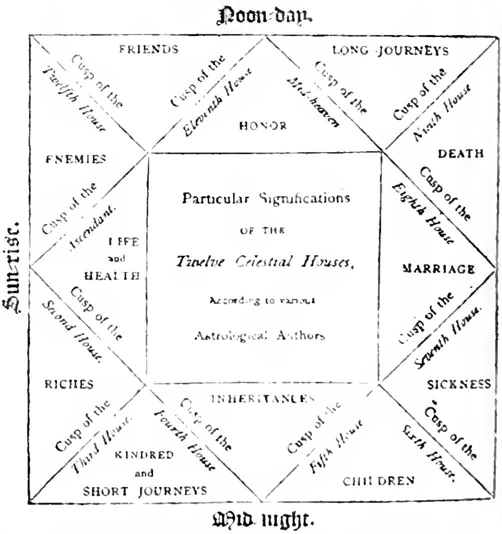
The astrological theory at once supplies the answer. If the builder believed what we know was actually believed by all the Oriental nations, respecting planetary and stellar influences, it was worth his while to expend that and more on the Pyramid, to read the stars for his benefit, and to "rule" stars and planets to his advantage.

2. If the Pyramids were but vast tombs, why should they be astronomically oriented with extreme care,—to assume for a moment that this is the only astronomical relation established certainly respecting them?

Astrology answers this difficulty most satisfactorily. For astrological study of the heavens, the Pyramid (in its incomplete or truncated condition) could not be too accurately oriented.

3. Granted that the Great Pyramid was for a time used as an astronomical observatory, and that its upper square platform was used for cardinal directions in the way shown in the figure, what connection is there between these direction lines (the only ones which would naturally arise from the square form) and astrological relations?

These lines remain to this very day in use among astrologers. The accompanying figure, taken from Raphael's Astrology (Raphael being doubtless some Smith, or Blodgett, or Higginbotham), represents the ordinary horoscope, and its relations (now unmeaning) to a horizontal, carefully-oriented square plane surface, such as the top of the Pyramid was, with just such direction-lines as would naturally be used on such a platform:—



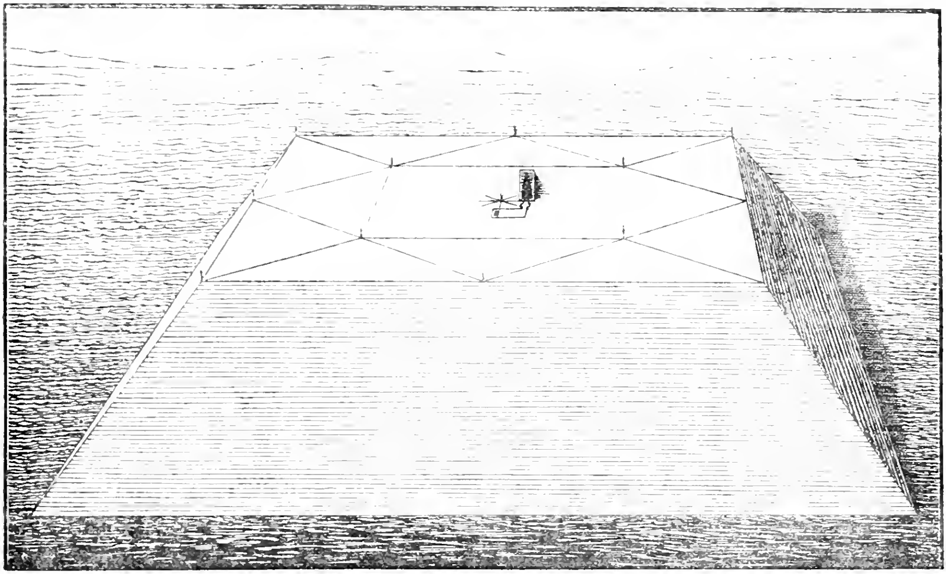
4. Why did each king want a tomb of his own? Why should not a larger family mausoleum, one in which all the

expense and labour given to all the Pyramids might have been combined, have been preferred?

Astrology at once supplies a reason. Dead kings of one family might sleep with advantage in a single tomb; but each man's horoscope must be kept by itself. Even to this day, the astrological charlatan would not discuss one man's horoscope on the plan drawn out and used for another man's. Everything, according to ancient astrological superstition, would have become confused and indistinct. The ruling of the planets would have been imperfect and unsatisfactory, if King Cheops' horoscope platform had been used for Chephren, or Chephren's for Mycerinus. The religious solemnities which accompanied astrological observations in the days when the chief astrologers were high priests, would have been rendered nugatory if those performed under suitable conditions for one person were followed by others performed under different conditions for another person.

on a smaller scale. Probably, the astrology of those days assigned the proper proportion in which the horoscope-platform for a son should be less than that for a father. It is noteworthy, at any rate, that the linear dimensions of the Pyramid of Asychis are less than those of the Pyramid of Mycerinus, in just the same degree that these are less than the linear dimensions of the Pyramid of Cheops.

6. It is certain that if Mycerinus had built his own Pyramid, he would have erected one larger, not smaller, than his father's, while Asychis would have made his Pyramid larger yet; whereas, as a mere matter of fact, the Pyramid of Asychis is utterly insignificant in size compared with the Pyramid of Cheops. The sides of the bases of the four Pyramids were roughly as follows: The Pyramid of Cheops, 760 feet; that of Chephren, 720 feet; that of Mycerinus, 330 feet; that of Mycerinus, 160 feet. The Pyramid of Cheops exceeds that of Asychis much more than 150 times in volume. It is not in accordance



5. How is it that the Pyramid of Chephren (Cheops' brother), though about as large, is quite inferior to the Pyramid of Cheops, the Pyramid of Mycerinus (Cheops' son) much smaller, and that of Asychis (Cheops' grandson) very much smaller, while to the younger sons and daughters of Cheops very small Pyramids, within the same enclosure as the Great Pyramid, are assigned?

The astrological answer is obvious. Cheops not only had full faith in astrology—as, indeed, all men had in his day—but his faith was so lively that he put it in practice in a very energetic way for the benefit of himself and dynasty. Chephren probably had similar faith. For the two brothers, separate Pyramids, nearly equal in size, were made, either at the command of Cheops alone, or with such sanction from Chephren as his (probable) separate authority required and justified. *At the same time*, and because his fortunes were obviously associated in the closest manner with those of his father and uncle, Cheops (or Cheops and Chephren) would have a Pyramid made for Mycerinus, but

with what we know of human nature to suppose that Asychis would have been content with so insignificant a version of his grandfather's Pyramid. Rather than that, he would have had no Pyramid at all, but invented some new sepulchral arrangement. Yet it adds enormously to the difficulties of the Pyramid problem to suppose that Cheops and Chephren arranged for the erection of all the Pyramids, or, at any rate, that the smaller Pyramids were raised to the horoscope-platform level during their lifetime.

Here, however, the astrological theory, instead of encountering, as all other theories do, a new and serious difficulty, finds fresh support; for this arrangement is precisely what we should expect to find if the Great Pyramid was erected to its observing platform for astrological observation and the religious observances associated with them. It is certain that with the ideas Cheops must have had (on that theory) of the importance of astronomical observations to determine, and partly govern, his future, he would not

have left him alone without their pyramidal horoscopes. Even if we suppose he entertained such jealousy of his brother Chephren, as Oriental (and some Occidental) princes have been known to entertain of their near kinsfolk and probable successors, that would be but an additional reason for having his brother's horoscope Pyramid erected on such a scale as the astrologers and priests considered suitable in the case of such near kinship. For by means of the observations made by the astrological priesthood from Chephren's horoscope platform, Cheops could learn, according to the astrological doctrines in which he believed, the future fortunes of his brother, and even be able to rule the planets in his own defence, where their configurations seemed favourable to Chephren and threatening to himself.

7. But it may be urged that, beyond the general statement that the Pyramids were intended as the tombs of their respective builders, we learn too little from ancient writers to form any satisfactory idea of their object.

It so happens, however, that the only precise statement handed down to us respecting the use of the Pyramids—not merely of the Great Pyramid, but of all the Pyramids—accords with the astrological theory in every detail, and with no other theory in any degree. For we learn from Proclus that the Pyramids of Egypt (which, according to Diodorus, had existed 3,600 years before his history was written, about 8 B.C.) terminated above in a platform, from which the priests made their celestial observations.

Observe how much is implied in this short statement:—

First, *all* the Pyramids had a use independent of their final purpose as tombs, a use, therefore, during the life-time of their future tenants, and presumably—one may say certainly—relating to the interests of those persons.

Secondly, this use was precisely such as we have been led to infer with all but absolute certainty, already, from the study of the Great Pyramid.

Thirdly, the astronomical observations were made by priests, and were therefore religious in character—a description which could only apply to astronomical observations made for astrological purposes. In all probability, the priests who made these observations professed a religion differing little from pure Sabaism, or the worship of the heavenly host. But it must be remembered that astrology was the natural offspring of Sabaism. Wherever we find an astronomical priesthood, there we find faith in astrology. But to say truth, Where among ancient Oriental nations was such faith wanting? The Jews had less of it than other Oriental nations, but they were not free from it. As they had all their religious observances regulated by the heavenly bodies, so they recognised the influence of the "stars in their courses." If they believed the heavenly bodies to be for "seasons" (of religious worship), and for "days and years," they believed them also to be for "signs." This also was the view of the ancient Chaldeans. "It is evident," says the late Mr. George Smith, "from the opening of the inscriptions on the first tablet of the Chaldean astrology and astronomy, that the functions of the stars were, according to the Babylonians, to act not only as regulators of the seasons and the year, but also to be used as signs, as in Genesis i. 14; for in those ages it was generally believed that the heavenly bodies gave, by their appearance and positions, signs of events which were coming on the earth."

In fine, while there is no other theory of the Pyramids generally, and of the Great Pyramid in particular, which has either positive or negative evidence in its form, the astrological theory is supported by all the known positive evidence; and strong though such support is, it derives yet greater strength from the utter failure of all other admissible theories to sustain the weight against them. There are difficulties in the astrological theory, no doubt, but they are difficulties arising from our inability to understand how men ever had such fulness of faith in astrology as to devote enormous sums and many years of labour to the pursuit of astrological researches, even for their own interests. Yet we know in other ways that astrology really was accepted in those days with the fulness of faith thus implied. While, however, the only serious difficulty in the astrological theory thus disappears when closely examined, the difficulties in the way of all other theories are so great, that, to all intents and purposes, they are not so much difficulties as impossibilities.

DOMESTIC VENTILATION.

A LESSON FROM THE COAL-PITS.

By W. Mattieu Williams.

The problem of domestic ventilation as compared with coal-pit ventilation involves an additional requirement, that of warming, but this does not at all increase the difficulty, and I even go so far as to believe that cooling in summer may be added to warming in winter by one and the same ventilating arrangement. As I am not a builder, and claim no patent rights, the following must be regarded as a general indication, not as a working specification, of my scheme for domestic ventilation and the regulation of home climate.

The model house must have an upcast shaft, placed as nearly in the middle of the building as possible, and communicating with every room, either by a direct opening or through a lateral shaft. An ordinary chimney built in the usual manner is all that is required.

There must be no stoves nor any fire-places in any room excepting the kitchen, of which anon. All the windows must be made to fit closely, as nearly air-tight as possible. No downcast shaft is required, the pressure of the surrounding atmosphere being sufficient. Outside of the house, or on the ground-floor, on the north side, if possible, should be a chamber heated by flues, hot air, steam, or water pipes, and with one opening communicating with the outer fresh air, and another on the opposite side connected by a suitable shaft or airway with the hall of the ground-floor and the general staircase. Each room to have an opening at its upper part into the chimney, like an Arnott's ventilator, and capable of adjustment as regards area of aperture, and other openings of corresponding or excessive combined area leading from the hall or staircase to the lower part of the room. These should be covered with perforated zinc or wire gauze, so that the air may enter in a gentle, broken stream.

All the outer house doors must be double, *i.e.*, with a porch or vestibule, and only one of each pair of doors opened at once. These should be well fitted, and the staircase air-tight. The kitchen to communicate with the rest of the house by similar double doors, and the kitchen fire to communicate with the upcast shaft or chimney by as small a stove-pipe as practicable. The kitchen fire will thus start the upcast and commence the draught of air from the warm chamber through the house towards the several openings into the shaft. In cold weather, this upcast action will be greatly reinforced and maintained by the general warmth of all the air in the house, which itself will bodily become an upcast shaft immediately the inner temperature exceeds that of the air outside.

But the upcast of warm air can only take place by the admission of fresh air through the heating chamber, thence

to hall and staircase, and through the rooms into the final shaft or chimney. The openings into and out of the rooms being adjustable, they may be so regulated that each shall receive an equal share of fresh warm air; or, if desired, the bed-room chimney valves may be closed in the daytime, and thus the heat economised by being used only for the day rooms: or *vice versâ*, the communication between the upcast shaft and the lower rooms may be closed in the evening, and thus all the warm air be turned into the bed-rooms at bed-time. If the area of the entrance apertures of the rooms exceeds that of the outlet, only the latter need be adjusted; the room doors may, in fact, be left wide open without any possibility of "draught," beyond the ventilation current.

So far for winter time, when the ventilation problem is the easiest, because the excess of inner warmth converts the whole house into an upcast shaft, and the whole outer atmosphere becomes a downcast. In the summer time, the kitchen fire would probably be insufficient to secure a sufficiently active upcast. To help this there should be in one of the upper rooms—say an attic—an opening into the chimney secured by a small well-fitting door, and altogether enclosed within the chimney, a small automatic slow combustion stove (of which many were exhibited at South Kensington, that require feeding but once in twenty-four hours), or a large gas-burner. The heating-chamber below must now be converted into a cooling-chamber by an arrangement of wet cloths presently to be described, so that all the air entering the house shall be reduced in temperature. Or the winter course of ventilation may be reversed by building a special shaft connected with the kitchen fire, which, in this case, must not communicate with the house shaft. This special shaft may thus be made an upcast, and the rooms supplied with air from above down the house shaft, through the rooms, and out of the kitchen *viâ* the winter heating-chamber, which now has its communication with the outside air closed.

Reverting to the first-named method, which I think is better than the second, besides being less expensive, I must say a few concluding words on a very great supplementary advantage which is obtainable wherever all the air entering the house passes through one opening, completely under control, like that of our heating-chamber. The great evil of our town atmosphere is its dirtiness. In the winter it is polluted with soot particles; in the dry summer weather, the traffic and the wind stir up and mix with it particles of dust, having a composition that is better ignored, when we consider the quantity of horse-dung that is dried and pulverised on our road-ways. All the dust that falls on our books and furniture was first suspended in the air we breathe inside our rooms. Can we get rid of any practically important portion of this?

I am able to answer this question, not merely on theoretical grounds, but as a result of practical experiments. On March 19, 1879, I read a paper at The Society of Arts, recommending the enclosure of London back-yards with a roofing of "wall canvas," or "paper-hanger's canvas," so as to form cheap conservatories. This canvas, which costs about threepence per square yard, is a kind of coarse, strong, fluffy gauze, admitting light and air, but acting very effectively as an air filter, by catching and stopping the particles of soot and dust that are so fatal to urban vegetation. I made a series of experiments, which are described in the Journal of the Society, March 21, which proved this filtering action, and after these, when my paper was announced, was told that similar experiments had been made in the Houses of Parliament. I went there accordingly, and obtained some very interesting information from Mr. Prim, the assistant engineer to Dr. Percy, who superintends the ventilation arrangements of the whole building.

There I found that, after trying many materials, they had finally selected the same as I had, but were using it rather differently. The air supplied to the building is passed through a succession of screens of this material, all kept moist by the trickling of water over them. In the summer, the outer air is thus cooled as well as filtered. The effectiveness of the filtration is proved by the fact that the screens become so clogged with sooty abominations, that they have to be regularly washed once a fortnight, and the water in which they are washed becomes of inky blackness.

I propose, therefore, that this well-tried device should be applied at the entrance aperture of our heating chamber, that the screens shall be well wetted in the summer, in order to obtain the cooling effect of evaporation, and in the winter shall be either wet or dry, as may be found desirable. The Parliament House experiments prove that they are good filters when wetted, and mine that they act similarly when dry.

By thus applying the principles of colliery ventilation to a specially-constructed house, we may, I believe, obtain a perfectly controllable indoor climate, with a range of variation not exceeding four or five degrees between the warmest and the coldest part of the house, or eight or nine degrees between summer and winter, and this may be combined with an abundant supply of fresh air everywhere, all filtered from the grosser portions of its irritant dust, which is positively poisonous to delicate lungs, and damaging to all. The cost of fuel would be far less than with existing arrangements, and the labour of attending to the one or two fires and the valves would also be less than that now required in the carrying of coal-scuttles, the removal of ashes, cleaning of fire-places themselves, and the curtains and furniture they befoul by their escaping dust and smoke.

It is obvious that such a system of ventilation may even be applied to existing houses by mending the ill-fitting windows, shutting up the existing fire-holes, and using the chimneys as upcast shafts in the manner above described. This may be done in the winter, when the problem is easiest, and the demand for artificial climate the most urgent; but I question the possibility of summer ventilation and tempering of climate in anything short of a specially-built house or a materially-altered existing dwelling. There are doubtless some exceptions to this, where the house happens to be specially suitable and easily adapted, but in ordinary houses we must be content with the ordinary devices of summer ventilation by doors and windows, plus the upper openings of the rooms into the chimneys expanded to their full capacity, and thus doing, even in summer, far better ventilating work than the existing fire-holes opening in the wrong place.

I thus expound my own scheme, not because I believe it to be perfect, but, on the contrary, as a suggestive project to be practically amended and adapted by others better able than myself to carry out the details. The feature that I think is novel and important, is that of consciously and avowedly applying to domestic ventilation, the principles that have been so successfully carried out in the far more difficult problem of subterranean ventilation, in which I have had some practical experience.

Eclipse Map of Egypt.—This map, promised for the present number, will be given next week. It seemed desirable to the Editor to supplement the mere track of the shadow's centre with the elliptical shadow outlines for each of nine stations indicated along the track. A figure will also be given explaining the simple geometrical construction for determining the shape and position of this elliptical shadow, as well as the position of the sun in the sky at the time of central eclipse at each station.

FOUND LINKS.

By Dr. Andrew Wilson, F.R.S.E., F.L.S.

PART VI.

THE quadrupeds, or Mammals, as they are technically called, form an important group of animals, not only because in structure they represent the perfection of the animal world, but because they stand at the head of the animal creation, apparently separate and distinct from all other and lower classes. The distinctive nature of the quadrupeds, in fact, has been tacitly acknowledged in zoology in the systems of classification which themselves are mere expressions of the varied relationships of the classified beings. For, whilst the fishes and frogs have been united to form a province of Vertebrate animals, and whilst reptiles and birds have also been arranged in one chief group by reason of their affinities, the quadrupeds have been made to form a province by themselves. The hairy nature of the body covering, the nourishment of the young by means of milk, the fact that the young are born alive, and many other characters well known in popular zoology, attest the distinctive nature of the highest group of animals.

But whilst these statements cannot be questioned, it must not be imagined that the quadrupeds are thereby entirely separated from all other animals. On the contrary, they possess their own affinities with lower forms, such as evolution pre-supposes, and such, indeed, as that theory of nature demands. The lowest mammals, to begin with, are by no means like the higher quadrupeds; and it is in the lowest confines of the class, as we shall presently see, that the approach to lower animals is made. The warmth of blood so characteristic of quadrupeds has already made its appearance in the birds, and although the exact origin of the mammals is yet a matter of doubt, it seems pretty clear that the root stock of the class to which man himself belongs, may be sought for in some common territory whence, from a half-bird type, the lowest quadrupeds arose, or whence the mammals on the one side, and birds and reptiles on the other, have independently arisen. Such a conclusion seems to be that at present supported by facts as they stand; and although further research may modify this view, there will still exist the demand for the links that bind the quadrupeds to their lower Vertebrate neighbours.

There can exist, at least, no doubt of the remarkable likeness which the lowest quadrupeds present to the bird and reptile groups. To understand thoroughly the zoological aspects of the matter, I may remind the reader that the class of mammals is very sharply split into two main divisions. These, to avoid technicalities, we may term Higher and Lower Mammals. The former group includes forms ranging from man downwards through the apes, bats,

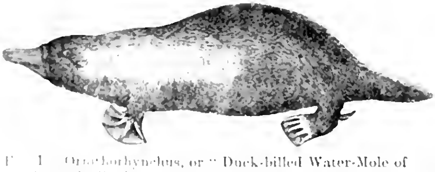


Fig. 1.—Ornithorhynchus, or "Duck-billed Water-Mole of Australia" (showing the "bill" and webbed feet).

rodents, and hoofed quadrupeds, to the whales, sloths, anteaters, and their kith and kin. These animals are distinguished by the higher brain structure and by the general possession of all the typical characters of quadrupeds. The Lower Mammals are the Ornithorhynchus, or "Duck-billed Water Mole" of Australia (Fig. 1), and its neighbours the Echidnas or "Porcupine Anteaters" of Australia; these two genera forming the lowest order (*Monotremata*) of all. A little above them, but still shut off from the higher ranks of the class, are the kangaroos, wombats, phalangers, &c. in a word, the whole *native* population of Australia (along with the New World opossums), forming the order of *Marsupialia*, or that of the "pouched" quadrupeds. In Fig. 2 is represented the *pelvis* or haunch-bones of a kangaroo. At *a*, *a*, the *Marsupial bones*, or those supporting the well-known pouch, are seen. These bones are only found in the Marsupials and Monotremes, and whilst in most of the former they support a pouch, they are never associated with that structure in the Monotremes.

Now, it is in the Monotremes—represented by the *Ornithorhynchus* (Fig. 1) and the *Echidnas*—that the characters linking quadruped life to lower life are most typically seen. It may be well to strengthen our position at the outset, by reminding the reader that in the early life of *all* quadrupeds, without exception, there are to be perceived evidences of their connection with lower forms of life. Thus, every Vertebrate, at an early stage of its development, exhibits certain clefts or openings in the sides of the neck, known as branchial clefts, and which are bounded by

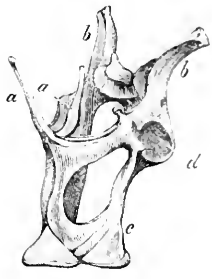


Fig. 2.—Haunch bones of Kangaroo: *a*, *a*, marsupial bones; *d*, socket for thigh-bone.

folds called branchial arches. These, in fishes, come to bear the gills, but in reptiles, birds, and quadrupeds they simply disappear—useless rudiments of structures, once necessary in the life of aquatic quadruped-ancestors, and still retained in the developments of to-day by the law of inheritance. Thus, in the development of a rabbit, the biologist sees three pairs of branchial arches behind the mouth of the embryo, and four branchial clefts. Three of the clefts disappear, and the fourth, by the modification to which development has been subjected in the evolution of the quadruped tribes, is converted into the Eustachian tube and other structures belonging to the ear. The presence of "branchial clefts" in the developing mammal would alone suffice to show its evolution from lower life. Denying that probability, which to the biologist is a fact, there is no explanation whatever of the cause or existence of these vanishing structures in the history of the quadruped race.

Concentrating our attention on the "Monotremes" themselves, however, we may speedily discover numerous links which unite them with lower life, and specially with the bird-type. There, firstly, exists in these quadrupeds what Huxley has called "a striking feature" of reptiles, of birds, and of the frog-class as well, in the structure of the shoulder. In the shoulder of an ordinary quadruped, and of a kangaroo and its marsupial race as well, there are

but two distinct bones. One of these is the shoulder-blade, or *scapula*, the other being the collar-bone, or *clavicle*. In the shoulder of a bird (Fig. 3) there are three distinct elements, the *scapula* (Fig. 3, *d*) *clavicle* (*c*), and the *coracoid bone* (*b*,*b*.) This last in quadrupeds, a mere process of the shoulder-blade, forms, as shown in Figure 3, the chief support of the wing in birds, and arises directly from the breastbone (*a*). Now, it is a remarkable fact that the Ornithorhynchus and Echidna, alone of all quadrupeds, possess a distinct *coracoid bone*, which, as in birds and reptiles, springs from the breastbone. Again, there is another bone, called the *epicoracoid*, which is found in reptiles, and which exists likewise in the

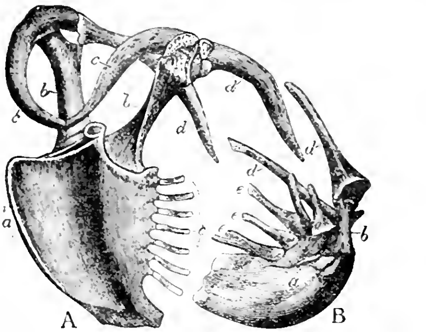


Fig. 3.—Shoulder-bones of (A) an Eagle, and (B) an Ostrich.

Ornithorhynchus and Echidna. In the bird, again, as everybody knows, the two collar-bones unite to form the "merrythought," or *furculum* (Fig. 3, *c*). In these lowest quadrupeds the collar-bones (Fig. 4, *cc*) are joined by a T-shaped bone, called the *interclavicle* (Fig. 4, *i*), unknown

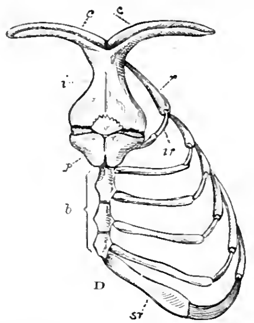


Fig. 4. Shoulder-bones of Ornithorhynchus: *c*, *c*, clavicles; *i*, interclavicle; *b*, breast-bone; *sr*, sternal or breast-bone ribs, as in birds.

in any other mammals; and here, again, we find a character which is decidedly reptile-like and bird-like, and which is not seen in other mammals. Professor Flower tells us that the shoulder-girdle of these quadrupeds "differs widely in many points from that of any other mammal, and far more resembles that of the Lizards."

The jaws of the Ornithorhynchus are prolonged to form a flattened horny bill (Fig. 1), on the upper aspect of which the nostrils are seen. It is the possession of this bill which has given origin to the name "duck-billed," applied to this animal. The Echidna possesses no such structure, but has simply a flexible snout.

There also exist in the internal anatomy of these curious animals certain characters which relate them to the birds and reptiles. For example, the bones of the head are firmly ossified together, as in birds, and the *sutures*, or lines of union of the skull-bones, do not persist, as is usually the case in quadrupeds, whilst the hollow of the haunch-bones (Fig. 2, *d*), in which the head of the thigh works to form the hip-joint, is not fully ossified, and thus comes to resemble the similar structure in birds and crocodiles. The ears of these lower quadrupeds differ from those of other mammals in not possessing a spiral arrangement of that part of the organ named the *cochlea*. As in very many reptiles, the upper, or front, or neck-ribs of these quadrupeds long remain as separate bones; and the same remark holds good of the curious little pivot (*odontoid process*) on which the head turns. This pivot in quadrupeds is firmly joined to the second bone of the neck: but in the "Monotremes," as in reptiles, it remains separate and distinct till a very late period—if, indeed, it becomes ossified at all.

The internal anatomy likewise reveals characters of bird and reptile life which can only be alluded to here. The arrangements of the internal organs in many respects present the closest likeness to the anatomy of birds and reptiles, and this is particularly the case with those structures in the quadrupeds which represent the egg-producing organs of the bird and reptile. Even the typical mammalian characters are but feebly represented in these lowest quadrupeds. We know that the young, although born alive, as in quadruped life at large, are provided with a horny knob on the upper jaw, such as is seen in the young bird; and no teats exist in the milk-glands of these forms, a feature represented in all other members of their class.

Summing up the inferences to be drawn from our brief study of the lowest quadrupeds, we may legitimately hold, firstly, that they are of essentially lower structure than other mammals: secondly, that all the points in which they evince this inferiority ally them, at the same time, to birds and reptiles; and thirdly, that the only feasible explanation of the differences in question is that which regards them as arising from the nearer relationship—the result of heredity and descent—which these lowest quadrupeds present to birds and reptiles.

In a concluding paper, I shall strive to show the nature of the links which unite the Vertebrate animals to their lower and Invertebrate neighbours.

CAMBRIDGE AND OXFORD STYLES.

(To an old Club Captain.)

DEAR SIR,—As the stroke oar of the winning boat in the Oxford and Cambridge race, 1845-46, and, I believe, the introducer of the slow recovery by which both races were won (though I can really, however, say that only of the first, for, in the latter race Milman pulled very nearly my stroke), I wish to say how much I approve of all you have written in Knowledge: and to remark that Cambridge loses now because they both hurry forward and go too far back—at least, this latter is often a fault with them. The rapid recovery is always a fault, and, I think, the most exhausting fault that can be made either in the old boat or the new boat, but very particularly so in the new boat.—Yours faithfully, C. G. Hill.

Reviews.

THE PROPERTIES AND MOTIONS OF FLUIDS.*

MR. STANLEY has taken as the subject for this work, having first made it the subject of experimental research, the properties and motions of fluids, an inquiry of extreme difficulty, and he has treated it with great skill and acumen. Anyone who has studied the mathematics of pneumatics, hydrostatics, and hydrodynamics, and has compared the processes and results with those used and obtained in the application of mathematics to optics or astronomy, knows how very far from exactness is all our knowledge of the former subjects, and will recognise the justice of Sir J. Herschel's remark, that "if there be one part of dynamic science more abstruse and unapproachable than another, it is the doctrine of the propagation of motion in fluids." Even the doctrine of the tides, supposed by many to have been thoroughly established since the time of Newton, is as yet perplexed by the difficulties belonging to all discussions of fluid motion.†

It would be an injustice to Mr. Stanley to attempt to give anything like an abstract of its contents, for the simple reason that though the book is no small one, it presents a closely-reasoned account of the experiments made by the author, and of the results to which they have led. As the experiments actually made were in most cases nearly ten times as numerous as those described, we may regard the book as itself an abstract.

The first three chapters are speculative, and in part hypothetical; they are generally attempts to apply mechanical principles to hydrostatics, and "needed," Mr. Stanley says, "more leisure on some points than he could command." In the second chapter, the theory that liquids have tensile surfaces is opposed, the author's experiments showing, in his opinion, that the surfaces of fluids are extensile, except in the case of free films, which are clearly tensile in consequence of the position of the attractive matter composing them. Some of the experiments illustrating this chapter can be very easily tried, and would be of great interest to the student. For instance, here is an experiment originally made by Descartes. An ordinary

Fig. 1.

sewing needle placed on the surface of still water floats in apparent contact with the water (see Fig. 1, presenting an enlarged view of a section of the needle, and showing how a bulk of water about eight times the volume of the needle is displaced, so that the needle lies in a trough of the deflected water. The conditions for making this experiment most satisfactorily are described by Mr. Stanley,—in particular, the surface of the needle should be perfectly clean and free from the slightest speck of rust. Then there is a pretty modification of the experiment. Take a polished wire an inch long and about one twenty-fifth of an inch in diameter, well cleaned with potash liquor, and wiped on a clean cloth. Carefully attach to each end, upon one side of the wire, a fibre of cotton silk, by means of shellac varnish. Place the suspended wire in the centre of a vessel, and pour in water till the wire is nearly reached. If now we take a small syphon of glass, filled with water, and place one end deep into the vessel already described, and the other end in another vessel containing water; then by raising or lowering the second vessel we may very slowly raise or lower the water to or from the suspended wire. If the water be raised, the wire will float as in the former experiment. If we lower the water, the wire as it

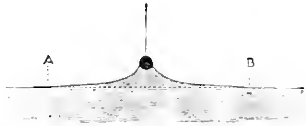


Fig. 2.

comes above the surface will draw the water with it. When the elevation is at a certain point, the same form of curvature, but inverted, will be produced as was seen in the depression.

Another experiment, illustrated in Fig. 3, is important. The figure explains itself, only it will be understood that there must be a can of water from which a small caoutchouc tube supplies the jet:—

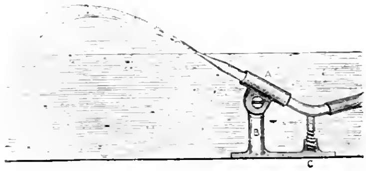


Fig. 3.

Height of projection of free jet in air $1\frac{1}{2}$ inch, distance 5 inches. Immersing the jet for an instant, and then restoring it to its place, so as to carry up a film of water: height of projection with this film $\frac{1}{2}$ an inch, distance $1\frac{1}{4}$ inch.

The third chapter includes experiments illustrating the passage of water through various apertures and passages; also a discussion of the passage of water past a peg or post in a flowing stream, showing how the current divides before reaching the place of absolute resistance

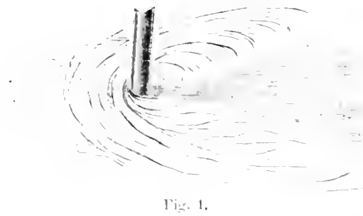


Fig. 4.

(Fig. 4). This illustrates, Mr. Stanley believes, the division of the great Atlantic equatorial current 300 miles before it reaches Cape St. Roque.

But we would direct the reader's special attention to Chapter IV., in which the theory of rolling contact of part of a fluid system is discussed, a theory bearing in a most

* "Experimental Researches into the Properties and Motions of Fluids, with Theoretical Deductions Therefrom." By William Ford Stanley. (E. & F. N. Spon, London.)

† The explanation amusingly given in our books of geography and elementary astronomy is about as valuable as the statement that a top held aslant will fall if let go, regarded as an explanation of the fact that a spinning top will not fall though its axis be aslant. It is simply an imperfect account of the statical theory, according to which there would be high water under the moon and opposite, whereas, according to the true theory, there would be high water there but for frictional effects.

instructive manner on the subject of oceanic currents. Chapter V., also, dealing with the principles of resistance in fluids, is of extreme interest. In one of the experiments illustrating this part of the subject, a leaden bullet fired directly against a thin parchment film covering a small water surface is found to be shattered into fine films, which the author arranges in the following form :—

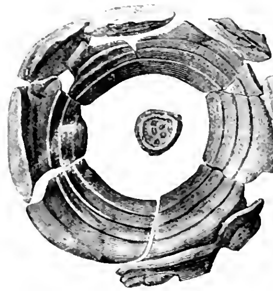


Fig. 5.

In the sixth chapter, Mr. Stanley discusses the generation of bi-whirl systems under the action of conic resistance, while in Chapter VII. we have an interesting series of experiments with pipes and channels. Chapter VIII. deals with the important subject of the projection of solids in fluids, showing how the solid, carrying forward with it a volume of fluid, acts like a fluid projected in a fluid. The author considers this principle further supported by an experiment by Mr. J. Scott Russell, in which a boat stopped in a canal was found to project forward a volume of water greater than its bulk, this projected water forming a wave half-a-yard in height, which Mr. Russell followed on horse-back to a distance many miles from the stopped boat. Mr. Stanley thus represents (as experiment has established) the forms of the following whirls in this case.

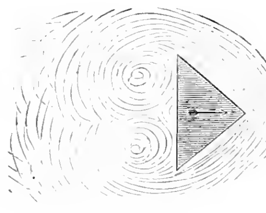


Fig. 6.

His application of this part of his research to the theory of rudder action is full of interest.

Passing over other valuable experiments, we come to the second section, to which, however, we can give space very inadequate to its real importance. Mr. Stanley here considers the general conditions of fluids on the globe under the influence of the sun's heat and the earth's revolution. He erroneously describes, by the way, as Dr. Carpenter's theory of oceanic circulation that according to which the chief motive force of the system of oceanic circulation is the sun's heat in tropical regions. This theory had been adopted before Dr. Carpenter advanced his views, the special point of which is the action of cold water descending from melting ice in the arctic regions. We believe there can be very little doubt that solar action on the equatorial and tropical seas is altogether more potent than any other cause—melting ice, trade winds, or the like—in generating and maintaining the system of oceanic circulation. Mr. Stanley's researches admirably illustrate this subject, and we would specially recommend this portion of Mr. Stanley's work to the careful study of all who take interest (as who does not?) in the wonderful mechanism of the ocean currents.

The discussion of aerial circulation is also full of interest, though of necessity this part of the subject is less fully illustrated by experiments than those which precede it. We wish our space would permit us to present a full account of what Mr. Stanley says in these important portions of his work. It would be an injustice to him to give a mere abstract.

We cannot too warmly commend Mr. Stanley's book to all interested in the discussion of fluids and their movements, in open sea, in rivers and canals, and in more limited spaces. He has dealt, and dealt successfully, with some of the most difficult of hydrodynamical problems.

MEAT PRESERVATION.

THE second part of a demonstration of a new method (under Jones's patent) of meat preservation was given on Friday at the Cannon-street Hotel, when the efficacy of the process was proved in a practical way by the serving for luncheon of mutton killed on March 6, or thirty-nine days before. The carcases from which the joints served up were cut had been kept in a butcher's shop at a temperature varying between 50° and 60° Fahr. Mr. Hardwicke, who presided, remarked, on sitting down to lunch, that the meat would be found to have been prepared in the simplest way, in order that the flavour and quality of the flesh might be better appreciated. The experiment showed that the mutton, of which boiled and roasted joints were served, was perfectly free from any taint or taste of the antiseptic chemical used to preserve it, and that the meat retained its natural juiciness and flavour. It was, moreover, very tender. The process, which was described in the *Times* of March 9, differs from other applications of antiseptics to the preservation of meat in one important feature—the preservative substance (boracic acid) is injected into a vein while the creature, though stunned by a blow on the head, is still alive, and the action of the heart is relied upon to pump it through every part of the body into which the vascular system ramifies. With regard to a question which has been raised as to the innocency of boracic acid as an antiseptic, Colonel Harger quoted Dr. F. P. Atkinson, who says:—"Considering the well-known properties of boracic acid, it is exceedingly curious how little it has been administered as an internal remedy. Its effect in diphtheria, both locally and internally, is very marked." This he proceeds to show by reference to observations of Dr. Cossar Ewart and Dr. Malcolm Simpson, and subsequently states that, "A dose is 5 to 15 grains. It has one particular recommendation, and that is its tastelessness." In the room was shown the carcass of a sheep killed on the 6th of March last, still in perfect condition, and to preserve which 5 oz. of boracic acid were used, the whole carcass weighing 74 lb. The two hind-quarters of another sheep, killed on the 20th of February, or 54 days ago, were also shown. No difference was perceptible between the condition of one or the other. To preserve the latter, which weighed 89 lb., 6 oz. of boracic acid had been used. As, however, a large proportion of the solution injected probably flows away with the blood when the creature is struck by the butcher, two minutes after the injection of the boracic acid, it is impossible in the absence of any *data* from careful quantitative analysis to calculate the quantity remaining in the fibre of, say half-a-pound of meat before cooking. But small as this quantity must be, there can be no doubt that, introduced in this way into the living organism it suffices to preserve, not only the carcass, but also the heart, liver, kidneys, and other organs of the body. The economical advantages of a method which, if used in the *abattoirs* of Sydney or Melbourne, would only render it necessary to reduce the temperature in the storing-rooms during the voyage to 50 deg. Fahr., instead of 30 deg. Fahr., must evidently be considerable. The loss of weight in a sheep thus preserved and kept for one month has been found to be about 5 per cent.—*Times*.

A NEW use has been found for graphite in the shape of paint to protect articles of iron, notably roofs, bridges, smoke-stacks, &c., against rusting and corrosion.

SOLAR MOTION AND COMETS FROM OUTSIDE.

AN interesting question is suggested by the approach of the coming comet, one that Mr Proctor can elucidate to the readers of KNOWLEDGE, and thereby solve a problem that has probably suggested itself to others besides myself. It is this:—Supposing that this comet (or any other) is a visitor to our solar system from the outer regions of space, and is now moving in a direction that will bring it within, say, 10 millions of miles of the present position of the sun by the middle of June, where will it be in relation to the sun at that date, assuming that the sun is travelling through space with his regular attendants at the rate of nearly 500,000 miles per day? One of the components of the comet's orbit, the solar gravitation, will, of course, change its direction with that of the sun, but the other, the comet's original motion, remains of its original component value.

How do astronomers deal with this question, and does the course of such a comet verify the theory of solar motion in space?

W. Mattieu Williams.

The astronomer can only judge of a comet's course by observations made from the earth, which shares whatever motion the sun may have in space. He finds the comet's orbit relatively to the sun to be such and such, and whether the sun be at rest or in motion, the movements of the comet with respect to the sun and solar system will be the same. If, however, we imagine an observer in space knowing the exact course a comet is pursuing, that is the exact direction and rate of the comet's motion, and knowing also the exact position of the sun, but not knowing anything about the sun's motion in space, it is certain that the calculations of such an observer as to the comet's position at any given future time, would be altogether incorrect if he assumed the sun to be at rest, and the sun is really moving very rapidly through space. He would make no correction for that part of the comet's motion at the moment, which is equal and parallel to the solar motion, and which, therefore, must be regarded as removed before the computation of the comet's course with respect to the sun can be taken into account. Thus, suppose the comet moving from A to C (Fig. 1) in space, while the sun is moving from S to S' in the same time (if both continued on an unchanging course), and let CB be parallel and equal to SS'. Then our imagined observer would calculate for the velocity and direction represented by AC, and get a very different orbit round S than he would if, knowing of the sun's motion, he took the right velocity and direction, viz., that represented by the line AB. The observer on earth cannot make this mistake, for whether SS' be large or small, and whatever its direction, the terrestrial observer can only recognise the velocity and direction AB, that is the comet's actual velocity and direction, corrected for the sun's.

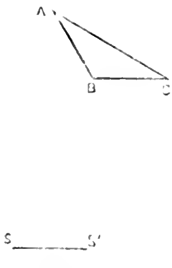


Fig. 1.

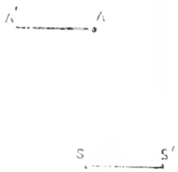


Fig. 2.

Another difficulty which is, I think, commonly experienced, is this. If we imagine a comet leaving the domain of a sun moving in one direction, or perhaps at rest, and coming within the effective influence of our sun moving in another direction, how does the swift motion of the sun away from the position it had had at first with respect to the comet, affect the comet's eventual course? In reality, this is the same difficulty as the other. It matters not where the comet is, whether it has the motion represented by AC in our figure, or whether the motion was originally acquired, or what the direction and amount of that motion may be; that motion, corrected into motion AB by the application of a motion CB parallel and opposite to the sun's, is the motion determining the comet's course with respect to the sun. Of course, when the comet is equally, or nearly equally, attracted by the sun it is leaving, and by our own sun, the former attraction must be taken duly into account in estimating the future course of the comet. But suppose our sun, for a moment, the only sun in existence, and imagine that a comet is placed at rest at A (Fig. 2), and left free to yield to solar attraction. How will it behave if the sun, instead of being at rest at S, is moving in direction SS', with velocity represented by the length SS. We apply to the comet the motion AA', equal and parallel to SS', but in the opposite direction, and so learn the nature of the comet's orbit with reference to S. It will be either a parabola or a hyperbola, having vertex at A; or an ellipse, having A as an apse (either perihelion or aphelion); or a circle, according to the distance AS and the velocity AA' or SS'; the real path of the comet in space will, of course, be obtained by combining the comet's motion in this path round S with the advancing motion of S, and will be either a skewed parabola or hyperbola, or a series of looped or wavy curves. These if the comet's relative path is circular, will be curtate or prolate cycloids, according to the sun's velocity; or in one particular case will be the common cycloid.—Ed.]

OPTICAL BLINDNESS TO RED LIGHT.

A CURIOUS effect of bright white light upon the vision is recorded in a recent number of the *Journal de Physique* by MM. J. Macé de Lépinay and W. Nicati. After passing some hours in a snow-field brilliantly lighted up by sunshine, it was observed that at least eight hours afterwards all gaslights, candles, and artificial lamps appeared to be strongly coloured green. In other words, the red rays of such lights were not perceived. The reason of this was supposed to be the fatigue of the retina for red, which partial effect lasts longer than a similar weariness of other colours. The truth of this supposition may be proved in a very simple manner by obtaining three coloured glasses—red, green, and blue—of such relative depth of colour that they could be seen through with about equal visual effect with a given power of light. An observer furnished with these glasses is then to place himself at a convenient distance before one of the sight-testing placards commonly used by oculists, and consisting of a white ground printed with black characters of various sizes. If the room is now almost darkened, the blue glass will still permit the observer to distinguish the medium-sized characters on the placard, while through the red screen not even the white sheet itself is perceptible. After a time, however—the same degree of semi-darkness being continued—the visual acuteness through the red glass is increased, so that the larger characters on the placard may be discerned. The visual perception through the blue glass remains as at first. It is therefore clear that colour-blindness, of a temporary nature, to the red rays, is more persistent than in respect of the blue rays. Hence may be assigned to physiological reasons the well-known fact that a prolonged or even temporary exposure of the eye to the electric light renders it for some considerable time afterward incapable of fully estimating the illuminating power of a gas-flame, which is so much richer in red rays.

RED SNOW.

AT a recent meeting of the San Francisco Microscopical Society, Dr. Harkness presented a bottle of "red snow," which he gathered last June on the Wasatch Mountains. The red snow was found on the north side of a spur which rose about 10,000 feet above the sea level. When fresh, the snow has the appearance of being drenched with blood, as though some large animal had been killed. The "red snow" is caused by the presence of a one-celled plant called *Protococcus nivalis*, which reproduces itself by subdivision; that is, the cell divides itself into several new cells. This is done with great rapidity, and a few cells lodged in the snow, under favourable conditions, soon will give it the appearance called "red snow." It was remarked that the phenomena of red snow had been observed from the earliest times, as Aristotle has a passage which is thought to refer to it. The subject was, however, lost sight of until brought up by the investigations of Saussure, who found it on the Alps in 1760. He made chemical tests which showed him that the red colour was due to the presence of vegetable matter, which he supposed might be the pollen of some plant. In 1819, an Arctic expedition under Captain Ross brought some specimens from the cliffs around Baffin's Bay, and they were examined by eminent botanists, some of whom mistook the nature of the plant, and there was long discussion as to its proper classification, some holding it to be a fungus, some a lichen; but it was finally set at rest as one of the unicellular algæ. It is of interest also that some of the early examiners pronounced the colour due to animalcules, but this was disproved. Dr. Harkness said that during his last visit to England, he saw the original bottle of specimens brought from the Arctic more than sixty years before, and in which the *protococcus* could still be seen with the microscope.—*Scientific American.*

WEATHER DIAGRAM, FOR WEEK ENDING SATURDAY, APRIL 15.

Several Correspondents point out that the Weather Diagrams, though "very pretty," are not sufficiently precise. We therefore substitute a Tabular Statement.

STATION.		Aberdeen.							Liverpool.							Valencia.							London.						
Day of Week.		Sun.	Mon.	Tues.	Wed.	Thurs.	Fri.	Sat.	Sun.	Mon.	Tues.	Wed.	Thurs.	Fri.	Sat.	Sun.	Mon.	Tues.	Wed.	Thurs.	Fri.	Sat.	Sun.	Mon.	Tues.	Wed.	Thurs.	Fri.	Sat.
Thermometer in Shade…	Highest …………	52	46	43	43	45	48	41	59	54	53	55	54	58	51	62	60	57	57	57	55	51	62	59	54	60	59	51	60
	Lowest …………	35	31	35	38	37	39	35	35	37	35	39	44	47	39	42	39	46	17	19	46	10	37	36	34	38	47	46	41
	Range in 24 hrs.…	17	15	8	5	8	9	6	24	17	18	16	10	11	12	20	21	11	10	8	9	11	25	23	20	22	22	8	19
Barometer………………	Inches …………	30	30	29	29	29	29	29	30	30	29	29	29	29	29	30	30	29	29	28	28	29	30	30	29	29	29	29	29
	100ths of an inch	·11	·15	·95	·88	·69	·27	·51	·35	·14	·92	·71	·27	·04	·12	·24	·17	·77	·11	·92	·96	·53	·30	·17	·96	·83	·48	·29	·36
Wind ………………	Direction …	0	N N W	N	E N E	E	E N E	N	E	S E	S E	E S E	SE	W S W	N E	0	0	E S E	E S E	S S E	S W	N E	N E	E	0	S S W	S S E	S W	W
	Force (0 to 12) …	0	1	2	2	8	8	7	3	1	2	2	4	4	3	0	0	2	4	7	3	6	2	1	0	3	2	5	1
Weather ………………	Beaufort Scale*	o	m	o	o	o p	r	c s p	m	m	m	m	o	o	r o	b m	b	c m	r	c	o m u	b c	o	b f	f g	c	r	b c q	c
Rainfall ………………	100ths of an inch	0	0	0	14	15	85	31	0	0	0	0	17	59	2	0	0	1	6	42	40	26	0	0	0	0	3	24	0

* Weather.—*Beaufort Scale* is, b. blue sky; c. detached clouds; d. drizzling rain; f. fog; g. dark, gloomy; h. hail; l. lightning; m. misty (hazy); o. overcast; p. passing showers; q. squally; r. rain; s. snow; t. thunder; u. ugly, threatening; v. visibility, unusual transparency; w. dew.

NEW METHOD OF FORECASTING STORMS.

In the course of his investigations on the behaviour of magnets, Father Secchi, the well-known Italian physicist, perceived the interesting fact that the disturbance of magnetic equilibrium which attends magnetic storms, and usually accompanies displays of Aurora Borealis, is also exhibited under the influence of cyclonic systems, or what are commonly known as the approach and passage of storms of wind and rain.

M. Descroix, Meteorological Director of Montsouris Observatory, Paris, has recently been following up the pregnant hint bequeathed by Father Secchi, and has published, in a recent Bulletin of the French Meteorological Society, his general conclusions. Fully six days before the arrival on the western shores of Europe of a cyclonic disturbance, the several magnetic needles (of declination, inclination, and horizontal force) showed decided symptoms of sympathy with the coming storm. The earth acts as a telegraphic conductor in announcing the approach of the gale.

M. Descroix is at present engaged in the endeavour to express in accurate formulæ the laws governing the action of distant cyclones on the magnetic needles, and we may hope at no very distant day to possess useful and accurate information on this important subject.

ICEBERGS.

Ice-fields and icebergs appeared off Newfoundland nearly two months earlier than usual this season. The steamship *Averill*, from West Hartlepool, England, was the first to tell of ice on the Banks, having sighted it in latitude 47° north, longitude 47° west, on Feb. 11. She was surrounded for twelve hours. Nearly every day since then the arriving steamships have reported ice, which has drifted to the southward and eastward. The steamship *Vandalia*, which passed around the ice-field, Feb. 11, sighted two towering bergs about 60 feet in height and 120 and 200 feet on the sides.

The White Star steamship *Germanic*, from Liverpool, reports that on March 1, in latitude 43° 35′ north, longitude 49° 10′ west, she was confronted with a great field of ice, and did not reach clear water for two hours. This seems to indicate that the ice extended for at least twenty-five miles. As no icebergs were seen, it is probable that in floating 205 miles to the southward and about 90 miles to the eastward, they crumbled under the influence of warmer waters. The Belgian steamship *Helvetia* encountered a field of ice and icebergs, and was forced to run to the southward 80 miles before she got to clear water. The steamship *New York*, from Bristol, fell in with large fields of ice and bergs, varying from 60 to 300 feet in height, and ran a south-south-east course for 160 miles at slow speed before she found open water. The British steamship *Milanese*, from Boston, February 18, for London, was so seriously damaged by the ice on the Banks of Newfoundland, that she put back to Halifax for repairs.—*Scientific American.*

COPYING DRAWINGS.

Tilhet's method of copying drawings in any desired colour is thus described in the *Polytechnisches Notizblatt*: "The paper on which the copy is to appear is first dipped in a bath consisting of 30 parts of white soap, 30 parts of alum, 40 parts of English glue, 10 parts of albumen, 2 parts of glacial acetic acid, 10 parts of alcohol of 60°, and 500 parts of water. It is afterward put into a second bath, which contains 50 parts of burnt umber ground in alcohol, 20 parts of lampblack, 10 parts of English glue, and 10 parts of bichromate of potash in 500 parts of water. They are now sensitive to light, and must, therefore, be preserved in the dark. In preparing paper to make the positive print, another bath is made just like the first one, except that lampblack is substituted for the burnt umber. To obtain coloured positives, the black is replaced by some red, blue, or other pigment.

In making the copy, the drawing to be copied is put in a photographic printing-frame, and the negative paper laid on it, and then exposed in the usual manner. In clear weather an illumination of two minutes will suffice. After the exposure the negative is put in water to develope it, and the drawing will appear in white on a dark ground; in other words, it is a negative or reversed picture. The paper is then dried and a positive made from it by placing it on the glass of a printing-frame, and laying the positive paper upon it and exposing as before. After placing the frame in the sun for two minutes, the positive is taken out and put in water. The black dissolves off without the necessity of moving it back and forth.

TO CORRESPONDENTS

Letters to the Editor.

[*The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with their writers. All communications should be as short as possible, consistently with full and clear statements of the writer's meaning.*]

All Editorial communications should be addressed to the Editor of Knowledge; *all Business communications to the Publishers, at the Office, 74, Great Queen-street, W.C.*

All Remittances, Cheques, and Post-Office Orders should be made payable to Messrs. Wyman & Sons.

∴ All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.

All Letters or Queries to the Editor which require attention in the current issue of Knowledge, *should reach the Publishing Office not later than the Saturday preceding the day of publication.*

(I.) Letters to have a chance of appearing must be concise; they must be drawn up in the form adopted for letters here, so that they may go untouched to the printers; private communications, therefore, as well as queries, or replies to queries (intended to appear as such) should be written on separate leaves.

(II.) Letters which (either because too long, or unsuitable, or dealing with matters which others have discussed, or for any other reason) cannot find place here, will either be briefly referred to in answers to correspondents, or acknowledged in a column reserved for the purpose.

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

"God's Orthodoxy is Truth."—*Charles Kingsley.*

Our Correspondence Columns.

CADDIS-WORM CASES—INTELLIGENCE IN ANIMALS.

[385] I notice caddis-worms use, among other things, little pieces of plants. I further notice, so long as the caddis has need of them, so long do they keep their life and original colour. How does the worm keep them alive?

What do scientific men understand by reason? A good definition wanted. Why divided into positive and abstract? What acts in man are the result of reasoning powers? Is reasoning power necessary for man to perform the acts of daily life? Would the acts performed by animals, if performed by men, be considered to require reasoning powers? John Alex. Ollard.

DOES THE MIXTURE OF BLUE AND YELLOW MAKE GREEN LIGHT?

[386]—If your correspondent on this subject (April 7, p. 496) would consider the experiment referred to by Helmholtz for mixing the lights reflected from two-coloured spots by means of a piece of polished glass held upright between them, he would quickly perceive that the image is produced by half the light from one of the spots, mingled with half the light from the other. But this is not the only way of mixing differently-coloured lights on the retina. It is easy, by means of a lens, to throw together the yellow and blue (that is the ultramarine blue) prismatic rays, and to see that they do not make green, but neutralise each other perfectly. The persistence of the sensations excited by light on the retina enables us also to produce the same effect by rapid rotation of a circular disc, painted half with lemon yellow and half with French blue.

In his fact 1, the yellow light from the sodium salt makes, with the sea-green or verdigris light from the chloride of copper, a yellowish-green mixture, which agrees with the theory he controverts.

In his fact 2 and fact 3, the greenish colour which he notices arises no doubt from the circumstance that the flames from the blowpipe and from the Bunsen burner give out, not a pure or ultramarine blue, but a sea-green blue light. When the yellow from the heated wire, or from the incandescent particles of carbon, is seen through these flames, the result is a greenish appearance, which is again consistent with the theory.

In his fact 4, as described, it seems utterly inexplicable how the green streak was produced in the prism; but as the sunbeam was admitted between two laths of a Venetian blind, and Venetian blinds are commonly painted green, I suspect the lens must have thrown an image of a bright edge of a green lath across the prism. W. Benson.

COLLISIONS AT SEA.

[387] With reference to Mr. Stewart Harrison's signals, alluded to in the article on Collisions at Sea last week, may I mention that by Article 19 of Rule of the Road, steamers are already at liberty to signal their intentions to each other by means of their steam whistles, as follows:—

One short blast—"I am directing my course to starboard," *i.e.* to my right.

Two short blasts—"I am directing my course to port," *i.e.* to my left.

Three short blasts—"I am going full speed astern."

It would not be safe to use any signals asking another ship to alter her course in any way, as the signal might be obeyed by the wrong ship—if two or three were in company, in an estuary or channel—and they might get mixed. Sailors would be glad to be able to avoid collision, though you remark on their apparent unconcern in the matter. They know how difficult it would be to enforce obedience to rules requiring certain signals to be made, and how confusing it would be if those signals were not made when expected. A frequent cause of collision is that the ship A, which ought to give way, keeps on too long, and frightens the ship B, which ought to keep her course, into acting upon Article 23, and swerving, in order to avoid what she considers immediate danger of collision; perhaps starboarding at the very instant that A "ports," and that, too, at the very last moment; so that there is no time to remedy the mistake. The blame ought, of course, to lie with A, who, though he certainly did give way, did not do so in time to let B know he was doing it. I have seen this state of things from the deck of a small sailing-vessel, when I have not dared to alter my course for fear of baulking the steamer, and when the steamer has carried on, and "shaved" me, her tall sides actually taking the wind out of our sails as she cut her way close by; and yet, had we altered our course, we might have run right under her bows. I have also seen the same thing from the opposite point of view, viz., from a steamer's bridge, the officer in charge apparently thinking that, so long as he knows that he intends to keep clear, it is of no consequence whether those in charge of the other ship get scared or not. There are practical difficulties in the way of placing the red and green lights as suggested in your article; in a sailing-ship, the sails and rigging would to a greater or less extent mask the midship light. A streak of luminous paint all round the hull, in addition to the present side lights, has been proposed, and probably would better show small changes of course at night than anything else. F. C. G.

REPLIES TO QUERIES.

Adjusting Equatorial.—In taking the declination of a star six hours from the meridian, for the purpose of adjusting an equatorial, the refraction for its altitude at that instant should be taken out of Bessel's, or some other table. We must, though, of course, know what that altitude is, and this we find simply by adding the logarithm sine of the latitude to the logarithm sine of the star's declination. Thus, what, let us say, is the altitude of α Cygni when six hours from the meridian in latitude 51° 30′?

51° 30′ lat.	Sine 9·893645
44° 52′ dec. of α Cygni.	Sine 9·848472
33° 31′ 11″	Sine 9·742117

Turning now to any table of refractions, we find that the mean refraction corresponding to 33° 31′ is 1′ 33·9″. Of course, part of this operates in shifting the star in right ascension; but we may use it all for our present purpose without introducing any error likely to be sensible in our instrumental adjustments.

350.—"Whitby" is informed that jet is nothing but a compact variety of coal, and has been formed in precisely the same way. Its vegetable structure is readily seen, in thin sections, under the microscope. F.R.A.S.

The number of asteroids that have been discovered is now 220. Recent researches by Herr Hornstein (communicated to the Vienna Academy) appear to prove that the number of those with a diameter of over twenty-five geographical miles is extremely small, and that probably all such were discovered before 1850. On the other hand, the number of asteroids with a diameter less than five miles seems also to be very small, at least in the parts of the asteroid zone next Mars; in the outer regions next Jupiter there may be a more considerable number of these very small bodies. Most asteroids seem to have a diameter of between five and fifteen miles.

Answers to Correspondents.

**** All communications for the Editor requiring early attention should reach the Office on or before the Saturday preceding the current issue of* KNOWLEDGE, *the increasing circulation of which compels us to go to press early in the week.*

HINTS TO CORRESPONDENTS.—1. *No questions asking for scientific information can be answered through the post.* 2. *Letters sent to the Editor for correspondents cannot be forwarded; nor can the names or addresses of correspondents be given in answer to private inquiries.* 3. *Correspondents should write on one side only of the paper, and put drawings on a separate leaf.* 4. *Each letter should have a title, and in replying to a letter, reference should be made to its number, the page on which it appears, and its title.*

[In response to the wishes of a large number of correspondents, answers to questions which *hereafter* may reach us will not be given under this heading. We have had to discontinue Queries and Replies because, while taking up much space, they only interested a few. The answers under the present heading were open to the same objection, and to the further one that they were mostly unintelligible to all save the individual querists. As we had to find room somewhere in response to the growing demands upon our space, we have decided to find room in these sections, in which only the few take interest. Questions which hereafter may reach us will be acknowledged and forwarded to those who write for us upon the subjects to which such questions relate; and should those writers see fit, may suggest articles, or paragraphs for articles, of interest, we trust, to all our readers. Hereafter, KNOWLEDGE will occupy the same position in this respect as the *Athenæum*, *Academy*, and *Nature*, in which there are no columns for queries or replies, and very few answers to correspondents. Questions which, being suitable in themselves, come in suitable form, will appear under head "Letters to the Editor," and may be answered under the same heading.—ED.]

MISCELLANEOUS.

E. D. G. Yes; Ladder of St. Augustine appeared in 1858. Why did you not remember this before?—G. R. WYNNE. If only we could have 100pp., such letters would be useful. But what can we possibly do with 2 ft. × 1 ft. MSS.? — H. G. A. W. East wind is not cold in summer, or in all countries.—J. A. OLLARD. Many thanks for kind suggestions; will consider them, and attend to other matters later.—ROBERT MACPHERSON. The subject of legal responsibility in cases of mental disturbance is full of interest. I shall not overlook the suggestion. Has the enunciation of rules about "shall" and "will" ever made any difference as to the use of these words? Macaulay remarks, in his essay on "Lord Bacon," that though "not one Londoner in ten thousand can lay down the rules for the proper use of *will* and *shall*, not one Londoner in a million ever misplaces his *will* and *shall*;" and that though "Doctor Robertson could undoubtedly have written a luminous dissertation on the use of these words; he sometimes misplaces them ludicrously, even in his latest works."—DESESPOIR. We cannot in any way be responsible for the *bona fides* of advertisers in KNOWLEDGE.—E. ROWE. No; we regret much more than you can such stupidity on the part of some booksellers.—W. H. JONES. I do not think that Professor Tyndall, or any other student of science, doubts that in past times, under particular conditions, there may have been spontaneous generation. He only asserts that under any conditions, now, which exclude living germs, no spontaneous generation occurs.—C. J. BROWN. Your letter rendered almost illegible by blurring. We, too, were disappointed that no direct remedy from bad fires and ventilation was given, short of rebuilding all our houses. On the spoil-bank matter, Mr. Williams did not mean, we think, that spontaneous generation was always the cause, but only that it was a common cause.—S. M. SUTTON. So far as a mere outsider can judge, the "inner voice (so termed)" utters nonsense; perhaps others may find a meaning in "the combatical force of pseudo metallurgy."—A. Yes; but violet is not blue with red in it. Have you ever seen the violet of a pure spectrum?—R. J. HOULTON. Thanks for replies. Dog anecdote resembles others with which our readers are familiar.—JONES & BARBER. Thanks for invitation.—PHILALETHES. Egyptologists agree, *nem. con.*, in the earlier dates, and have not overlooked the point to which you call attention. Osburn's views are now entirely out of date. Can you name a living Egyptologist of any standing who maintains that the commencement of the 4th Dynasty could not have been earlier than 2200 B.C.—ALEXANDER HOWELL. It is very good of you to correct Sir Edmund Beckett about the law of patents; but for our own part we should as soon think of explaining to Mr. Muybridge the elementary principles of photography.—A. M. SMITH. Without direct evidence, such theories are of little value; to the question "May not so and so be such and such?" the answer is either "It may be so," or "It may not be so," neither of which advances us much.—W. F. S. May we not express our thanks for your kind and encouraging words?—IRIS wishes to learn where a prism such as Mr. Higgins describes at p. 170, No. 22, and mounted as described, can be obtained.—A. W. W. W. W. Thanks, but explanation rather doubtful.

ASTRONOMICAL AND MATHEMATICAL.

M. H. C. Ought we not rather to say that Sir E. Beckett rejects the theory which you show to be incorrect, and that the centrifugal theory as modified (but not as it has been propounded by several) is correct enough? This, however, applies rather to the correction of the centrifugal force than to his omission of moon's total attraction. But it is difficult to say what have been the exact ideas of some who have propounded a centrifugal theory of the tides.—CANTAB. You do not consider the earth's more rapid motion in perihelion at time when her orbit was at its greatest eccentricity. The influence of a short, very warm summer would not counteract the influence of a long, very cold winter.—H. C. W. Does not that amount to what we have already said about the log. paradox?—H. B. L. Clifford and Helmholtz have not merely maintained that material space is or may be limited, but that abstract space may be. The former question would be purely speculative; the latter would be admissible if we could admit that the twelfth axiom of Euclid is incorrect. Although the question you discuss belongs to natural, not to dogmatic religion, it is scarcely suitable for discussion here.—S. M. B. G. We thought the papers too diffuse, and the subject-matter too familiar. But further, you selected a subject in relation to which we have exercised some self-denial already, in admitting papers from others which we might very well have written ourselves. We can only give a certain portion of our space to that subject, and we want to leave a *little* of that space to ourselves. We cannot answer by letter.—AIR. This is scarcely the place for aërial tricycle.—J. P. H. Try your method in case of a rectangle fifty times as long as it is broad, and show how two cuts suffice in that case.—G. F. OGILVY. Precisely the same, whether centre of motion is outside or inside the body so moving.—A. T. SKINNER. Your solution of Mogul's problem correct.—F. H. S. The subject is closely related, as you say, to inquiries into nature of space and time; in time we hope to find space for it.—VIGILANS. My Library Star Atlas would be of more use for telescopic work than either of the series of maps (by me) you name.—J. W. C. The transmission of such wave motion is akin to what we see on the surface of water, where the motion of the particles is (in the main) at right angles to the direction of transmission.—HUGH COLEMAN DAVIDSON. The earth's poles describe two small circles, as you suppose.—G. L. H. Each ray from the disc of a planet is disturbed, just as the rays from a star; but as the disc of a planet is not appreciably a point, as a star is, and the different rays from different points of the disc are differently disturbed, the disturbances in some directions counterbalance those in other directions, and we have a steady light.—M. L. ROUSE. Yes, the paradox is as you describe it.—J. B. SHIPLEY. One can only consider a lever as intended to move something. The P and W of the account of any lever can always be interchanged; and you can also, if you will, interchange R, the resistance at fulcrum, with W. In inquiring into propulsive force on boat, we must regard the boat with its load as the weight, the energy exerted by the rowers as the power. It may be very absurd, but we cannot help ourselves.—G. RIMINGTON.—Thanks, but reply about Sinking Funds rather longer than the general interest taken in subject justifies.—COMETS. Alas! your theory will not do. It is hopelessly remote from possibility. The term "paraboloid," by the way, is already in use, not for your mixed curve, but for a surface.—JOHN HAMER. You could get clearer ideas of the moon's rotation if you studied a trustworthy treatise on dynamics, than by waiting for ideas "in the watches of the night." The idea of the moon not rotating on its polar axis, but on the equatorial axis! Define polar axis, and see what comes of your idea.—JAS. OFFORD. Received, and will be used. Do you wish Zodiac of Denderah to be engraved?—W. EDWARDS. An annular eclipse of the sun observed in this country on Sunday, May 15, 1836.—JAS. DEAS. You have not either statement quite right. The earlier (in my essay, Gamut of Light) was erroneous. The determination of stellar motions of approach and recession too difficult for anything like exactness to be obtained.

Letters Received.

Alex. Howell, Onward, A Novice, Z. O. Z., Montpellier, A. Fisher, W. G. Williams, Mary Powell, J. F. Humphrey, W. P. B., Crossbar (questions vague, or already answered, or otherwise unsuitable). C. Carter, W. J. Hamsmith, A. J. Doherty, J. Pepperson, W. G. Williams, Nag's Head (*Nag's* Head? Are you *sure*?), Emily F., J. Harvey, M. Murgetson.

Notes on Art and Science.

An experiment with a system for using petroleum instead of coal for fuel was tried on the Long Island Railroad recently, and was pronounced a success. The train was run on schedule time, and the cost was $1.20, as compared with $2, the price for coal. The new fuel is a vapour produced by the intermingling of jets of petroleum, superheated steam, and hot air.

A new and interesting proof that the earth is round has been presented by M. Dufour in a paper recently read before the Helvetic Society of Natural Sciences. In calm weather the images of distant objects reflected in the Lake of Geneva showed just exactly the same degree of distortion which calculation would predict through taking into consideration the figure of the earth.

Recent investigations by Dr. Hann indicate that the mean temperature of the southern hemisphere is the same as that of the northern, but between 40 degrees and 45 degrees south latitude, the southern hemisphere becomes warmer than the northern in the same latitude, and this difference continues at least to the confines of the hypothetical Antarctic continent.

Gray's well-known work on anatomy has been translated into Chinese, and the translator, the late Dr. Osgood, is said to have succeeded in giving Chinese names to the multifarious and minute structures which constitute the human body—a difficult task, as the Chinese know scarcely anything of anatomy, or of the functions of the various organs of the body.

A Georgian, of scientific attainments, residing at Darien has discovered that lenses for telescopes can be manufactured from the virgin drip of rosin. The largest lens made of glass is only thirty inches in diameter. This magnitude can be greatly increased by the new method, and consequently there is no telling what wonderful astronomical results may flow from its adoption. Gentlemen who are conversant with science say that the Darien discovery is worthy of a thorough test.

A new plan to deaden floors has been patented, and is being tested in a new building at Philadelphia. A six-by-three plank is inserted between each joist two inches from the bottom of the joists, and projecting four inches beneath. Underneath the intervening planks the ceiling boards are nailed and the space filled with sawdust to within an inch of the joists. By this method the waves of sound are carried off, and it is claimed that the most vigorous hammering cannot be heard in the story beneath.

Our Mathematical Column.

THE LAWS OF PROBABILITY.

By the Editor.

At first sight nothing seems clearer than that the answer given by mathematicians to the Petersburg problem is untrue. I have even heard persons to whom the problem and its answer have been submitted assert that no amount of reasoning would convince them that so preposterous a solution was just. Unfortunately, the reasoning given in treatises on probability, through sound, is commonly too recondite to convince these sceptics. Let me repeat the problem, and re-state the answer: and then let us try to see our way to a clear interpretation of the seeming paradox. The problem runs thus:—

Each person in a certain lottery is to stake £x on the following conditions. A coin is to be tossed until head appears; if head comes at the first toss the person is to receive £2; if at the second toss, he is to receive £4; if at the third, he is to receive £8; if at the fourth £16, and so on. Required the value of x.

The startling answer is that x is equal to infinity; in other words, that though each person staked a sum never so great, the "bank" would lose.

Now it seems so obvious that if a large sum were paid for a chance in the lottery, the speculator would lose, that it is difficult to believe that some fallacy does not underlie the reasoning by which the above answer is obtained. Accordingly even first-rate mathematicians (like d'Alembert) have questioned the justice of the answer. Yet I believe I shall be able to convince even non-mathematicians that the answer is sound.

The main objection is founded on the difficulty of believing that in any series of trials, however long the series might be, tail would be tossed many times running. For example, a sequence of twelve tails seems utterly unlikely to occur even in many millions of trials. Especially does this seem to be the case, when we try to consider the case of a person who should keep on continually tossing a coin until he had tossed twelve tails in succession. He might toss twenty, thirty, a hundred, nay a thousand or ten thousand times without success, and at the end of all those trials he would have no better chance of succeeding in a fresh series of trials than at first commencing. We cannot recognise any reason why so remarkable a set of throws as twelve successive "tails" should *ever* reward his patience.

Yet it is not difficult to show that, given only a sufficiently large number of trials, the really wonderful thing would be that twelve successive "tails" should *fail* to be thrown.

To simplify matters, let us conceive that instead of one person making a series of trial-tossings, we have a large number of persons, each of whom is to toss until head appears. Let us set the number at one million. It is obvious that when each of these million persons has tossed his coin once, about one-half will have thrown tail. Say half exactly, for convenience of computation; since, at any rate, we cannot regard it as a very wonderful circumstance if as many as 500,000 of the million toss tail. These 500,000 are now to toss again. About one-half will again toss "tail." Say as before, exactly one-half. The 250,000 who have tossed tail twice toss it yet again; and about 125,000 toss "tail" a third time. Then the 125,000 toss a fourth time, and about 62,500 toss tail a fourth time. So about 31,250 toss "tail" a fifth time running; about 15,625 a sixth time; about 7,812 a seventh time; about 3,906 an eighth time; about 1,953 a ninth time; about 976 a tenth time; about 488 an eleventh time; about 244 a twelfth time; about 122 a thirteenth time; about 62 a fourteenth time; about 31 a fifteenth time; about 16 a sixteenth time; say 8 a seventeenth time; 4 an eighteenth time; 2 a nineteenth time; and *one* a twentieth time. When we get among these smaller numbers we feel less confident of the result; but among the larger numbers, though we can by no means feel certain as to the exact number of "heads" and "tails" that would be tossed, we feel the utmost confidence as to the general character of the result. Thus, supposing 31,000 had tossed "tail" five times running; then it would be a highly improbable thing that less than 14,000 or 15,000 out of the 31,000 would toss "tail" on the next trial. And even as respects the smaller numbers there would be at least as fair a chance of as many "tails" being tossed as the above reckoning assigns, as the contrary. So that, though a first, or second, or third trial with our million tossers failed to give one person, at least, who tossed "tail" twenty times in succession; yet a few successive trials (each trial including all the million persons) would undoubtedly insure this seemingly incredible result, that twenty successive tossings of a coin could give an identical result.* As for merely twelve successive "tails," we might be sure of getting upwards of a hundred instances of that sort on the very first trial.

If we calculate how much would be paid on the lottery after one of these sets of a million tossings, we shall at once begin to see why each tosser should pay a large sum for his chance. Instead of doing this directly, let us begin with the case of a few tossings, and estimate the effect of increasing the number of trials—assuming, for convenience, that exactly half those who toss in any case, toss "head," the other half tossing "tail." This assumption does not influence the reasoning, because it is clear that if more than half toss either head or tail, it is as likely that more tails than heads as that more heads than tails will be tossed.

If there are four persons, two toss "head" and receive £2 each, or £4 in all. On the second trial, one tosses "head" and receives £4. On the third, say the one tosser left throws "head," and receives £8. The money to be divided between the four persons is thus, £16; or an average of £4 to each.

If there are eight persons, four toss "head" at the first trial, and receive £2 each, or £8 in all; two toss "head" at the second trial, and receive £4 each, or £8 in all; one tosses "head" at the third trial, and receives £8; the last tosses "head" at the last trial (say), and receives £16. In all, the sum of £40 is to be paid to these eight persons, or an average of £5 to each.

In like manner, if there are sixteen persons, eight will get among them £16; four will get among them another £16; two will receive a third £16; one will get £16; and the last £32; or £96 in all will have to be divided among sixteen persons, that is, an average of £6 to each.

* In ten successive trials with our million of tossers, the odds are more than 10,000 to 1 that 20 successive "tails" will be tossed. And only 693,147 out of the million need take part in one trial to give an even chance of tossing twenty successive heads. De Morgan's book says 70,000; but there must be a misprint.

And by proceeding in this way it will be found that (on the assumption made), if thirty-two persons engaged in the speculation, an average of £7 would have to be paid to each; if sixty-four engaged, an average of £8 to each; if 128 engaged, an average of £9 to each; if 256, £10 each; if 512, £11 each, and so on. The general rule being that, according to the assumption, if 2^n persons engaged, an average of n + £2 would have to be paid to each. So that as there is assumed to be no limit to the number of persons who may try their chances—or, what practically comes to the same thing, no limit to the number of trials which may be made—we have n as large as we please, and therefore $(n+2)$ £ the average number of pounds the bank would have to pay for each out of 2^n trials may be made as large as we please, or equals *infinity*.

It will be noticed that in the above remarks I have not overrated the value of the several chances. For instance, if there are eight tossings, the occurrence of four tails cannot be thought an unlikely event. And in one respect I have systematically underrated the value of each set of trials; for when but one person is left who has not tossed "head," I have invariably supposed the single toss to give "head." It is easily seen that the effect of this is to diminish the estimated value. In fact, two trials where four persons are engaged correspond to one trial with eight persons; four trials with four persons correspond to two trials with eight, or to one trial with sixteen persons; so that, as might be expected, the repetition of any of the several kinds of trial above considered leads to a steady increase (on the assumption made throughout) in the mean value of each person's expectation.

It is also well to notice how slowly this mean value increases with the increase of the number of trials when once we have reached large numbers. Thus, for 2,048 persons, the mean value of each person's expectation is £13, and for 4,096 persons, the mean value is £14; an increase of only £1, though 2,048 persons are added; and 4,096 persons must be added to increase the mean value to £15; 8,192 persons more to increase the mean value to £16; and so on.

But now, returning to our million of tossers, let us consider how their various fortunes illustrate the general doctrine of probabilities, and more particularly the subject of luck. When we consider the million as a whole, we find nothing in the result of the tossings which seems to indicate either good or bad luck; for in each fresh series of trials about one-half have tossed "head" and about one-half "tail." But if we conceive the various individuals of our army of tossers to remain unaware of the real nature of the process in which they are taking part, and only to know the results of a few tossings taking place in their immediate neighbourhood, it will be seen that opinions resembling those formed in the world at large respecting good luck and bad luck would be found among our tossers. Those 240, or thereabouts, who tossed "tail" twelve times running, would be regarded by those around them (severally) as exceptionally lucky men. Many might be disposed to back the luck of one or more of these fortunate individuals of whose success they might become cognisant. These 240 are not a whit more likely (severally) to toss "tail" than to toss "head" at the eleventh tossing; and yet if one were to reason with those who backed one of the lucky 240, it might be found very difficult to persuade him of the folly of his course. One might reason that there was no such thing as trustworthy luck; that though such and such a tosser had been lucky so far, yet no inference could be drawn from his past success as to the success of his next venture; and so on. But the reasoning would seem good in answer, that there *must* be such a thing as good luck, for had not this particular tosser thrown "tail" twelve times running, whereas no one else of those around had thrown "tail" more than four or five times running? His luck had been trustworthy in the past, why might it not be trusted as respects the future also? In fine, the proposing backer might remain obstinate in the belief that he was doing a rather clever thing in backing the luck of the fortunate tosser, and perhaps at heavy odds.

On the other hand, there is a line of reasoning equally unsound, by which a directly opposite conclusion may be reached. A person who had heard of the tossing of "tail" twelve times running, might conclude that "head" would be almost certain to come at the next trial. We can see that this is not so, when we remember how our 240 (or so) successful losers are to proceed to a thirteenth trial, and that only about half of them may be expected to succeed. But any reasoning founded on the abstract probabilities might fail in this case, as in the former; because specious reasoning may be urged in favour of failure on a thirteenth trial. Thus it might be urged that to toss "tail" twelve times running is altogether unusual; much more, therefore, must it be unusual to toss "tail" thirteen times running. And the reasoner, forgetting altogether that the only question he has to consider is the single tossing about to take place, and *its* chances, might confine his attention to the *à priori* improbability of tossing thirteen "tails" in succession. In betting on the result, he might persuade himself that it was this unusual event he was betting upon, and so take heavy odds against it; whereas, in reality, the event he was betting upon would be simply the result of the tossing of a coin *once*. It is certain, at any rate, simple as the question is in reality, that nine men out of ten do reason in this unsound manner.*

Two highly important lessons may be drawn from the consideration of these matters, and it would be well if those who have a taste for gambling would study those lessons carefully.

In the first place, we hear accounts from time to time of very lucky gamblers; of runs of luck by which men have "broken the bank" at Baden or Homburg, and so on; and many are led to believe that there really is such a thing as luck that can be depended upon, and so are encouraged either to court fortune by backing those who have been lucky, or else to try whether they may not themselves be lucky in gambling ventures. The consideration of the St. Petersburg problem has shown that where many gamble, there *must* be some who have an extraordinary run of luck. Because, although the problem as dealt with only relates to the tossing of a coin, it is obvious that similar conclusions would have been deduced, whatever ventures had been considered, and even though the odds were heavy against success in each separate venture, instead of being even, as in the case of tossing a coin. If a large number of men cast each a die, about a sixth will throw Ace; of this sixth, again about a sixth will throw Ace on a second trial, and so on; and clearly, it only requires that the original numbers should be large enough, to get several who will throw ace, ten, twelve, twenty, or any number of times running. And in every such instance we shall always have our lucky men, amongst whose ranks, however, the next trial will make the same relative gap as among a similar number of untried, or of hitherto unlucky, persons. So it is with the multiplied trials continually going on in the gambling world. There *must* be many seemingly lucky men; and there *must* be some few who seem lucky, even among the lucky. But neither the lucky, nor the luckiest of the lucky, are better worth backing in a new venture than some unfortunate who has hitherto never had the smallest modicum of good fortune. Take a man who has broken the bank half-a-dozen times at Baden or Homburg, and let him risk his money on some fair venture with a man who has never sat at the gambling table but to lose every penny in his possession, yet there is not a straw of odds upon either.

The other lesson is equally important, and the mistake which it tends to correct has been as mischievous in its results as the one just considered. The belief that "the luck must change" has over and over again led the unfortunate gambler to persist in making fresh ventures. "I have been unfortunate so long," he reasons, "that now I may expect a run of good luck; to give up gambling now would be to throw away the good fortune I have been so long waiting for." The Petersburg problem teaches precisely the same lesson respecting ill-fortune as respecting good fortune, since the same results would follow whether we regarded the tossing of "tail" as an event to be *rewarded* by a money payment, or as an event which should compel the loser to *pay* money. We see that the sequence of many events of the same kind—*i.e.*, a run of luck—can teach us *nothing* as to future events. A run of bad luck should be regarded by the gambler as belonging altogether to the past; the "whirligig of time" may or may not "bring in its revenges," or what may appear as such; but the past ill-luck of the gambler will in no sort affect his future fortune. He has not the slightest valid reason for expecting a run of good luck to counterbalance his former bad luck.†

(To be continued.)

* The old story of the sailor, who put his head through a hole made by a ball in the side of his ship, confiding in the improbability that a second would strike the ship in the same place, is true to nature;—only we are not bound to believe that the sailor was a Briton.

† A change of luck he may, in one sense, expect; that is, he may hope not to have a run of bad luck such as he has already had. But he has no other reason for hoping this than the actual improbability of a run of luck, either good or bad, in a given series of trials. A man who has lost five games (of pure chance) in succession, may expect a change of luck, in so far as he may hope to win some, at least, of the next five games. But he has no better chance of winning some of these five games than he would have had if the first five had not been played. Thus a cessation of bad luck repeatedly takes place when many games are played. If the seeming change of fortune follow after a change of seat, or the use of a new pack of cards, or some like observance of gambling superstition, the fact is noted (the failure of the observance would not be noted) and the superstition is encouraged.

ANSWER TO MATHEMATICAL QUERY

(341.)—The number of ways in which five girls can be chosen out of eleven is $\frac{11 \times 10 \times 9 \times 8 \times 7}{5 \times 4 \times 3 \times 2} = 462$. Each day's arrangement uses seven of these ways, namely, one in a separate group of five and six in the group of six. Therefore all the ways will be used in $\frac{462}{7} = 66$, days, which is the answer to the query, provided the girls can be so arranged as to make use of all combinations. I do not see how this can be determined except by trial, but by this means I find that it can be done as follows. Let the groups of five for six days be as follows (it will be unnecessary to give the groups of six which will consist of the remaining girls):—

1.2.3.4.6 – 1.2.3.7.10 – 1.2.3.8.9 – 1.2.4.5.8 – 1.2.4.7.9 – 1.2.6.8.10

From each of these groups make groups for ten more days by successive additions of 1 to each number except 11, which you must reduce to 1, instead of increasing it to 12.

I add the method of conducting the experiment:

If the numbers 1 to 11 are supposed to be arranged in order in a circle so that the distance from 11 to 1 is the same as that from any number to the next, the sum of all the distances between the consecutive numbers of any five selected will be eleven, e.g., if the five numbers be 1.3.4.7.11, the differences will be 2.1.3.4.1, and the same differences in the same order may be used for eleven different groups of five according to the number chosen to begin with. The following list is easily made of all the different arrangements of differences:—

11117	11225	11342	12125	12242	13142
11126	11234	11414	12134	12314	13214
11135	11243	11423	12143	12323	13223
11144	11252	11432	12152	12332	13232
11153	11315	11513	12215	12413	13322
11162	11324	11522	12224	12422	14222
11216	11333	11612	12233	13133	22223

Selecting a group of five having one of these sets of differences we see what sets of differences belong to the combinations of five which can be made out of the group of six which was left when the first group of five was made. Seven sets of differences are thus disposed of, and it will be found easy to divide the whole forty-two into six such sevens. I believe, however, that there are only two ways in which this can be done. Of course after this is done each seven sets of differences can be used eleven times, thus solving the problem.

I had some doubt whether the true construction of the problem was that the five who did not present themselves to the giver of the bouquets constituted a separate group. If they did not it would seem as if only six groups of five were used in a day, and that the answer might be 77 days, but I do not know how groups of six can be selected out of eleven for 77 days without having the same five in a group twice. It cannot be done by the method I have used.

ALGERNON BRAY.

Our Chess Column.

END-GAMES.

IN one important respect, at least, end-games are of more consequence than the openings. Any weak move made in the beginning of a game does not necessarily entail its loss, as in the middle game a player has many chances to re-establish the balance of position, or even to obtain a superiority, notwithstanding his unfavourable commencement; but the end-play directly influences the result—there is no appeal. A single weak move to compromise a position will have the loss of the game as its consequence. This axiom has a twofold application in actual play—it holds good both "for winning a game" and "defending a game."

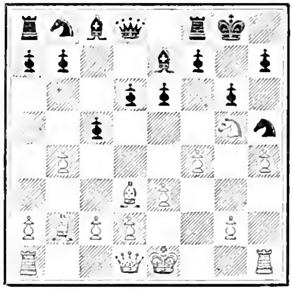
Defending a game naturally includes playing to obtain a draw; while winning a game, also means playing to prevent a draw. The greatest possible amount of precision is required in either of the above cases, which fact renders play in an ending far more difficult than in the middle or in the opening. Every position has its limited number of probable moves, and if through receiving odds or by any other means a player has a better position than his opponent, he will not have much difficulty in recognising and following up the natural advantages of his position, as, to a certain degree, the advantage manifests or developes itself. Equally it may be said that the player having an inferior game will have great difficulty in avoiding the natural outcome of his position. We have played many a game where we plainly saw our defeat impending in ten or twelve moves. Our opponent did not see it; nay, perchance he might have even thought his own game lost; but the position played itself. Move after move he adopted the most promising line of play, till suddenly, to his surprise, he found himself the winner. In the end the positions are generally less suggestive, and, therefore, a player is thrown more upon his own resources.

There are two kinds of endings—first being that termination of the game brought about by a brilliant sacrifice, or a series of moves of great power and deep and fine play. This ending is the most beautiful and ingenious. From it the art of problem-making has sprung—problems are merely correct endings having a mate in a certain number of moves. As a fine example of this class, we give the following end-game, which occurred a few days ago at the Birmingham Chess Club. Mr. W. Cook gave his opponent a Knight, and after eight moves only he arrived at a position which enabled him to win the game in a brilliant manner.

Position after the eighth move.

MR. WILSON.

BLACK.



WHITE.

MR. W. COOK.

White here announced Mate in four moves, and proceeded as follows:—

Q takes Kt	B takes Kt (last)

(Black cannot take the Queen, on account of B takes RP mate.)

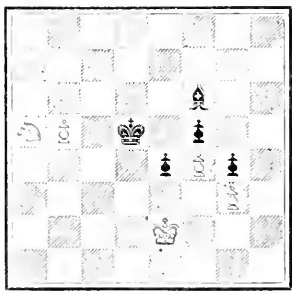
Q takes RP (ch)	K takes Q
RP takes B (ch)	K to Kt sq
R to R8 (mate)	

Play of this kind has very truly been called the "poetry of the game;" but of far more importance to the learner are those examples where the game is won by correct and strong play only. The most interesting endings are those where the Knight plays a leading part. We give as an illustration an end-game which occurred in the match between Messrs. Blackburne and Gunsberg, showing how, with an equal position, the Knight with correct play did win against a Bishop.

Position after Black's 52nd move.

MR. BLACKBURNE.

BLACK.



WHITE.

MR. GUNSBERG.

In this position, which (as can be seen from the number of moves made) was arrived at after prolonged manœuvring with the

Knight, White conceived the idea of advancing his single Pawn in such a manner as to draw the Black King a sufficient distance away from his own Pawns. The game proceeded as follows :—

53. P to Kt6	53. B to Qsq
54. P to Kt7	54. B to B2
55. Kt to Kt3*	55. B to Kt sq
56. Kt to Rsq†	56. K to B3‡
57. Kt to B2	57. K takes P
58. Kt to Q4	58. B to R2
59. Kt takes P	59. K to B3
60. Kt to R6	60. K to Q4
61. Kt takes P	61. K to K3
62. Kt to R6	62. K to B3
63. P to Kt4	63. B to B4
64. Kt to B5	64. K to K3
65. Kt to Kt3	65. K to Q4
66. P to Kt5	66. B to Q3
67. P to Kt6	67. K to K3
68. P to B5(ch) then Kt to R5(ch)	68. K to K2, if K to B3
69. Kt takes P	69. B to K4

70. K to B3, Kt4; and Kt5, and White ultimately won the game.

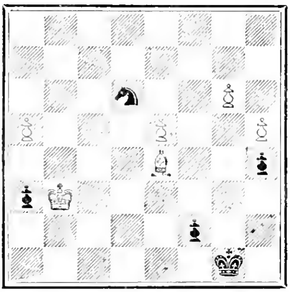
Finally, we draw the attention of our readers to the game by correspondence, carried on between our chief Editor and our Chess Editor. In our last week's number we gave the moves, showing how Black endeavoured by a series of wide tours with his Knight to catch any of his opponent's Pawns; but fortune was too much against him, and he lost.

GAME BY CORRESPONDENCE.—(*Continued from p. 524.*)

Position after Black's 46th move, Kt to K6.

Chief Editor.

white.



Black.

Chess Editor.

White played.

47. B to K4

This move destroys all hope of Black. Had White played 47. B to K6 (a very likely-looking move) then Black could have drawn the game by playing 47. Kt to Kt7. 48. P. to R5, 48. Kt to B5(ch), and he wins the Rook's Pawn, in which case he would not have had much difficulty in drawing the game. This ending may serve as a good example to our readers to show how, by a slight error of judgment, a won game may be turned into a drawn game. Black replied :—

	47. Kt to Kt5
48. P to R5	48. K to R2
49. B to B5	49. Kt to K6
50. B to K6	resigns, as

after K takes P, White would advance his Rook's Pawn.

---

* The proper moment to abandon his Pawn.

† The only move to win. It wins by commanding both K3 and Q4 after getting to B2.

‡ Best, for the Kt threatened to force the King to K3, and then play to R6, winning a piece.

SOLUTIONS.

Problem 26 next week.

Problem 27, p. 461.

1. K to Ksq.

If 1. K takes R, 2. Q takes P(ch), and mate next move.

If 1. R takes Q, 2. B to QB5; if 2. P takes B, 3. Kt takes P (mate); or, if 2. B takes R, 3. B to Q5 (mate), 2. K takes R, 3. Kt to B2 (mate), or R to K3 (mate).

If 1. R takes RP, 2. Kt to B5(ch), 2. P takes Kt, 3. Kt to B2 (mate).

Problem 28, p. 461.

1. R to Qsq, and mates accordingly.

Problem 29, p. 461.

1. Q to Q3, and mates accordingly.

Problem 30, by J. A. Miles, p. 486.

1. Kt to Q6	1. Kt to Q7; or (*a*)
2. Kt to K4	2. Kt takes Kt, 3. P to B4 (mate); or else Kt to B6 (mate)

(*a*) If 1. R to B8, 2. B to B5ch, and 3. P to Kt5 (mate).

If 1. B takes R, 2P to B4 (mate).

Problem 31, by Leonard P. Rees, p. 486.

1. B to B6, and mates accordingly.

Problem 32, by B. G. Laws, p. 486.

1. Kt to B4	1. K to Q4
2. Q to Q6(ch)	2. K takes Kt, or K to K5

3. Kt to R3, or 3. Kt to Q2 (mate).

If 1. K to Q2, 2. Kt to B3, and mates accordingly.

If 1. K takes P, 2. Q to K5(ch), and 3. Q to Kt 7 (mate).

CORRECTION.

Page 505, White's 7th move ought to be Kt to B3; his 8th move Q to K2.

ANSWERS TO CORRESPONDENTS.

*** *Please address Chess-Editor.*

G. W. *versus* Freeman.

Muzio.—Solution Nos. 33 and 34 correct.

H. Planck.—Nos. 25, 28, 33, and 34 correct.

F. H. Jones.—26 incorrect, as 2.Q takes R with a check. No. 32, Kt to QB3 does not mate. 30 and 31, see solution. 25 correct. 33 and 34, solutions correct.

Alfred B. Palmer, and Ringwood.—25, 33, and 34 correct.

William Wod.—No. 25 correctly solved. Answers depend on the pressure of correspondence.

R. A. Standen.—Solutions 28, 29, 31, 33, and 34 correct and neat.

G. W.—Solutions correct. Have sent address.

A. McDonnell.—25 and 33 correct. 34 to Kt to Kt3. Have corrected misprint.

Edward Sargent.—Solutions correct except No. 35, if Q to B3, then P takes P, and there is no mate.

W. Byng.—30 and 31 incorrect, 32 correct.

R. G. Brothers.—Game received with thanks, and will give it full consideration.

Henry H. Higgins.—Received with thanks.

Leonard P. Rees.—In the position of the Evans' Gambit, brought about by 1. PK4; PK4. 2. KtKB3; KtQB3. 3. BB4; BB4. 5. PQKt4; B takes P. 6. PB3; BB4. 7. Castles; PQ3. 8. PQ4; BKt3. White would proceed with 9. P takes P; P takes P (best). 10. Q takes Q; having a slight superiority in position. (If 9.... Kt take P. 10. Kt takes Kt; P takes Kt. 11. B takes BP(ch)! In your diagram, the Rook's Pawns were omitted. Many thanks for problems.

Moleque.—25, 33, and 34 correctly solved; 30 incorrect.

This is Macaulay's description of small-pox in the seventeenth century, when it has been computed that 300 persons in every 1,000,000 died annually of the disease :—"The small-pox was always present, filling the church-yards with corpses, leaving on those whose lives it spared the hideous traces of its power, turning the babe into a changeling at which the mother shuddered, and making the eyes and cheeks of the betrothed maiden objects of horror to her lover." Such facts may be commended to the attention of those who doubt the great value of the improvement brought in by the introduction of vaccination.—*Monthly Record.*

Our Whist Column.

By "Five of Clubs."

THE HANDS.

A	Y
Spades Q, 3, 2	Spades A, Kn, 5.
Hearts 9, 8, 5, 2	Hearts A, 3.
Clubs A, Q, 10, 7, 4	Clubs 3.
Diamonds Kn	Diamonds A, K, 10, 8, 5, 4, 2.

B	Z
Spades 10, 9, 4	Spades K, 8, 7, 6.
Hearts K, Q, 10, 4	Hearts Kn, 7, 6.
Clubs K, Kn, 9	Clubs 8, 6, 5, 2.
Diamonds 7, 6, 3.	Diamonds Q, 9.

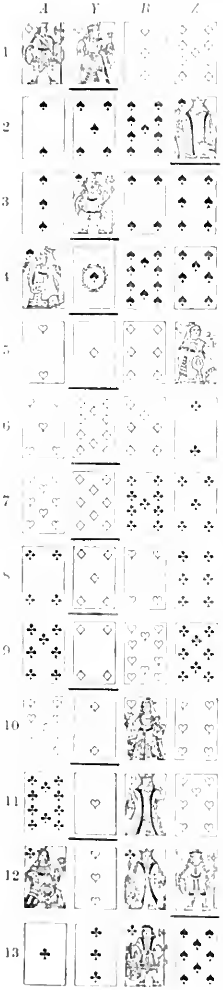
B
Y Z Dealer
Trump Card, Spade Eight
A

Score: A B, 4; Y Z, 0.

THE PLAY.

Note.—The underlined card wins trick, and card below it leads next.

A Y B Z



REMARKS AND INFERENCES.

1. A leads our old friend the singleton. The score being at 4, he hopes to get a trick or two by ruffing, and to secure the odd trick and the game. Y, from his own hand, and from his familiarity with A's way (who, however, apart from his weakness for a singleton lead, is a strong player) knows that A has not led from strength, he therefore, though with only three trumps,

2. Leads a trump. B probably holds 10 and a small one (A being presumably weak in trumps).

3. The finesse here is perfectly sound. Z has returned the 6 of Spades, 8 being the trump card, therefore he held four originally. It is very unlikely that B holds Queen. With Queen, 10, 9, and small one, he would not have played 9 to trick 2.

5. Having cleared out trumps, except his partner's turn-up card, Y proceeds with his long suit.

7, 8, 9, and 10. B's discards are bad. A, having discarded Hearts, can be strong only in Clubs (for there has been no such overwhelming trump strength against A and B as to justify A in discarding from his best suit). B therefore should have retained his Hearts; he certainly should not have unguarded his King. It would have made no difference so far as game was concerned; but as it is, Y Z make every trick. Had A originally led Ace of Clubs, Y Z would have made no more than the odd trick.

Solutions of Problem III. by W. N., A. J. K., Spencer Cox, jun., S. J. Allen, M. Michaels, R. C. T., correct. J. B. Harston. Problem sound; try again.

Solutions of Problem IV. by T. D. M., J. L. P., R. J. P., David Maxwell, W. F., G. T. Brown, R. C. T., Jack, A. J. K., G. Brown, correct; Moleque not quite correct. Several correspondents consider Editor in Chief's objection just; but it is not. The problem is quite sound. Five of Clubs.

D. O'P. Miley. With such limitations, the intermediate call seems worth adopting. (After all, there are similar limitations in the case of nearly all signals.)—Five of Clubs.

Mogul considers the hand in No. 22 an unsatisfactory illustration of the weakness of lead from short suit; and that the tricks would have been identical, though played in a different order. This seems to me an incorrect view. If A had indicated his strength in trumps, B, though he could not have returned trumps, would have led a heart after his diamonds were established. As the game was actually played, B had nothing to show that at trick 6 he should have led a heart. Of course if he had done so, the game would have been saved (as Mogul points out) in spite of A's bad lead.—Five of Clubs.

How Nutmegs Grow.—Nutmegs grow on little trees which look like pear trees, and are generally not over twenty feet high. The flowers are very much like the lily-of-the valley. They are pale, and very fragrant. The nutmeg is the seed of the fruit, and mace is the thin covering over the seed. The fruit is about as large as a peach. When ripe it breaks open and shows a little nut inside. The trees grow on the islands of Asia and tropical America. They bear fruit for seventy or eighty years, having ripe fruit upon them all the seasons. A fine tree in Jamaica has over 4,000 nutmegs on it every year. The Dutch used to have all this nutmeg trade, as they owned the Banda Islands, and conquered all the other traders, and destroyed the trees. To keep the price up, they once burned three piles of nutmegs, each of which was as big as a church. Nature did not sympathise with such meanness. The nutmeg pigeon, found in all the Indian islands, did for the world what the Dutch had determined should not be done—carried those nuts, which are their food, into all the surrounding countries, and trees grew again, and the world had the benefit.

Contents of Knowledge *No. 24.*

NOTICES.

The First Volume of Knowledge will be published early in June next, bound in red cloth, gilt lettered. Price 10s. 6d. Vol. I. will comprise the numbers from the commencement (Nov. 4, 1881) to No. 30 (May 26, 1882). As there is only a limited number of copies, the Publishers advise that orders should be sent in without delay, to prevent disappointment.

Binding Cases for Volume I. will also be published, price 1s. 6d. each. Complete copies bound (including case) for 2s. 6d. each.

The Back Numbers of Knowledge, with the exception of Nos. 2, 3, 4, 5, and 6 are in print, and can be obtained from all booksellers and newsagents, or direct from the Publishers. Should any difficulty arise in obtaining the paper, an application to the Publishers is respectfully requested.

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KNOWLEDGE
AN ILLUSTRATED
MAGAZINE OF SCIENCE
PLAINLY WORDED—EXACTLY DESCRIBED

LONDON: FRIDAY, APRIL 28, 1882.

CONTENTS OF No. 26.

TO OUR READERS.

IN our original Prospectus we explained what KNOWLEDGE was intended to be and to do. We come now before our readers and the public to tell them the results of our first half-year's experience, to renew our promises, and to refer in some degree to our performance.

In our first number we announced our intention to publish Original Articles, Serial Papers, Scientific News, a Correspondence Section (including columns of Notes and Queries), and Reviews of Scientific Treatises, suitable for general reading. We indicated the tone which we desired to attain—sound, yet clear, that all might understand, yet none be offended by seeing the truths and discoveries of science dealt with unworthily. We desired to avoid the Scylla of pedantry on the one hand, and on the other with equal watchfulness the Charybdis of triviality. We promised in addition sections for Mathematics and the scientific recreations, Whist and Chess.

In future volumes we shall aim at the same objects, and endeavour to attain them in the same way. The improvements which began with No. 10 (the first number for January) will be continued, and, as occasion arises, will be extended. But while we wish to improve in matters of detail, we see no occasion to modify our general mode of dealing with any sections in KNOWLEDGE, save two:—First, we propose to enlarge and improve our Notes on Art and Science,—in other words, our Scientific News; secondly, we must, perforce, limit the space we allot to Correspondence, and especially to Queries and Replies. These last have not only taxed our space unduly, but have still more seriously trespassed on our time; and with our rapidly growing circulation, the difficulty threatens to increase. We should be very glad to devote ten or twelve pages to these sections, for the sake of the hundreds who ask questions, if we could do so without unfairness to the thousands who wish to see KNOWLEDGE devoted to wider interests. But a paper large enough to do all that we feel bound to do for these, and all that we should like to do for those, would be in a pecuniary sense so much the greater loss as it was more widely patronised; and this is a point which we are bound to take into consideration. We shall still, however, keep our columns open for concisely-written letters: we shall assign a certain space, which must not be exceeded, to the correspondence section, considering letters received in the order of their merit, importance, and conciseness.

Comparing the first number with any of the later numbers, or the first two with the last two monthly parts, we believe we can claim to have decidedly done more than we promised in so far as the earlier numbers were to be regarded as indicating our plan and scope. We hope that this advance (which has not been correctly seen in new periodicals) will be a feature of KNOWLEDGE for many years to come: that in quality, as well as in circulation, KNOWLEDGE the Magazine, like the Knowledge spoken of by Tennyson, may "grow from more to more." Our readers can help us in this, as many have already done, by making our Magazine widely known to their friends.

On a point which has been rather warmly discussed by a small section of our readers, our decision is unwavering. We shall continue to exclude from our columns with equal rigour attacks on religion from the side of science, and attacks on science from the side of religion. We believe that, as we said in our first number, the study of science implies the surest belief that God's works are worth studying, the fullest recognition that the Author of these works is worthy of our reverence; but the intermixture of scientific research and dogmatic religion can only result, as Bacon has well said, "in heretical religion and fantastical philosophy." THE EDITOR.

NEWTON AND DARWIN.

IN Charles Darwin science has lost one who has done more than any since Newton to extend men's recognition of the wideness of the domain of law. When Copernicus and Kepler and Newton removed the earth from the central position in the universe, which had so long been assigned to it, they taught men to appreciate more justly than before the vast extension of the universe in space. The earth, which had seemed to surpass in importance every orb in existence, was seen to be a mere point in the solar system, and in turn the solar system was seen to be a mere point in the universe of stars, the stellar system (though so vast, that it appeared infinite by comparison with all that men had heretofore imagined respecting space), to be as nothing in the real universe. With the widening of men's ideas respecting space should have come a widening of their ideas respecting time, until from the few thousands of years over which they had extended their survey, they had learned to recognise the millions of years belonging to the lifetime of a planet, the far longer intervals measuring the duration of solar systems, and finally the eternities in which these periods of time, vast though they seem, are utterly lost. But with this widening of men's conceptions as to space and time, should have come also a widening of their ideas respecting the operation of law. Within the petty domains of space which they had surveyed, the growth of a plant or of an animal seemed naturally to belong to the domain of development; but wider and grander processes of evolution seemed as far outside men's thoughts as the infinite star depths in which modern science believes, or the vast périods of time during which modern science sees that planets and solar systems have existed. Newton taught men how wide in *space* is the domain of law, and rightly understood, what Newton taught should have shown men how long also in *time* last processes of development according to fixed law. Yet precisely as men were far readier to accept the doctrine of infinite (or practically infinite) space, than that of inconceivably vast periods of time, so also were

they far readier to believe in a law like that of universal gravitation, operating throughout regions of space practically without limit, than to perceive (what this in fact implied) that in time, as in space, the domain of law must be to our conceptions—limitless.

It came, therefore, as a shock even to many of the more thoughtful among us, when Darwin propounded a law of Nature, less grand than Newton's great law, in that instead of ranging visibly throughout infinitudes of space, it dealt only with the families of living creatures inhabiting this earth, but grander in relation to time (or rather in its more obvious relation to time), in that it required us to believe in processes of development operating throughout tens and hundreds of thousands—nay, throughout millions of years. Men were not prepared to extend on a sudden their conceptions of time in the same degree, or in anything like the same degree, that they had perforce extended their conceptions of space. The more profound, indeed, had already seen that this was only a logical consequence of the widening of our ideas of space,—that the vastness of God's domain involved its correspondingly extended duration. But Darwin was the first to give definite tangible evidence of the practically infinite extension of time during which the processes going on all around us have gone on in the past, and (presumably) will go on in the future. The universe, as Newton presented it, might have been framed in a second by an Almighty Being, to last an hour, or a year, or a century, and then to be replaced by some new order of things: though everything in it, even as thus presented, suggests that in duration, as in extent, it was, and is, and will be infinite to our conceptions. But Darwin showed the traces of long-past æons, of long-past æons of æons, traces affording evidence as clear to the eye of science as is the evidence of the vastness of space afforded by the faint rays of telescopic stars.

I do not know whether the grandeur of the universe, as pictured by Newtonian astronomy, or the vastness of past and future time, as pictured by the Darwinian system, is the more impressive. Certainly there can be imagined nothing much more wonderful than those vast depths of space in which we are absolutely compelled to believe since Newton established the great law which bears his name. But if there is aught grander than this, aught more solemn in its impressiveness, it is the thought of the immeasurable vistas of past time, during which the races inhabiting earth came into being under the action of the laws assigned to them; the still vaster time-intervals belonging to the generation of systems of worlds; the periods so vast that we cannot regard them otherwise than as infinite, during which not solar systems, but whole galaxies of such systems, and systems of such galaxies—nay, higher and higher orders of such systems, absolutely without end, as without beginning—came into existence.

That this widening of our conceptions of time as of space, and thence the widening of our ideas as to the domain of law, and consequently the recognition of the infinitely perfect natrre of the laws of the universe (for only very excellent laws can work for long, and only perfect laws can work for ever) should have been regarded as antagonistic to religion in its wider and nobler sense, can only be regarded as resulting from the blindness, or the perversity, or the wrong-headedness, of the ignorant. That some of the fancies of dogmatic religion, some parts of the complex systems which the Rabbinistic type of erudition has invented in all religions, should seem incompatible with these developments of our knowledge and still wider enlargements of our conceptions, can be understood. But *that* religion, in which all men may (in which all reasoning men *must*) agree, has been rendered infinitely grander—infinitely more impressive by our new knowledge. It has also been rendered infinitely more reasonable. Men had spoken of God as Omnipresent and Almighty, but they had assigned a mere point in space as his domain; they had described him as Eternal, but they had recognised his influence as existing for the merest second of time; and finally they had in words attributed all Wisdom to Him, while in fact they had limited His wisdom to the provision of laws capable of operating but imperfectly, and for a brief period. Science shows now the infinite domain of the Omnipresent, its inconceivably vast duration, the perfection of the laws which so rule it that they operate throughout all space and all time. Yet a few who cannot raise their eyes from this petty earth to the heavens, or extend their thoughts to perceive the perfection of the laws governing a universe for all time (as we know time) find no nobler teaching in these grandest revelations of science than that "God is set on one side in the name of universal evolution." It is as though men who had observed but the working of a clock's escapement should regard the discovery of the train of wheels leading to the escapement-wheel proof positive that no reasoning mind had fashioned the mechanism. That which the bigoted on either side, the religious and the irreligious fanatic alike, agree in regarding as the disproof—if admitted—of a Being working "in and through all things," affords in reality the most overwhelming evidence, the solemnest demonstration that such a Being exists: though science must say of Him now as was said of old by Elihu, "as touching the Almighty we cannot find him out."

CONSUMPTION.*

By Prof. Tyndall.

On March 24, 1882, an address of very serious public import was delivered by Dr. Koch before the Physiological Society of Berlin. It touches a question in which we are all at present interested—that of experimental physiology—and I may, therefore, be permitted to give some account of it in the *Times*. The address, a copy of which has been courteously sent to me by its author, is entitled "The Etiology of Tubercular Disease." Koch first made himself known by the penetration, skill, and thoroughness of his researches on the contagium of splenic fever. By a process of inoculation and infection he traced this terrible parasite through all its stages of development and through its various modes of action. This masterly investigation caused the young physician to be transferred from a modest country practice, in the neighbourhood of Breslau, to the post of Government Adviser in the Imperial Health Department of Berlin.

From this department has lately issued a most important series of investigations on the etiology of infective disorders. Koch's last inquiry deals with a disease which, in point of mortality, stands at the head of them all. If, he says, the seriousness of a malady be measured by the number of its victims, then the most dreaded pests which have hitherto ravaged the world—plague and cholera included—must stand far behind the one now under consideration. Koch makes the startling statement that one-seventh of the deaths of the human race are due to tubercular disease, while fully one-third of those who die in active middle age are carried off by the same

* On account of its importance, we assign to this letter by Prof. Tyndall a place in the section of Knowledge usually devoted to original communications only.—Ed.

cause. Prior to Koch it had been placed beyond doubt that the disease was *communicable;* and the aim of the Berlin physician has been to determine the precise character of the contagium which previous experiments on inoculation and inhalation had proved to be capable of indefinite transfer and reproduction. He subjected the diseased organs of a great number of men and animals to microscopic examination, and found, in all cases, the tubercles infested with a minute, rod-shaped parasite, which, by means of a special dye, he differentiated from the surrounding tissue. It was, he says, in the highest degree impressive to observe in the centre of the tubercle-cell the minute organism which had created it. Transferring directly, by inoculation, the tuberculous matter from diseased animals to healthy ones, he in every instance reproduced the disease. To meet the objection that it was not the parasite itself, but some virus in which it was imbedded in the diseased organ, that was the real contagium, he cultivated his *bacilli* artificially, for long periods of time, and through many successive generations. With a speck of matter, for example, from a tuberculous human lung, he infected a substance prepared, after much trial, by himself, with the view of affording nutriment to the parasite. Here he permitted it to grow and multiply. From this new generation he took a minute sample and infected therewith fresh nutritive matter, thus producing another brood. Generation after generation of *bacilli* were developed in this way, without the intervention of disease. At the end of the process, which sometimes embraced successive cultivations extending over half a year, the purified *bacilli* were introduced into the circulation of healthy animals of various kinds. In every case inoculation was followed by the reproduction and spread of the parasite and the generation of the original disease.

Permit me to give a further, though still brief and sketchy, account of Koch's experiments. Of six guinea-pigs, all in good health, four were inoculated with *bacilli* derived originally from a human lung, which, in fifty-four days, had produced five successive generations. Two of six animals were not infected. In every one of the infected cases the guinea-pig sickened and lost flesh. After thirty-two days one of them died, and after thirty-five days the remaining five were killed and examined. In the guinea-pig that died, and in the three remaining infected ones, strongly pronounced tubercular disease had set in. Spleen, liver, and lungs were found filled with tubercles; while in the two uninfected animals no trace of the disease was observed. In a second experiment, six out of eight guinea-pigs were inoculated with cultivated *bacilli*, derived originally from the tuberculous lung of a monkey, bred and re-bred for ninety-five days, until eight generations had been produced. Every one of these animals was attacked, while the two uninfected guinea-pigs remained perfectly healthy. Similar experiments were made with cats, rabbits, rats, mice, and other animals, and, without exception, it was found that the injection of the parasite into the animal system was followed by decided, and, in most cases, virulent tubercular disease.

In the cases thus far mentioned inoculation had been effected in the abdomen. The place of inoculation was afterwards changed to the aqueous humour of the eye. Three rabbits received each a speck of *bacillus*-culture, derived originally from a human lung affected with pneumonia. Eighty-nine days had been devoted to the culture of the organism. The infected rabbits rapidly lost flesh, and after twenty-five days were killed and examined. The lungs of every one of them were found charged with tubercles. Of three other rabbits, one received an injection of pure blood-serum in the aqueous humour of the eye, while the other two were infected, in a similar way, with the same serum, containing *bacilli* derived originally from a diseased lung, and subjected to ninety-one days' cultivation. After twenty-eight days the rabbits were killed. The one which had received an injection of pure serum was found perfectly healthy, while the lungs of the two others were found overspread with tubercles.

Other experiments are recorded in this admirable essay, from which the weightiest practical conclusions may be drawn. Koch determines the limits of temperature between which the tubercle-*bacillus* can develope and multiply. The *minimum* temperature he finds to be 86° Fahrenheit, and the *maximum* 104°. He concludes that, unlike the *bacillus anthracis* of splenic fever, which can flourish freely outside the animal body, in the temperate zone animal warmth is necessary for the propagation of the newly-discovered organism. In a vast number of cases Koch has examined the matter expectorated from the lungs of persons affected with phthisis, and found in it swarms of *bacilli*, while in matter expectorated from the lungs of persons not thus afflicted he has never found the organism. The expectorated matter in the former cases was highly infective, nor did *drying* destroy its virulence. Guinea-pigs infected with expectorated matter which had been kept dry for two, four, and eight weeks respectively were smitten with tubercular disease quite as virulent as that produced by fresh expectoration. Koch points to the grave danger of inhaling air in which particles of the dried sputa of consumptive patients mingles with dust of other kinds.

It would be mere impertinence on my part to draw the obvious moral from these experiments. In no other conceivable way than that pursued by Koch could the true character of the most destructive malady by which humanity is now assailed be determined. And, however noisy the fanaticism of the moment may be, the common sense of Englishmen will not, in the long run, permit it to enact cruelty in the name of tenderness, or to debar us from the light and leading of such investigations as that which is here so imperfectly described.

PHOTOGRAPHY FOR AMATEURS.

By A. Brothers, F.R.A.S.

PART IV.

It is not at all necessary that the amateur should make collodion—it can be purchased of many makers, of excellent quality. If bought in small quantities it will be ready for use, but if a pint or more be procured, the collodion and iodizing solutions should be kept separate, and a few ounces mixed a day or two before required for use.

The glass plates and collodion being ready, we have next to consider the "bath," as the solution of nitrate of silver is called. The same term is also applied to the vessel used to contain the silver solution. This vessel may be of glass, porcelain, or vulcanite, or it may be of varnished wood, in the form called a "well-bath," but glass should have the preference—there are objections to each of the others. The capacity of the vessel being ascertained—say 20 ounces of water—that quantity should be placed in a bottle, and $1\frac{1}{2}$ ounces of re-crystallised nitrate of silver should be added, and allowed to dissolve. The water used should be distilled, or if that cannot be obtained, rain water may be substituted. This silver solution requires to be iodized, that is a weak solution of iodide of ammonia or potassium must be added, a few drops only, or, which will answer equally well, a plate coated with collodion may be left in the solution for a

few hours, when it will be found that the creamy appearance of the plate, which may have been noticed two or three minutes after it was placed in the solution, will have disappeared. This "creamy" appearance is caused by the conversion of the iodide in the collodion into iodide of silver. Unless the silver solution be treated in this way, it is too active, and no certainty could be placed in the plates. Add one drop of nitric acid, and then the bath should be filtered.

We are now ready to make use of the collodion and silver solution, but all the operations after the plate is coated with collodion must be conducted in a darkened room. In order to practice photography with pleasure and comfort, it is necessary to have a room specially arranged, or, at least, a portion of a room, the window of which may be darkened without much trouble. White light must be quite excluded, and this may be effected by covering a frame with black calico or brown paper, leaving about two or three feet, which may be covered with *yellow paper*. This will admit sufficient light to work by, and the light, passing through the yellow paper, being rendered non-actinic, is harmless, and the sensitive collodion film may be manipulated without fear of injury. The frame may be made removable, but it must fit so as to admit no white light. If convenient, a sink should be arranged, and a supply of water laid on. There should be a shelf for bottles and a table for the bath and other things in use. If the room cannot be used exclusively for photographic purposes a cupboard should be appropriated. One thing to be carefully avoided is *dust*.

If such information should be required, the dealer who supplies the camera and lens will show how to "focus" an an object. The ambitious amateur will probably think the best subject for his first essay should be a portrait, but our advice is decidedly in favour of a more simple subject. If in a room, place a table near a window with a good north light, and on the table place a statuette or any other suitable object. If it is preferred to work out of doors, it matters very little what the object may be; an engraving placed in a vertical position, with the camera arranged "*square*" before it, would be an easy subject.

We are now ready to prepare a plate. Take one of the cleaned plates, and see that it is free from dust. Specs of dust would form small black spots in the negative, for we are about to take a *negative*, "*glass positives*" being quite out of date. Hold the glass by one corner in the left hand, then pour on to it what may appear to be sufficient collodion to cover it. Allow the fluid to flow to each corner, carefully preventing it touching the thumb holding the plate, otherwise, the surplus collodion may run where it is not required; then pour off into the bottle, move the plate "to and fro" for a second or two to prevent the collodion "*setting*" in streaks. The collodion bottle can now be put aside, and the stopper replaced. One corner of the plate may be touched, and it will be seen whether the collodion has *set*. Put the plate at once on to the *dipper*, and slowly lower it into the bath, but do not hesitate. A halt for a moment would cause a line to be marked on the plate which would spoil the negative. The bath should be in such a position with respect to the light that the plate can be easily inspected. After about three minutes, it may be withdrawn from the solution, and if it look "streaky" it is not yet ready, and it must remain in the bath of nitrate of silver until it presents an even surface when withdrawn. When ready, the plate must be placed in the "dark slide" of the camera with the prepared surface downwards, that is, facing the lens when placed in the camera. Up to this point, everything should be done deliberately and without hurry. In hot weather it is desirable that the time between taking the plate from the bath and its development should not be longer than necessary, in order to avoid the drying of the plate or the partial drying. All sorts of troubles arise if too much of the silver solution is allowed to drain away, but we must not linger here to describe what those troubles are. Probably by the time they are first seen the amateur will have become sufficiently expert to detect the causes of his failures.

No rule can be given as to the proper time necessary to "expose" the plate, as much depends on the state of the light; one or two trials should be made. If the picture "flashes" up too quickly, too much time may have been given; and if the image does not appear in a few seconds after the developing solution has been poured on the plate, two little time may have been given—experience alone is required. In a good negative there should be a proper balance in the lights and shades. A very little practice will suffice to determine this point.

The next important operation will be the development of the pictures, and this must be deferred till the next paper. Up to this point the amateur can be getting into working order.

CHARLES R. DARWIN.*

By the Editor.

CHARLES DARWIN, the Newton of Biology, died on Wednesday, April 19, 1882, aged 73 years. He was born on Feb. 12, 1809, at Shrewsbury. His father was Dr. R. W. Darwin, F.R.S.; his grandfather Dr. Erasmus Darwin, F.R.S., author of "The Botanic Garden," "Zoonomia," and other works. Shrewsbury Grammar School may fairly be proud of the circumstance that the most eminent naturalist of the nineteenth century was trained under her care. In 1825 Darwin left Shrewsbury for Edinburgh, where he attended the University lectures for a period of two years, at the end of which he entered at Christ College, Cambridge. He took his degree in 1831. In this year he learned that Captain Fitzroy had offered to share his cabin with any competent naturalist who would accompany him in H.M.S. *Beagle*, which was about to sail on a voyage of circumnavigation. Darwin tendered his services, and doubtless the world owes to this circumstance, more than to any other, the wideness of Darwin's views as a naturalist, and the noble generalisation with which his name will in all future time be associated. The voyage in the *Beagle* has been described by himself in one of the most delightful works in the English language. The charm of foreign travel to a mind imbued as Darwin's was with a sense of the significance of all Nature's teachings, is graphically presented in the "Journal of Researches into the Geology and Natural History of the Various Countries visited during the Voyage of H.M.S. *Beagle* Round the World."

Returning home with shattered health, but with his mind prepared to search successfully into the secrets of Nature, Darwin was in no haste to propound crude or immature speculations. The facts he had observed, seemed, he tells us, to "throw some light on the origin of species—that mystery of mysteries, as it has been called by one of our greatest philosophers." But fanciful imaginings were not the means by which this light was to be concentrated.

* Abridged from a biographical notice (by the Editor of Knowledge) which appeared in the *Daily News* for Friday last.

It would be well if every one who desires to advance the interests of science would bear in mind how our great naturalist proceeded at this stage of his researches. "It occurred to me," he says, "that something might, perhaps, be made out by patiently accumulating and reflecting on all sorts of facts which could possibly have any bearing on it." Perhaps a few months might be thought no unsuitable period within which to arrange and systematise the observations which were available for Darwin's purpose. But no. "After five years' work," he says, "I allowed myself to speculate on the subject, and drew up some short notes. These I enlarged in 1844 into a sketch of the conclusions which seemed to me probable." But even then he regarded his labours as only beginning. He was engaged during many more years in steadily pursuing the great object of his researches. Prevented by impaired health from working continuously for any great length of time, he returned again and again to his labours, affording, as Dr. Lankester has well remarked, "a noteworthy example of what difficulties may be overcome by untiring zeal, great perseverance, and a remarkable amiability and kindness of disposition." During the interval, too, which preceded the publication of his "Opus Magnum," he published many valuable contributions to scientific literature. Among these may be specially mentioned his "Monograph of the Family Cirripedia"—that is of the class of animals to which the familiar barnacles and sea acorns belong. It is strange now to find that this work was spoken of in 1856 as that on which Darwin's future reputation would be founded. "His great work," says his biographer in that year, "and that on which his reputation as a zoologist will doubtless depend, is his 'Monograph on Cirripedia.' The excellent style, the great addition made to the existing knowledge of the family to which it is directed, and the remarkable caution exercised by the author in coming to his conclusions, render this work a model of the manner in which such works should be written." This was high praise, and praise bearing in a specially interesting manner on the estimate we are to form of that great work which was all this time in preparation. It is well to recognise that the chief characteristic of the man who has put forward the most daring biological theory of the present century was "remarkable caution in coming to conclusions."

In the year 1858, when the labours of Darwin on his theory of the origin of species were as yet unfinished, Mr. Wallace, who was then engaged in studying the history of the Malayan Archipelago, sent him a memoir embodying the same general conclusions to which he had himself been led, and requested that he would forward it to Sir Charles Lyell. This memoir was published in the third volume of the "Journal of the Linnaean Society." Sir C. Lyell and Sir Joseph Hooker, both of whom knew of Darwin's work, suggested to him that it would be advisable to publish with Wallace's memoir some brief extracts from his own manuscripts. This was accordingly done, and an abstract — necessarily imperfect, Darwin said — of the new theory of the origin of species by natural selection was published on November 24, 1859. It will be in the recollection of most of our readers with what a storm of mingled ridicule and indignation the new theory was received. Wild views spread on every hand as to its nature, and even those who had the means of mastering Darwin's reasoning joined in misrepresenting and ridiculing his doctrines. A considerable time elapsed before the general public would consent to inform themselves as to the real nature of the theory which they had been all but unanimous in abusing. Yet of this self-same theory, Professor Huxley (who from the beginning was one of its most earnest, eloquent, and laborious advocates) said ten years later before the Royal Institution of Great Britain, that so rapidly had it established itself in favour, that he began to think it would shortly require for its welfare a little healthful opposition. This would not be the place to discuss at length "the theory of natural selection (that is, of the preservation of favoured races in the struggle for existence)." Presented briefly, it amounts to this, that during a long course of descent, species, not only of animals, but of plants, are modified by the selective preservation of slightly varied forms, adapted somewhat better than their fellows to the circumstances in which they are placed. How far this doctrine of the modification of species extends, even Darwin himself has not claimed to assert with confidence; but he went very far. "I cannot doubt," he said, "that the theory of descent, with modification, embraces all the members of the same class. I believe that animals have descended from at most only four or five progenitors, and plants from an equal or lesser number." He looked forward even farther, however. "Analogy would lead me one step further," he said, "namely, to the belief that all animals and plants have descended from some one prototype; but this inference is chiefly grounded on analogy, and it is immaterial whether or not it be accepted. The case is different with the members of each great class, as the Vertebrata, the Articulata, &c., for here we have distinct evidence that all have descended from a single parent." Daring as these views seem even now, it is difficult to recall how much more daring they were when Darwin first propounded them. To a large proportion of the naturalists of our day Darwin's theory seems almost beyond question; the young and rising naturalists in particular, of whom Darwin expected with confidence that they would be able "to view both sides of the question with impartiality," have justified his confidence; but when he announced his theory, there were not twenty living men who were likely to receive it with favour. It was in an especial manner on account of its supposed bearing on religious questions that the Darwinian theory when first propounded was repugnant to the feelings of many conscientious men. Gradually, however, it was felt that the new theory, rightly understood, tended to raise instead of to degrade, as was alleged, our conceptions of the scheme of creation. To quote the noble words with which Darwin concluded his treatise: "From the war of nature, from famine and death, the most exalted object which we are capable of conceiving—namely, the production of the higher animals, directly follows. There is grandeur in this view of life, with its several powers, having been originally breathed by the Creator into a few forms or into one: and that whilst this planet has gone cycling on according to the fixed law of gravity, from so simple a beginning endless forms most beautiful and most wonderful have been and are being evolved."

In the "Origin of Species" Darwin had not actually expressed his views as to the ancestry of man, though he had left them to be very clearly inferred. "It seemed to me sufficient to indicate that by this work 'light would be thrown on the origin of man and his history,'" for this implied that man "must be included with other organic beings in any general conclusion respecting his manner of appearance on this earth." But in the "Descent of Man" Darwin dealt at length and boldly with that subject on which he had hitherto deemed it well to be reticent. He presented man as co-descendant with the catarhine, or "down-nostrilled" monkeys, from a hairy quadruped, furnished with a tail and pointed ears, and probably a climber of trees. Nay he traced back the chain of descent until he found, as the progenitor of all the vertebrate animals, some aquatic

creature provided with gills, hermaphrodite, and with brain, heart, and other organs imperfectly developed. The treatise in which this view is presented falls in no respect behind Mr. Darwin's other great work in closeness of reasoning and grasp of facts. The portion of the work more than one half—bearing on sexual selection, if somewhat less satisfactory and conclusive, forms yet a most important contribution to the wide subject of the genesis of species. The closing words of this treatise may fitly here be quoted. After speaking of the distaste with which many persons would probably regard his conclusions as to the descent of man, and then touching on the hopes which the advance of the human race in past ages seems fairly to justify, he says we are not, however, concerned "with hopes or fears, but only with the truth as far as our reason allows us to discover it. I have given the evidence to the best of my ability, and we must acknowledge, as it seems to me, that man with all his noble qualities, with sympathy which feels for the most debased, with benevolence which extends not only to other men, but to the humblest living creature, with his godlike intellect which has penetrated into the movements and constitution of the solar system—with all these exalted powers—man still bears in his bodily frame the indelible stamp of his lowly origin."

After the publication of his first great work, Darwin continued to gather evidence tending to strengthen his theory. In 1862 he published his remarkable work on the "Fertilization of Orchids;" and in 1867 his "Domesticated Animals and Cultivated Plants, or the Principles of Variation, Inheritance, Reversion, Crossing, Interbreeding, and Selection under Domestication." In 1872 Mr. Darwin published "The Expression of the Emotions in Man and Animals;" in 1875, "Insectivorous Plants;" in 1876, "Cross and Self-Fertilization in the Vegetable Kingdom;" and in 1877, "Different Forms of Flowers in Plants of the same Species." Only last year appeared his work upon earthworms, in which he traced the operations of worms in gradually covering the surface of the globe with a layer of mould.

OUR ANCESTORS.

III.—THE TEUTONS.

By Grant Allen.

It does not seem likely that the Roman occupation left much permanent mark upon the ethnology of Britain. So far as we can judge, the Romans held the soil very much as we ourselves hold India—by a purely military tenure. A little sprinkling of Italian blood may perhaps have been indirectly introduced by the legionaries, though comparatively few even of these were really Roman. Most of them were Gauls, Spaniards, Germans, and Low Dutch peoples; and their influence could only have been felt, ethnographically speaking, in the immediate neighbourhood of the military stations, where a few half-breeds may have mingled scantily here and there with the native population. A more important result of the conquest, however, would doubtless be found in the general amalgamation of the older Celtic and Euskarian elements under stress of the new overlords. There is reason to believe that the greater number of Britons sank into the position of serfs, either employed on the great corn farms into which the land was parcelled out, or in the mines of Cornwall, Sussex, and the Forest of Dean. This grinding and levelling system of slavery must have pressed pretty equally upon Celts and Euskarians, light-haired Belgæ and dark Silurians, the former conquerors and the former slaves. Confused together in such a common serfdom, the two types seem to have coalesced, so that the lighter and numerically weaker Aryan Celts became practically almost merged into the darker and more numerous Euskarians. At least we know that ever since the Roman days, and down to modern times, the so-called Celts of Wales, Cornwall, and the Highlands are, for the most part, dark-haired and dark-skinned people of a more or less distinctly Euskarian physique, intermixed with comparatively few individuals of the true light Aryan type; and, as the races were distinct in the days of Cæsar and Tacitus, the coalescence probably took place during the period of the Roman occupation.

After the Romans were gone, however, a second flood of Aryan immigration began to spread over the land. The new comers were the English and Saxons, two Teutonic tribes of Low Dutch pirates, who then inhabited Sleswick and the coasts of Hanover and Friesland. There is no doubt that the original English were a light-haired, light-skinned, blue-eyed people of the ordinary Aryan sort. They came over in small clans or families, and settled first on the east and south coasts, from the Firth of Forth to Southampton Water, making their way, in most cases, into the interior, as was natural for pirates, by means of the inlets or estuaries. Whether the Teutons utterly exterminated the native Britons or not is a question that has been much debated from the historical point of view; and the weight of mere historical authority is certainly on the side of extirpation. Mr. Freeman and Canon Stubbs are both in favour of the belief that the early English conquerors killed off all the Britons—that is to say, in terms of our present discussion, the mixed Celtic and Euskarian inhabitants of the Romanised province—while Mr. J. R. Green, the very latest writer on the subject, is of opinion that the Britons were simply driven off in the struggle, but not to any appreciable extent absorbed or enslaved by the conquerors. From the anthropological point of view, however, such a belief is absolutely untenable. The existing English people is certainly not a pure Teutonic race, nor anything like one. It is a mixture, partially Teutonic, partially Celt-Euskarian; and to this fixed ethnological fact the history must somehow or other be accommodated. Every competent anthropologist, from the days of Phillips and Thurnam to the days of Professors Huxley and Rolleston, has consistently maintained that thesis. It is impossible to twist the evidence of plain modern facts to suit the supposed history, but it is very easy to reconstruct the history so as to accord with the existing facts.

The earliest English settlements were undoubtedly made along the coasts of Kent, Sussex, East Anglia, and Yorkshire. In Sussex, it seems as though the Saxon invaders did really drive away almost all the "Welsh" into the forest of the Weald, where their descendants may still, perhaps, be found; but elsewhere the Britons appear to have been partially subdued and enslaved. In Kent, where a body of Jutes landed, the dark type is still quite common; while, in old interments of the heathen age, Jute and Briton are still recognised side by side, the anatomical peculiarities of their skulls being distinctly recognisable to a technical eye. In the plain of Yorkshire, Professor Phillips long ago pointed out that two very different types of physique still prevail, the one tall and light, of English or Danish origin; the other short, squat, dark, and black-eyed, of British or Euskarian origin. Similar dark people are also common among the supposed pure English of Lincolnshire and East Anglia; while they are not infrequent in the oldest settled parts of Wessex, about Hampshire, Wiltshire, and the Isle of Wight. In fact,

there is good ethnological reason for believing that, even in this most English part of England, the first Teutons did not wholly drive away the Britons, but conquered and enslaved some of them. This belief is fully countenanced by the few historians who have handed down to us some meagre traditional account of the English settlement; for both the Welsh monk, Gildas, who wrote a hundred years after the landing of the English in Kent, and the English monk, Bæda, who wrote nearly a century later, inform us that some of the Britons gave themselves up as slaves to their conquerors. No doubt such slaves would be quickly Teutonised in creed, and Anglicised in speech; but from the ethnological point of view a Euskarian is a Euskarian still, whatever religion he may happen to profess, or whatever language he may happen to speak. His tongue or faith would produce no immediate change in the colour of his skin and eyes. To this day, indeed, the darker people in the east of England are mainly to be found among the peasantry.

The midland districts of England were slowly conquered by the English setting out from their earliest colonies on the coast; and as they moved inward, they appear to have spared more and more of the native Britons at each advance, and even to have substituted political subjugation for personal slavery. For example, it seems likely that the West Saxons landed in Southampton Water about fifty years after the Jutish conquest of Kent. They settled in Hampshire after some years' hard fighting, but more than half-a-century elapsed before they conquered Old Sarum and occupied Wiltshire. Still more slowly did they proceed across Dorset and Somerset, reaching Bath after nearly a century, Bradford after a century and a half, and Taunton after two centuries. In these two counties the proportion of Celt-Euskarian blood is very strong; in Devon, which was only finally annexed more than three hundred years after the first landing, the Teutonic element even now represents a mere fraction. As to Cornwall, that of course retained even its Celtic speech till the last century, as some parts of Devon did till the reign of Queen Elizabeth. Indeed, Alfred the Great in his will describes all the people of Wilts, Somerset, Dorset, and Devon as Welsh-kind. This one example will show the comparatively small amount of Teutonic blood that the English invasion actually brought into the country. It was just the same elsewhere. In the Severn valley, for instance, Welsh and English coalesced very early, and the people of Gloucestershire, Worcestershire, Shropshire, and Cheshire belong very largely to the dark type, while those of Herefordshire and Monmouthshire are purely Welsh by blood. So in the north, a great Welsh kingdom of Strathclyde long held out between Glasgow and the Mersey, and when at last it was conquered by the English of Northumbria, its people still remained upon the soil as subject inhabitants. To this day, the dark type is common in Lancashire, Ayrshire, and the hill districts of the West Riding, though in Cumberland and Westmoreland there is a large later infusion of light Scandinavian blood, about which more hereafter.

Thus, the English occupation was really, to a great extent, rather a mere Teutonisation of Britain than an extermination of the original Britons. The light Aryan stock, no doubt, received a large accession of strength; but the dark Euskarian stock was not by any means annihilated or driven away. In Sussex, Essex, and the Lothians, the English seem to have settled very thickly, and to have spared very few of the native Britons, though we must remember that these parts were probably inhabited for the most part by fairly pure Celts (not Euskarians), whose descendants we cannot now discriminate from those of the equally Aryan Teutons. In Yorkshire, Lincolnshire, East Anglia, Kent, and Hampshire, the conquerors apparently enslaved a considerable number of the dark serfs whom they found upon the soil; and their type is still preserved amongst the peasantry of those districts. As we move westward and inland, however, we find fewer and fewer pure English, mixed with a larger and larger proportion of dark natives. In the eastern midlands, the light type is commonest; in the western midlands and the Severn valley, the dark type distinctly predominates. In Devonshire, Herefordshire, Lancashire, and Ayrshire, a few English overlords seem, after a long struggle, to have settled at last among a very large subject population. And finally, into Cornwall, Wales, and the Highlands, the English never penetrated at all, except as purely political conquerors. But we must leave over for another paper the settlements of the Scandinavians in Scotland, the Lake district, and Ireland, as well as the existing distribution of the ethnical elements in the British Islands of our own day.

THE CRYSTAL PALACE ELECTRICAL EXHIBITION.

Ninth Notice.

Amidst all the brilliancy of the electric light display, it is scarcely to be wondered at that several exhibits—indeed, we may say several classes of exhibits—pass almost unnoticed. Even in walking through the nave—the principal part of the Palace—we can observe a number of stalls richly laden with apparatus, more or less interesting and unique, around which hardly a person is to be seen; while, if we turn our eyes to either the Swan, Edison, Brush, or other electric light exhibits, we behold always a crowd of visitors, all more or less interested in what they are so intently gazing on.

One of the displays having great interest for the scientific visitor is that of Messrs. Blakey, Emmott, & Co., of Halifax. They have two stalls one in the nave, close to the Post-office exhibit, and the other in the gallery. Their exhibits are catalogued in eight out of the fourteen classes into which the exhibition is divided. In Class I., they exhibit apparently excellent frictional machines, of the Winter and other types, and other apparatus for explaining static electricity.

Class II. (Batteries and allied apparatus) is well represented, but here, as in a number of the other classes, there is considerable difficulty in tracing exhibits to their proper class. Apparatus is shown designed for innumerable purposes: but perhaps the greatest amount of interest centres in the telephonic display. A great feature one cannot help noticing is the extreme care that has evidently been taken to give the apparatus the highest attainable point of efficiency, which can only be accomplished by using the best materials, and paying unusual attention to the processes of manufacture and finishing. It must not be imagined, however, that we are championing extravagant apparatus—such for instance, as the very elaborate Crossley Transmitte[r] but our remarks refer more particularly to meaner-looki[ng] apparatus.

Assuming that our readers have seen a telephone circu[it] they will doubtless remember that such a circuit is generally furnished with a call-bell. An electric current is required to work the bell, and Messrs. Blakey & Co. exhibit some well-made apparatus, in which the current is generated by turning a handle, and so revolving a Siemens' armature (see our articles on "Electric Generators") between the poles of strong permanent magnets. The apparatus is very small, but is, nevertheless, said to be capable of ringing a

bell at a distance of 300 miles. The magnets used are very good, and are claimed to be capable of sustaining ten times their own weight.

In the same series of exhibits is a large switch-board (intended for telephone exchange offices), by means of which any one of the fifty subscribers it is constructed to accommodate may be put in communication with either of the other subscribers.

In the "Telegraphs" class there is a tolerably good display of certain forms of apparatus, although none of the high-speed instruments are shown. The British Government manufacture their own apparatus; but railway companies, submarine cable companies, and foreign and colonial administrations (all of which, generally speaking, use the Needle, Morse, and other low speed systems), doubtless give plenty of employment to makers of such instruments as are here exhibited.

In the class set apart for electro-medical apparatus, Messrs. Blakey & Co. have a very good exhibit, embracing several forms of battery, more especially noted for the fittings. Amongst the apparatus may be noted Messrs. Mottershead & Co.'s Leclanché Batteries. They consist of 20, 30, or 50 cells. These cells are made of vulcanite, and are all sealed up after being charged, so that none of the liquid can be spilled. Our readers may be aware that the Leclanché Cell contains no acid, and, if only required for a few minutes at a time, it will last for many months—perhaps years. The reason of this is that, whereas in an acid battery, chemical action takes place whether the electrical circuit is complete or not, in such batteries as the one we are considering, no action goes on unless there is a complete electrical circuit.

The batteries are manufactured in two series, the first being handsomely fitted in mahogany cases, with nickel-plated or gilt accessories, while the second series are fitted up in plain black wood cases. It is asserted that the same cells are used in each series, so that they are equally efficient. The utility of such an arrangement is obvious, and it is to be hoped that other makers will see their way to following so good an example.

A little switch (or, as they call it, a current selecter) is fitted to the case, to enable the operator to bring any required number of cells into action, a set of shunts being also provided for cutting off the initial cells, and so equalising the labour which the various cells are called upon to perform. It is hardly necessary or advisable to enlarge upon the various forms of electrodes exhibited, designed, as they are, for a multitude of purposes, nor can we now enter into a description of the other apparatus in this collection, but it is recommended that our readers visiting the Palace should inspect it for themselves.

There are several other exhibitors of electro-medical apparatus; but grave doubt exists in the minds of electricians, as well as physicians, as to the utility of a considerable proportion of the appliances now before the public, and a great deal remains to be proved before any appreciable amount of faith in them can be said to exist. We should have said more about them, but there are many other subjects which demand our consideration—such as secondary batteries and electro-motors, to one of which we must next direct our attention.

Reclamation.—A correspondent, with a signature so singularly business-like that we can only guess that his name ends with the letters "an," or "om," or "ard," informs us that the information respecting "Accumulative Sinking Fund," at p. 511, is taken without acknowledgment from his "In[illegible]ll's Foreign Slk (or Tlk) Manual." We regret that any correspondent should have failed to mention the source from which information sent to us has been obtained; but, doubtless, the omission was due to inadvertence.

Reviews.

THE STARS AND THE EARTH.*

A CHARMING little work. If there is any way in which the human mind can conceive the possibility of Omniscience, it is the way shown by the author (unknown) of this treatise. He shows that the universe encloses the pictures of the past, like an indestructible and incorruptible record containing the purest and clearest truth. "As sound propagates itself in the air, wave after wave, and the stroke of the bell or the roar of the cannon is heard only by those who stand nearest, in the same moment when the clapper strikes the bell or the powder explodes; but each more distant spectator notes a still greater interval between the light and the sound, until the human ear is no longer able to perceive the sound on account of the distance,—so, according to our ideas, the pictures of every occurrence propagate themselves into the distant æther upon the wings of the ray of light. Thus that record which spreads itself out further and further in the universe, by the vibration of the light, really and actually exists and is visible; but to eyes more powerful than those of man." This is the lesson expounded in the first part of this little book. The second still further illustrates this pregnant idea, by showing that a point of view is conceivable from which the universe no longer requires the expansion of time and space in order to exist and to be intelligible to us, and how with such a point of view we can imagine and completely understand the universe as the work of a single Creator.

Chinese Method of Manufacturing Vermilion.—There are three vermilion works in Hong-Kong, the method of manufacture being exactly the same in each. The largest factory consumes about 6,000 bottles of mercury annually, and it was in this one that the following operations were witnessed:—*First Step:* A large, very thin iron pan, containing a weighed quantity (about 14 lb.) of sulphur, is placed over a slow fire, and two-thirds of a bottle of mercury added. As soon as the sulphur begins to melt, the mixture is vigorously stirred with an iron stirrer until it assumes a black, pulverulent appearance, with some melted sulphur floating on the surface. It is then removed from the fire and the remainder of the bottle of mercury added, the whole being well stirred. A little water is now poured over the mass, which rapidly cools it. The pan is immediately emptied, when it is ready for the next batch. The whole operation does not last more than ten minutes. The resulting black powder is not a definite sulphide, as uncombined mercury can be seen throughout the whole mass; besides, the quantity of sulphur used is much in excess of the amount required to form mercuric sulphide. *Second Step:* The black powder obtained in the first step is placed in a semi-hemispherical iron pan, built in with brick, and having a fireplace beneath, covered over with broken pieces of porcelain. These are built up in a loose porous manner, so as to fill another semi-hemispherical iron pan, which is then placed over the fixed one and securely luted with clay, a large stone being placed on the top of it, to assist in keeping it in its place. The fire is then lighted and kept up for sixteen hours. The whole is then allowed to cool. When the top pan is removed, the vermilion, together with the greater part of the broken porcelain, is attached to it in a coherent mass, which is easily separated into its component parts. The surfaces of the vermilion which were attached to the porcelain have a brownish-red and polished appearance, the broken surfaces being somewhat brighter and crystalline. *Third Step:* The sublimed mass obtained in the second step is pounded in a mortar to a coarse powder, and then ground with water between two stones, somewhat after the manner of grinding corn. The resulting semi-fluid mass is transferred to large vats of water, and allowed to settle, the supernatant water removed, and the sediment dried at a gentle heat. When dry it is again powdered, passed through a sieve, and is then fit for the market.—*Journal of the Society of Chemical Industry.*

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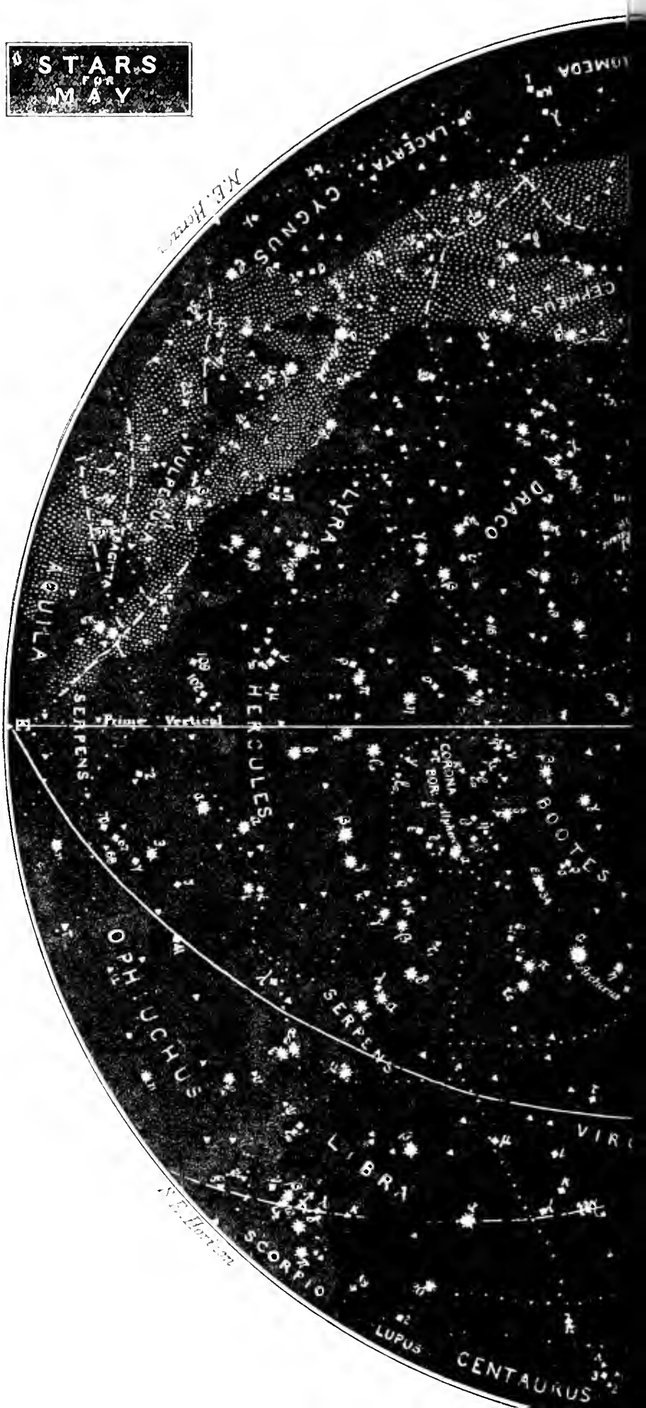
* "The Stars and the Earth: or Thoughts upon Space, Time, and Eternity." (Baillière, Tindall & Co., London.)

STARS FOR MAY

OUR STAR MAP.—The circular boundary of the map represents the horizon. The map shows also the position of the equator and of that portion of the Zodiac now most favourably situated for observation. For the motions of the planets Jupiter, Mars, and Uranus, consult the Zodiacal maps in Nos. 11 and 19. The names of ninety-nine stars of the first three magnitudes are given below.



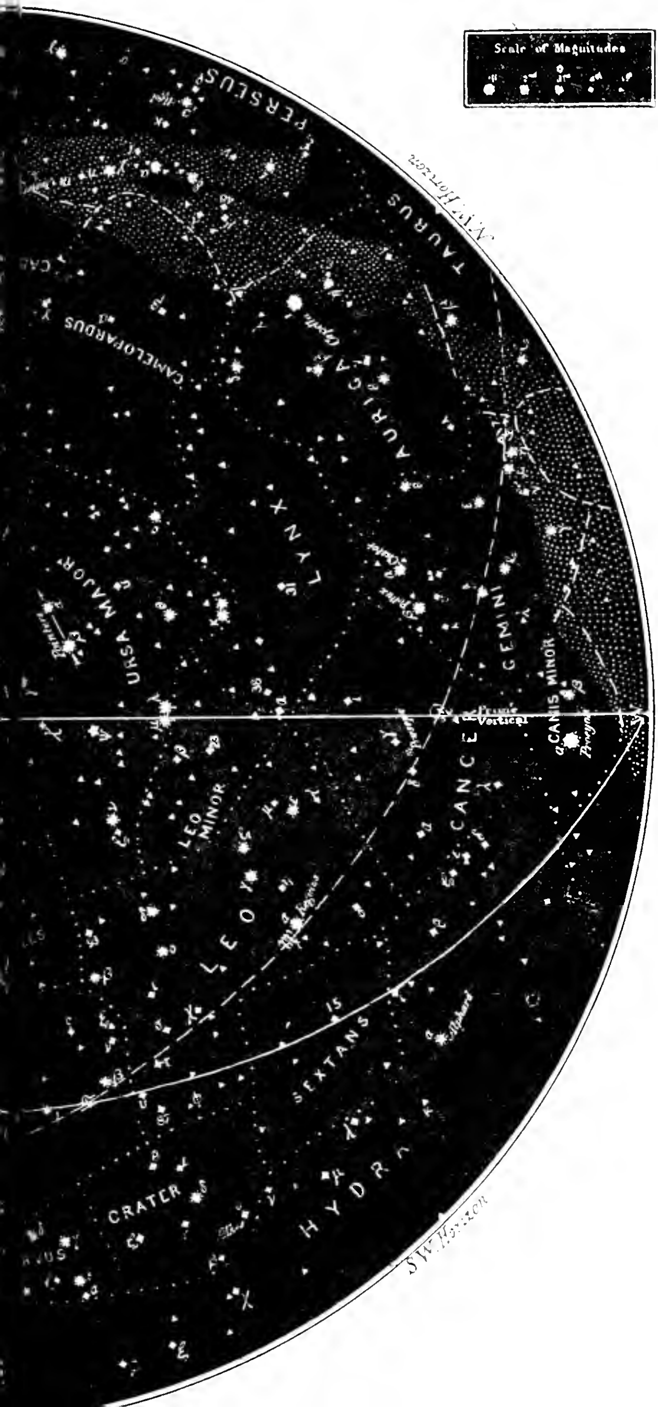
On April 30, at 10.30 p.m.
On May 3, at 10.15 p.m.
On May 7, at 10. 0 p.m.
On May 11, at 9.45 p.m.
On May 15, at 9.30 p.m.
On May 19, at 9.15 p.m.
On May 22, at 9. 0 p.m.
On May 26, at 8.45 p.m.
*On May 30, at 8.30 p.m.
*On June 3, at 8.15 p.m.
*On June 6, at 8. 0 p.m.

* It is daylight at these times.

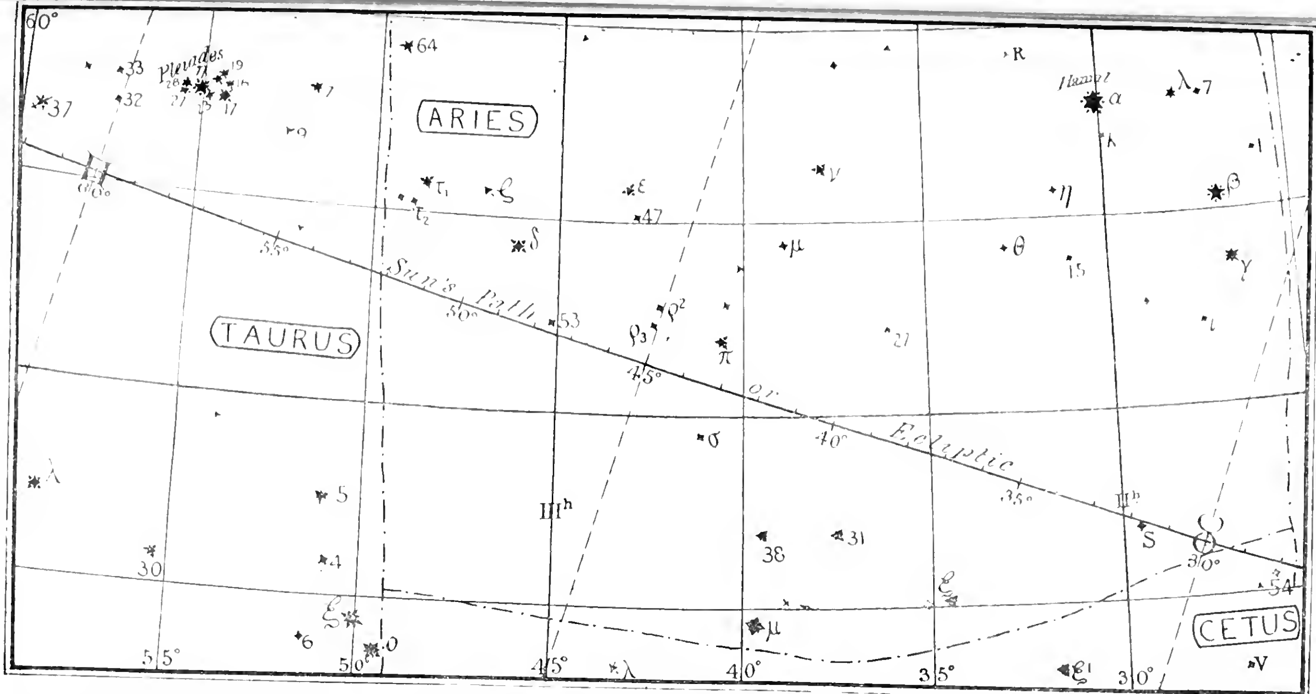
ARABIC NAMES OF STARS.

The following table exhibits the names of all the stars of the first three magnitudes whose names are in common use:—

α Andromedæ	... *Alpheratz*
β ———	... *Mirach, Mizar*
γ ———	... *Almach*
α Aquarii	... *Sadalmelik*
β ———	.. *Sadalsund*
ζ ———	... *Skat*
α Aquilæ	... *Altair*
β ———	... *Alshain*
γ ———	... *Tarazed*
α Arietis	... *Hamal*
β ———	... *Sheratan*
γ ———	... *Mesartim*
α Aurigæ	... *Capella*
β ———	... *Menkalinan*
α Boötis	.. *Arcturus*
β ———	... *Nekkar*
ε ———	... *Izar, Mizar, Mirach*
η ———	... *Muphrid*
α Canum Venat. .	... *Cor Caroli*
α Canis Majoris ...	... *Sirius*
β ———	... *Mirzam*
ε ———	... *Adara*
α Canis Minoris ...	*Procyon*
β ———	.. *Gomeisa*
α² Capricorni ..	.. *Secunda Giedi*
δ ——— . ..	... *Deneb Algiedi*



α	Cassiopeiæ ...	... *Schedar*
β	——— ...	... *Chaph*
α	Cephei	... *Alderamin*
β	———	... *Alphirk*
γ	——— ...	... *Errai*
α	Ceti	... *Menkar*
β	———	... *Diphda*
ζ	———	... *Baten Kaitos*
ο	———	... *Mira*
α	Columbæ	... *Phact*
α	Coronæ Borealis	... *Alphecca*
α	Corvi ...	... *Alchiba*
δ	——— ...	... *Algores*
α	Crateris ...	... *Alkes*
α	Cygni ...	... *Arided, Deneb Adige*
β	——— ...	... *Albireo*
α	Draconis	... *Thuban*
β	——— ...	... *Alwaid*
γ	——— ...	... *Etanin*
β	Eridani	... *Cursa*
γ	———	... *Zaurac*
α	Geminorum	... *Castor*
β	———	... *Pollux*
γ	———	... *Alhena*
δ	———	... *Wasat*
ε	———	... *Mebsuta*
α	Herculis ...	... *Ras Algethi*
β	——— ...	... *Korneforos*
α	Hydræ	... *Alphard, Cor Hydræ*
α	Leonis	... *Regulus, Cor Leonis*
β	——— ...	... *Deneb Aleet, Denebola, Deneb*
γ	——— ...	... *Algeiba*
δ	——— ...	... *Zosma*
α	Leporis ...	... *Arneb*
α	Libræ ...	... *Zuben el Genubi*
β	——— ...	... *Zuben el Chamali*
γ	——— ...	... *Zuben Hakrabi*
α	Lyræ ...	... *Vega*
β	——— ...	... *Sheliak*
γ	——— ...	... *Sulaphat*
α	Ophiuchi ...	... *Ras Alhague*
β	——— ...	... *Cebalrai*
α	Orionis ...	... *Betelgeux*
β	——— ...	... *Rigel*
γ	——— ...	... *Bellatrix*
δ	———	... *Mintaka*
ε	———	... *Alnilam*
α	Pegasi	... *Markab*
β	———	... *Scheat*
γ	———	... *Algenib*
ε	———	... *Enif*
ζ	——— ...	... *Homan*
α	Persei ...	... *Mirfak*
β	——— ...	... *Algol*
α	Piscis Australis	... *Fomalhaut*
ε	Sagittarii	... *Kaus Australis*
α	Scorpionis	... *Antares, Cor Scorpionis*
α	Serpentis	... *Unukalhai*
α	Tauri	... *Aldebaran*
β	———	... *Nath*
η	———	... *Alcyone* (Pleiad)
α	Ursæ Majoris	... *Dubhe*
β	———	... *Merak*
γ	———	... *Phecda*
ε	———	... *Alioth*
ζ	——— ...	... *Mizar*
η	———	... *Alkaid, Benetnasch*
ι	——— ...	... *Talitha*
α	Ursæ Minoris	... *Polaris*
β	——— ...	... *Kochab*
α	Virginis ...	... *Spica Azimech, Spica*
β	——— ...	... *Zavijava*
ε	——— ...	... *Vindemiatrix*



THE SUN IN MAY.

During April, as explained at p. 168, and illustrated by the map of the sun's path in that month, the motion of the sun along the ecliptic had nearly its mean value, being exactly at the mean value on April 1, when the earth was at her mean distance from the sun. Thereafter the sun's motion fell short of its mean value, but not much, even by the end of the month. Thus, the motion of the sun along the ecliptic was 59′ 8·0″, between noon March 31 and noon April 1, the exact mean motion being 59′ 8·193″; between noon April 30 and noon May 1 the mean motion is only 58′ 12·1″. The motion continues to diminish during May, and will only amount to 57′ 27·0″ between noon May 31 and noon June 1. During April, with its 30 days, the sun advances 29° 19′ 15·7″ along the ecliptic, while during the first 30 days of May he advances only 28° 53′ 57·1″, or less by 25′ 18·6″. But whereas the motion in longitude has thus fallen still further below the mean value, the motion in right ascension, which in April had nearly its least rate (because at the first point of Aries, reached by the sun on March 21, the meridians are at their farthest apart, and the inclination of the sun's path to them is at its maximum, in May) both these conditions are changed; the meridians are nearer together (compare the maps for April and May) when the ecliptic crosses them, and the ecliptic lies more squarely across them. In May, in fact, the effect of these conditions has about its mean value, so that whereas on or about March 21 a given motion along the ecliptic corresponds to the least amount of motion in right ascension; and, again, on or about June 20, a given motion along the ecliptic corresponds to the greatest amount of motion in right ascension on or about May 4, 5, or 6, a given amount of motion along the ecliptic corresponds to the same amount of motion in right ascension. Thus the real sun, though losing as compared with the mean sun, during the month of April, does not lose so much as he did (on the average, day by day) in April. The loss of the real sun in right ascension means, as I showed at p. 168, the gain of solar or apparent time on mean or clock time. In April, solar time had changed from being 3 m. 51·01 s. behind clock time to being 3 m. 2·68 s. behind mean time, or 6 m. 56·72 s. during the month. In May, solar time continues to draw ahead of mean time until the middle of the month — or, more exactly, Sunday, May 14, when at solar or dial, noon clock time is 3 m. 52·86 s. past noon. At this time the sun is losing as much by his less rate of motion along the ecliptic as he gains in right ascension by the increasing squareness of his course across the meridians, and their decreasing distance apart, as he increases his distance from the equator. After May 14 the clock gradually gains on the sun, until on May 31, solar or dial time is only 2 m. 36·15 s. ahead of clock time. The reader should carefully study the map for the sun's path in May, and compare it with the corresponding map for April.

THE LAMSON CASE.

Among the affidavits bearing on the case of Dr. Lamson received by Mr. A. W. Mills, the prisoner's solicitor, was one by Dr. H. H. Kane, who has charge of a hospital in New York devoted to the treatment of persons habituated to the use of opium and other drugs. He is described as author of the following works on the subject: "The Hypodermic Injection of Morphia; its History, Advantages, and Dangers," New York, 1879; "Drugs that Enslave; a Study of the Opium, Morphine, Chloral, and Hashisch Habits," Philadelphia, 1881; and "Opium Smoking in America and China," New York, 1882. After mentioning that the majority of his patients are and have been physicians or druggists, and dwelling upon the tendency to carelessness in prescribing morphia and other drugs which he had noticed in the case of those who had become accustomed to use large doses of such drugs themselves, Dr. Kane remarks that, as regards the question of insanity from the habitual use of opium or its alkaloids, more especially morphia, but little definite is known. Insane asylum reports every year record from one to eight or nine cases of insanity attributed to the prolonged use of opiates, and physicians in general practice recognise the use of narcotics as a rare, though well-established, cause of insanity. A person with an hereditary tendency to insanity, or with a mind weakened from any combination of circumstances, or from actual bodily disease, using this drug in large amount for a considerable time, could hardly escape some unsettling of his mental and moral powers. In the majority of instances the insanity thus produced is chiefly marked by weakening of the will power, entire change of the moral tone, loss of business ability, sundering of family ties, and carelessness about the ordinary duties of life. Actual mania, melancholia, and dementia are probably rare, but have undoubtedly occurred from this cause. Some persons inherit or acquire in after-life an idiosyncrasy which renders them more susceptible to the physical, mental, and moral ill-effects of opium, than obtains in the ordinary individual and a like idiosyncrasy has been known to lead to death from doses previously considered safe. This is especially true with reference to the hypodermic use of morphia. Certain persons can take large doses of opium for years with impunity, while others, of a peculiarly nervous temperament, are injured out of all proportion to the time the drug has been used or the amount taken. In the majority of cases, habitual users stop short of actual insanity as ordinarily classed, although they manifest marked deterioration or total abolition of will, power, and memory. A tendency to lie with reference to their habit, inattention to family and business, and the manifestation of a very decided change in moral tone may be marked. Dr. Kane would say, in conclusion, that of all forms of the opium habit, that of hypodermic injection as a rule works the most harm in the shortest time.—*Times.*

Ancient Tablets from Sippara, or Sepharvaim.—Nine cases, representing a portion of the results of the researches just on the point of being resumed by Mr. Hormuzd Rassam, who left this country for Alexandria and Babylon on the 6th instant, have just arrived in London. The tablets which they contain are, for the most part, small, and, either whole or in a fragmentary condition, are estimated to reach about five thousand in number. The texts on the tablets are large beyond precedent, as compared with the size of the vehicle on which they are inscribed. The new importation, so far as it has been investigated, consists chiefly of trade documents, and largely of contracts for the sale or supply of corn and other agricultural products. They are dated in the reigns of Samassumukin and Kandalanu, the Chinladanus of the Greeks, who were contemporary with the latter half of the reign of Assurbanipal, or Sardanapalus, of Assyria, about B.C. 646. The tablets are from Abo-habba, the site of the ancient Sippara, the Sepharvaim of the Old Testament, which is mentioned by Sennacherib in his letter to Hezekiah as one of the cities whose kings had been unable to resist the might of the Assyrians. Sippara—or Pantibiblon, as the Greeks called it—is mentioned by Berosus as having furnished five out of the ten Chaldean kings of the time before the Flood, and as the place where Xisuthrus, or Noah, buried the records of the antediluvian world at the time of the Deluge, and from which his posterity afterwards recovered them. The Hebrew term Sepharvaim, which is the verbal equivalent of the "two Sipparas," is applied to twin cities, one of which is situated on each side of the river. The Sippara from which the tablets just received in London have been procured, is the Sippara of Samas, *Tsipar sha Shamas*, or Sippara of the Sun God, as being a place, *par excellence*, where the sun was a chief object of worship. The other Sippara, or Sippara of Anunit, which is supposed to have contributed in ancient times to name the Sepharvaim of Scripture history, is up to the present moment unknown to modern investigation.

WEATHER DIAGRAM, FOR WEEK ENDING SATURDAY, APRIL 22.

STATION.	ABERDEEN.						
Day of Week.	Sun.	Mon.	Tues.	Wed.	Thurs.	Fri.	Sat.
Thermo'ter in Shade. Max.	43	39	46	49	57	61	58
Thermo'ter in Shade. Min.	27	30	36	35	41	47	37
Thermo'ter in Shade. Day's Range	16	9	10	14	16	14	21
Barometer. Inches 100ths	29·61	29·35	29·62	29·64	29·65	29·92	29·81
Wind. Direction	NW	S	NNW	SSW	W	SSW	0
Wind. Force (0 to 12)	4	1	6	5	1	6	0
Weather. Beaufort Scale*	b	r	b	r	c	b	m u
Rainfall. 100ths of inch.	3	44	8	20	8	0	0

STATION.	LIVERPOOL.						
Day of Week.	Sun.	Mon.	Tues.	Wed.	Thurs.	Fri.	Sat.
Thermo'ter in Shade. Max.	43	50	54	55	60	59	58
Thermo'ter in Shade. Min.	39	42	42	42	46	44	48
Thermo'ter in Shade. Day's Range	4	8	12	13	14	15	10
Barometer. Inches 100ths	29·76	29·39	29·67	29·81	29·89	30·19	29·68
Wind. Direction	SSW	SE	WNW	SW	W	SSE	ESE
Wind. Force (0 to 12)	1	2	4	3	6	2	3
Weather. Beaufort Scale*	b c	o	b	b c	b c	o	c
Rainfall. 100ths of inch.	10	0	2	7	20	0	4

STATION.	VALENCIA.						
Day of Week.	Sun.	Mon.	Tues.	Wed.	Thurs.	Fri.	Sat.
Thermo'ter in Shade. Max.	51	50	56	54	54	57	55
Thermo'ter in Shade. Min.	38	41	44	47	49	50	50
Thermo'ter in Shade. Day's Range	13	6	12	7	5	7	5
Barometer. Inches 100ths	29·64	29·42	29·90	29·70	30·01	30·03	29·32
Wind. Direction	S	WNW	WNW	SSW	WNW	S	SE
Wind. Force (0 to 12)	5	2	2	8	5	6	4
Weather. Beaufort Scale*	o g	o	c	o m u	c q	o	u g
Rainfall. 100ths of inch.	0	26	17	45	40	0	17

STATION.	LONDON.						
Day of Week.	Sun.	Mon.	Tues.	Wed.	Thurs.	Fri.	Sat.
Thermo'ter in Shade. Max.	55	55	58	54	56	65	65
Thermo'ter in Shade. Min.	35	43	45	42	50	40	53
Thermo'ter in Shade. Day's Range	20	12	13	12	6	25	12
Barometer. Inches 100ths	29·80	29·44	29·66	30·05	29·97	30·28	29·72
Wind. Direction	ENE	SSE	SW	S	WSW	SW	SE
Wind. Force (0 to 12)	1	2	3	3	5	2	1
Weather. Beaufort Scale*	b c	o	b c	o	c	b c	o
Rainfall. 100ths of inch.	0	0	10	0	2	0	0

STATION.	LYONS.						
Day of Week.	Sun.	Mon.	Tues.	Wed.	Thurs.	Fri.	Sat.
Thermo'ter in Shade. Max.	49	58	60	57	59	65	70
Thermo'ter in Shade. Min.	39	38	45	44	41	46	50
Thermo'ter in Shade. Day's Range	10	20	15	13	18	19	20
Barometer. Inches 100ths	29·67	29·94	29·99	30·27	30·27	30·33	30·00
Wind. Direction	S	SSW	WNW	NNW	N	N	S
Wind. Force (0 to 12)	1	1	1	1	1	1	1
Weather. Beaufort Scale*	o	c	b c	b c	b	b	b
Rainfall. 100ths of inch.	63	8	12	8	0	0	0

* Weather.—*Beaufort Scale* is, b. blue sky; c. detached clouds; d. drizzling rain; f. fog; g. dark, gloomy; h. hail; l. lightning; m. misty (hazy); o. overcast; p. passing showers; q. squally; r. rain; s. snow; t. thunder; u. ugly, threatening; v. visibility, unusual transparency; w. dew.

Letters to the Editor.

[The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with their writers. All communications should be as short as possible, consistently with full and clear statements of the writer's meaning.]

All Editorial communications should be addressed to the Editor of Knowledge; *all Business communications to the Publishers, at the Office, 74, Great Queen-street, W.C.*

All Remittances, Cheques, and Post-Office Orders should be made payable to Messrs. Wyman & Sons.

**** All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.*

All Letters or Queries to the Editor which require attention in the current issue of Knowledge, *should reach the Publishing Office not later than the Saturday preceding the day of publication.*

(I.) Letters to have a chance of appearing must be concise; they must be drawn up in the form adopted for letters here, so that they may go untouched to the printers; private communications, therefore, as well as queries, or replies to queries (intended to appear as such) should be written on separate leaves.

(II.) Letters which (either because too long, or unsuitable, or dealing with matters which others have discussed, or for any other reason) cannot find place here, will either be briefly referred to in answers to correspondents, or acknowledged in a column reserved for the purpose.

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

"God's Orthodoxy is Truth."—*Charles Kingsley.*

Our Correspondence Columns.

DOES THE MIXTURE OF BLUE AND YELLOW MAKE GREEN LIGHT?

[388]—In Vol. I., page 538, of Knowledge, Mr. W. Benson asserts that chloride of copper affords, before the "blowpipe," a sea-green or verdigris light. The following equally eminent authorities, on the other hand, maintain that this light is blue. (1) Crookes: "Mitchell's Manual of Assaying," page 370, "Chloride of copper alone colours the flame blue;" (2), Plattner, "Probirkunst mit dem Löthrohre," page 269. "The azure-blue flame of chloride of copper:" (3), Berzelius "On the Blowpipe," Whitney's translation, page 82, "Chloride of copper gives a beautiful blue flame, inclining to purple;" and at page 211, writing of atacamite, "alone it tinges the flame intensely blue;" (4), Fresenius, "Qual. Anal., 1872," page 192. "Chloride of copper affords a fine blue-coloured flame, inclining to purple;" (5), Elderhorst, "Man. Bl. Anal.," page 56. "An intense azure-blue colour, owing to the formation of chloride of copper;" (6), Landauer, "Man. Bl. Anal., 1881," page 27. "Copper chloride gives a sky-blue flame-colour." Mr. W. Benson's optical terminology seems to be derived chiefly from Winsor & Newton's colour-box; but if, by the expression "a verdigris light," he means a green light, I would ask where was the necessity of mixing the sodium-yellow with copper chloride-green, to make its flame green?

Similarly, the above authorities (except Mr. Benson) might be cited to show that the blowpipe pyrocone is also blue, not "sea-green;" and, indeed, I suppose that Mr. Benson, like everybody else, has remarked that the sea, within 100 miles of a shelving shore, is scarcely two consecutive days the same colour; grey, green,* yellow, mud-colour, &c., and more than 100 miles from shore, generally blue.

If by the name "ultramarine" he means lapis lazuli, I would remind him that that mineral is by no means pure blue, as it contains a considerable proportion of red, which makes it violetish blue. As regards the "green beam" phenomenon, would it not have been better for Mr. Benson to have taken a prism and lens, before he took a pen in hand, to write about a matter which, by his own admission, he does not understand? As a matter of fact, my Venetian blinds are white, not green; and I tried this experiment oftenest in a small room without any blind at all.

London, W. W. A. Ross.

* Green sea-water seems due to yellow rays, reflected from a sandy bottom, shining through deep-sea-water, which is blue.

SIR E. BECKETT'S INVENTION IN SCREWDRIVERS.

[389]—Many long and hard-worked years have gone by since I gave freely to my profession (that of a surgeon) a small invention of my own, in the form of a gilded spiral spring for treatment and cure of deeply-buried abscesses. It was a tube formed by running a soft metal wire over a mandril in a lathe, exactly resembling the spiral springs of a bell-hanger. This tube possessed a marvellous flexibility and self-adjusting power when inserted in the exit channel of such a sac of purulent matter or of putrefying blood as we not unfrequently meet with, and are at our wits' end to know how to empty. Not emptied, the patient has but a narrow shrift, for pyæmia begins out of this horrible bag of decomposition, and the patient very quickly goes out of the world like a rushlight.

A smart French surgeon conceived the happy thought that such a bag of horrors might be *drained*, and thus began a new practice in our art, called "abscess-drainage," with excellent results in some cases. His tubes were, however, made of vulcanised (that is, sulphuretted) caoutchouc; and giving off a constant, but very minute, quantity of sulphur, and of sulphur-laden gases, the tubes did more harm than good.

It then occurred to me to roll a little gilded wire round a knitting-needle, and withdrawing the stem, there was left to me the prettiest tube of close-siding coils of glittering wire. Try this, thou reader, and you will be as charmed as I. So inconceivably flexible and ready to take any figure you may bend it to is it, that it was but a little step to reach its applicabilities, and to find them quite too delightful. Immediately I found my fortunate moment. There lay—and long had lain—on a beautiful couch, in a lovely bed-chamber, filled with the soft and scented summer air of a grand London suburb, a man still young and full of life, but chained (under all those luxurious belongings) by a worse than Promethean bondage—an abscess, the sac of which lay half-a-foot from the nearest skin surface. The abscess was hopelessly sunk so low that no existing device in surgery could tap that vile reservoir of matter so as to drain it. My wire tubes at once reached the central receptacle, and no words can express the relief obtained, or the admirable ease with which the reservoir was emptied.

Under the name of my wire "drainage-tubes," these toy-like instruments caught the approbation of surgeons, and they are now in general use. By Sir James Paget's commendation, I sent a case to the late American President's surgeons, accompanied by his approval for their excellent work, and they were, I believe, adopted in that sorrowful instance.

I have written all this oddly non-referable memorandum, because I was sure it would interest some of your readers, and being transferred into the immortal pages of Knowledge, never be again forgotten.

Now for you, my excellent and much-adored old friend, Sir Edmund. Why hide the head and shank of your imprisoned screw (all screws ought to be imprisoned, but not hidden)? How can you see with your metal straight-waistcoat over the head and tail of your screw, which way the creature is tending—very likely to right of you, or to left of you, or, by turns (of course), all around of you?

This is the more excellent plan:—Make your guiding tube of springy, well-tempered steel wire, with the coils not too close to each other. Thus, you can see your inward way; the screwdriver head slowly, but surely, thrusts its captive "home," and lightened in its work, as in its self-clearing—you also—the great, wise, and thoroughly typical Englishman of many sciences will gratefully remember?—Your faithful, Robert Ellis.

TRICYCLES.

[390].—Would your correspondent, "Ex-Bicyclist," give us this further information. Has he tried the Omnicycle? And, if so, where does he rank it in comparison with the five or six he mentions? A Would-be Tricyclist.

[We have received several letters for "Ex-Bicyclist," but we have not his address, and we cannot undertake to forward letters to correspondents.—Ed.]

REPLIES TO QUERIES.

[352].—Add nitric acid (not too concentrated) to metallic copper. $3\,Cu + 4\,HO.NO_5 = 3\,CuO\,NO_5 + 4\,HO + NO_2$.—F. G. A.

[312].—Gold.—Put the gold-bedaubed paper into some nitro-hydrochloric acid and heat gently in a glass beaker. Filter carefully and evaporate to dryness, taking care that the acid fumes do not come into contact with anything. The dry residue should be heated, and will then be metallic gold.—F. G. A.

No. 18, p. 376, col. 2—Spinning Top.—Will the Editor kindly give the answer to these four questions, or say where I can find them (the answers)?—F. G. A.

Answers to Correspondents.

**** All communications for the Editor requiring early attention should reach the Office on or before the Saturday preceding the current issue of* Knowledge, *the increasing circulation of which compels us to go to press early in the week.*

Hints to Correspondents.—1. *No questions asking for scientific information can be answered through the post.* 2. *Letters sent to the Editor for correspondents cannot be forwarded; nor can the names or addresses of correspondents be given in answer to private inquiries.* 3. *Correspondents should write on one side only of the paper, and put drawings on a separate leaf.* 4. *Each letter should have a title, and in replying to a letter, reference should be made to its number, the page on which it appears, and its title.*

J. Ralph.—Your problem is not definite. An infinite number of ellipses may be drawn touching two straight lines at two given points in the same.—H. C. Standage. Table of pigments quite unsuited to us.—F. C. M. The mean temperatures in Whitaker are as it were smoothed down, so as to get rid of such peculiarities. But in the mean curve for the last seventy-five years the cold days of April and other three cold days in May are clearly seen.—S. Barber. Could not say till article received. H. Weatherhead. American Exchange, 149, Strand.—Senex. No, we cannot answer. J. Fraser. Your reasoning quite unsound, but it is impossible to explain the matter fully here. The rotation of earth on her axis has nothing to do with the theory.—Dnommah. Awfully paradoxical, as you say.—J. Clift. Thanks on Mr. Muybridge's behalf; but he has already photographed swiftly-running hounds.—T. R. Allinson. Letter received, and shall appear.—M. B. Alder. Gravity *very* essential to Dr. Siemens' theory. More about it hereafter. The change of earth's axial pose due to precession cannot be dealt with briefly: must be very fully illustrated.—A Hater of Superstition considers any reference to "The Almighty," "His glory," "God's ways," and so on, unsuited to a scientific journal. His objections, however, apply only to dogmatic, not to natural religion, which can never be out of place in dealing with science.—F. Miles. For astronomical observation generally, not for observing stars by day.—Antarctic. The low barometer of Antarctic regions was what suggested the theory.—J. Ralph. Cannot give further space to vegetarianism.—Curiosity. Letter received, and to appear when space permits.—J. McDowell. Thanks.—A. H. Empson. You would do well to accept Messrs. Dollond's view of the matter; 80 is a high power for a 2½ inch object-glass.—H. C. Wyatt. We have added Lyons.—J. A. Ollard. A room may be cut off from the rest, by suitable devices.—C. Staniland Wake. I should be willing to lend the pyramid blocks; but the articles are shortly to be published in book form. You will see that in the concluding paper of the article the religious use of the pyramid is referred to.—Geo. Miller.—The stern aspect of the hogs in the Scottish rhymes is amusing; but *lectoribus nostris major debetur reverentia*.—G. Shopland. Fear Newcomb's book is dear, about 18s., I think.—W. Smith. Quite unable to answer letters by post. Do not know who is the editor of the *Scientific American*. It is published weekly by Munn & Co., 261, Broadway, New York. Probably the reports of the Meteorological Society would suit you. It would be invidious to say which we think the best text-book of meteorology, and contrary to our rules.—F. Gibbs. The process is called fermentation, and is now recognised as depending on the development of living germs. The subject is one for an article, and we hope soon to find room for one.—J. Murray. I have endeavoured to show that, in my opinion, the P.D. system is utterly untenable.—A. Benett. You ask us how you can make Balmain's Patent Luminous Paint for your own use? Is the question quite fair? What are patents intended for?—Herbert J. Lamb. The atmosphere so forming round the moon would be of very small density.—John Hamer. Since noted.—Young Astronomer. The subject shall be treated soon. Thanks for encouraging letter. The shape of a halo depends in no way on shape of luminous body.—W. P. Hamsby. We referred to England. There is no evidence of any great perturbation in this country, A.D. 1,000.—Sandford Fleming. Thanks; but "Time Reform" rather a dry subject for readers of Knowledge.—W. H. Shirley. "Nautical Almanac" for 1882, 1883, &c., of Messrs. Murray, price 2s. 6d.—X. A very neat "martingale" will discuss it presently; but, in passing, note that as for every win you score out two figures, and for every loss you add one, you cannot win one "revolution" until the number of wins exceeds the number of losses by the number of figures first set out. The assumption that this must happen before your pockets are cleared out is the "fallacy."—Beccabunga, J. Reid, Erin-go-bragh, Joseph Wallis, Beccabunga, J. Wilson, C. J. Caswell, Photographer, R. F. S., Antarctic, Julias, E. W. Horton, H. D., J. F. R., H. R. L., M. Hargreave, P. K. Pattinson, L. T. P. R. Letters forwarded to contributor on such subjects.

Letters Received.

Sartor Resartus, J. S. P., Thomas Blundein, W. B., Mat, S. C. Wood, J. R. Musgrave, T. Y. S., G. Redfern, K. P. Wallis, M. Shortrede, P. Q., L. T. Turvey, J. C. B., queries already answered, too vague, or otherwise unsuitable; P. Jackson, S. Home (thanks), M. A., Professor of Mathematics, Ch. Harrison, Pertinax, *Sic transit*, L. T. R., Providence, M. Purvis, Excellens, &c.

Our Mathematical Column.

PROBABILITIES.

By the Editor.

Let us next consider cases not altogether so simple. I propose now to establish what may be regarded as the fundamental proposition of direct probabilities. To introduce it, I take the following simple illustrative case:—

Suppose that in an urn there are three white balls and seven black; and in another urn two white balls and three black, what is the chance that when a ball is drawn from each urn, both the drawn balls will be white?

Applying to this problem the two fundamental principles of the science of probabilities, we inquire, first, how many possible events there are, and, secondly, how many are favourable. Now any one of the ten balls in the first urn may be drawn, and any one of the five balls in the second urn. So that any one of the first set of ten balls may appear in company with any one of the second set of five. There are thus 50 (10 times 5) possible events. Again, the pair of drawings may result in giving two white balls in 3 times 2 different ways; since any one of the three white balls in the first urn, by being drawn in company with any one of the two white balls in the second urn, would give the required result. Six events, then, out of fifty are favourable; and, therefore, since any one of the fifty events is as likely to occur as any other, the chance of drawing two white balls is $\frac{6}{50}$ or $\frac{3}{25}$

We can see from the method here applied to a special case that the following general rule may be deduced:—If there are two independent events, and the first can happen in p ways out of $p+q$ all equally likely, and some one of which *must* happen, while the second can happen in p' ways not of $p'+q'$ all equally likely, and some one of which *must* happen; then the chance that both events will happen is $\frac{pp'}{(p+q)\ (p'+q')}$, that is, $\frac{p}{p+q} \times \frac{p'}{p'+q'}$; or the chance of both events happening is obtained by multiplying together the chance of each considered separately.

It follows obviously that if we add a third independent event, which may happen in p'' ways out of $p''+q''$ all equally likely, and some one of which must happen, then the chance that all three events will happen is

$$\frac{p\,p'\,p''}{(p+q)\ (p'+q')\ (p''+q'')}$$

or the product of the three several chances. For we have already seen that the chance of the two first events happening is the product of *their* several chances. We may regard this as a single chance. Taking, then, the third event, we have the chance of its occurring as well as both the former, equal to the product of the chance of the third event by the chance of both the former happening; that is $\frac{p''}{p''+q''} \times \frac{p\,p'}{(p+q)\ (p'+q')}$.

And so we obtain this general law, that *the chance of several independent events* all *happening is equal to the product of the chances of the several events.*

As examples of the application of this rule take the following:—

Ex. 1.—*The chance that a horse will win a certain race is reckoned at $\frac{1}{2}$, or the betting is even upon him; the chance that an oarsman will win a certain boat-race is reckoned at $\frac{1}{3}$ (the odds 2 to 1 against him); the chance that a county will win a certain cricket match against another county is reckoned at $\frac{1}{4}$ (the odds 3 to 1 against the former). What are the odds against all three events happening?*

The chance that all three events will take place is $\frac{1}{2}\times\frac{1}{3}\times\frac{1}{4}$, or $\frac{1}{24}$. Therefore, the odds against all three happening are 23 to 1.

Example 2.—*I am waiting for the morning post. I reckon the chance that I shall get a letter from a certain correspondent, A, at* $\frac{1}{3}$; *the chance that I shall get a letter from B at* $\frac{1}{4}$; *the chance that I get a letter from C at* $\frac{1}{6}$; *and, finally, the chance that a letter will reach me from some other quarter at* $\frac{1}{10}$. *What are the odds that I get a letter by said post?*

Here we must not multiply the several chances together, because the question is, *not* whether I get a letter from all the sources named, but whether I get a letter at all. Clearly, however, we shall get the chance that I do not get a letter by multiplying together the chance that I do not get one from each of the four several sources. Now, the chance that I get a letter from A is $\frac{1}{3}$, so that the chance that I do not get a letter from him is $\frac{2}{3}$. In like manner the chance that I do not get a letter from B is $\frac{3}{4}$; the chance that I do not get one from C, $\frac{5}{6}$; and the chance that I do not get a letter from any other source, $\frac{9}{10}$. Hence the chance that I get no letter at all is $\frac{2}{3}\times\frac{3}{4}\times\frac{5}{6}\times\frac{9}{10}$, or $\frac{3}{8}$. That is, the odds are 5 to 3 in favour of my getting a letter.

Example 3.—*The chance that there will be rain on any day of the year is* $\frac{1}{2}$. *A prophet announces that there will be rain on one of three successive days. What are the odds in favour of the prophesy?*

The chance of failure on the first day is $\frac{1}{2}$, on the second $\frac{1}{2}$, on the third $\frac{1}{2}$; the one chance of failure on all three days is, therefore, $\frac{1}{2}\times\frac{1}{2}\times\frac{1}{2}$, or $\frac{1}{8}$. The odds are, therefore, 7 to 1 in favour of the prophet.

Our Whist Column.

By "Five of Clubs."

Play Second Hand (Plain Suits).

(Continued.)

We can now do, for play second hand, what we have already done for the lead, viz., reduce it to system by showing, not as heretofore, what card to play from particular hands, but under what conditions such and such cards should be played. This, as in the case of the lead, has a double advantage; it gives simpler rules, and it combines with the rules *for* play the inferences *from* play.

Ace, Second Hand,

is played on King, Queen, or Knave, from Ace and small ones; on Knave from Ace, Queen, and small ones; and from Ace four small ones, on a small card led, if the game is in a critical state or there is reason to believe that the lead is from a singleton.

King, Second Hand,

is played on Queen or Knave, from Ace, King, with or without small ones, and from King not more than two small ones; on Queen from King, ten, &c.; on a small card, from Ace, King, with or without small ones; from Ace, King, Knave; from King one small one, only when second player has special reason for desiring a lead.

Queen, Second Hand,

is played on Knave, from Queen and not more than two small ones, and from Queen, ten, and others; on ten, from Queen and one other; on a small card, from Ace, King, Queen, with or without others; from Ace, Queen, ten; from King, Queen, with or without others; from Ace, Queen, and three others, or more, only if weak in trumps; from Queen one small card, only when a trump lead is specially required.

Knave, Second Hand,

is played from Queen, Knave, and not more than one small one; and from Ace, Queen, Knave.

Ten, Second Hand,

is played from Knave, ten, and not more than one small one; from Ace, Queen, Knave, ten; and from King, Knave, ten.

Nine, Second Hand,

is played from ten, nine, and not more than one small one; from King, Knave, ten, nine.

Lowest, Second Hand,

is played in all other cases, unless to signal, when the lowest but one is played.

Problem 3.—Solutions by Phiz, K. M., R. C. T., J. Harrison, S. Febrook, M. Murchison, Hanky Panky, correct. Phiz, R. C. T., M. Murchison, and others, ask (unnecessarily) if trump lead may not come first. Of course it does not matter in what order the first three tricks are made so that Ace of Spades takes either first or second trick.—Five of Clubs.

Problem 4.—We have received twenty-seven more solutions, all correct. Several suggest that solution should not be published, but we have (implicitly) promised solution. Will defer it. As a help to several who have failed, note that if after thirteenth trump led Y discards a heart, the problem—as Chief Editor pointed out—can *not* be solved if A lead small heart. Hence infer A's proper lead.—Five of Clubs.

A Two Suit Hand.

A correspondent, J. F., writes: "The other evening playing Whist I had the following hand of cards dealt me; six diamonds, seven hearts (clubs being trumps). This occurred in the midst of a long evening's play, the cards being shuffled before each deal in the ordinary manner. Required the probabilities against the occurrence of such a hand."

A set of six cards all of one suit can be formed in

$$\frac{13\cdot12\cdot11\cdot10\cdot9\cdot8}{1\cdot2\cdot3\cdot4\cdot5\cdot6}$$

ways, and a set of seven cards all of one suit can be formed in as many ways, since for each set of six cards of a suit there is left a set of seven cards of that suit. The total number of ways, then, in which a Whist hand can be formed of six cards of one suit and seven cards of another is given by the formula—

$$\left(\frac{13\cdot12\cdot11\cdot10\cdot9\cdot8}{1\cdot2\cdot3\cdot4\cdot5\cdot6}\right)^2\times\frac{4\cdot3}{1\cdot2}\quad(A)$$

if the two suits may be any whatever, in which case there are $\frac{4\cdot3}{1\cdot2}$ ways in which the available suits may be taken 2 and 2 together. But if the two suits are not to be trumps, then for $\frac{4\cdot3}{1\cdot2}$ we must substitute in the above expression $\frac{3\cdot2}{1\cdot2}$, the number of combinations of the three available suits 2 and 2 together. In the former case the number of possible hands being

$$\frac{52\cdot51\cdot50\cdot49\cdot48\cdot47\cdot46\cdot45\cdot44\cdot43\cdot42\cdot41\cdot40}{1\cdot2\cdot3\cdot4\cdot5\cdot6\cdot7\cdot8\cdot9\cdot10\cdot11\cdot12\cdot13}\quad(B)$$

the chance of a two-suit hand, six cards being of one suit and seven of the other, is represented by a fraction having A as numerator and B as denominator. In the latter case there are only 51 cards available for the hand, as the dealer cannot hold it, and the required chance is represented by the fraction

$$\frac{\left(\frac{13\cdot12\cdot11\cdot10\cdot9\cdot8}{1\cdot2\cdot3\cdot4\cdot5\cdot6}\right)\times\frac{3\cdot2}{1\cdot2}}{\frac{51\cdot50\cdot49\cdot48\cdot47\cdot46\cdot45\cdot44\cdot43\cdot42\cdot41\cdot40\cdot39}{1\cdot2\cdot3\cdot4\cdot5\cdot6\cdot7\cdot8\cdot9\cdot10\cdot11\cdot12\cdot13}}$$

This reduces to $\frac{736161}{39688347475}$; or to rather less than $\frac{1}{53912}$: the odds then are rather more than 53911 to one against the occurrence of such a hand as J. F.'s.

The probability of such a two-suit hand, whether trumps or not, is obviously equal to the above multiplied by $\frac{4}{2}\cdot\frac{39}{52}$ or by $\frac{3}{2}$; whence the chance is rather less than $\frac{1}{35911}$, or the odds rather more than 35910 to 1 against the occurrence of the hand.—Ed.

An Unsound Finesse.—Clay was looking on when second player, "whom he favoured not," holding Ace, King, Knave, finessed the Knave. "The Queen made, third hand; Ace and King were afterwards trumped. The player then turned to Clay, and asked whether the finesse of the Knave was justifiable. To him the following crushing rejoinder, spoken very deliberately at the wall opposite, instead of to the querist:—

"'At the game of whist, as played in England (pause), you are not called upon to win a trick (another pause) unless you please!'"

—Cavendish's "Card-table Talk."

Our Chess Column.

A very pretty game played last Friday, April 21, by Mr. Blackburne, at Brighton, being one of eight games played simultaneously blindfolded.

Allgaier Thorold.

White. Mr. Blackburne.	Black. Mr. Bowley.	White. Mr. Blackburne.	Black Mr. Bowley.
1. P to K4	P to K4	10. B to Q3 (c)	Kt to B3
2. P to KB4	P takes P	11. Castles	B takes Kt
3. Kt to KB3	P to KKt4	12. P takes B	K to Kt2
4. P to KR4	P to Kt5	13. P to K5	Kt to KR4
5. Kt to Kt5	P to KR3	14. P to Kt3	R to B sq (d)
6. Kt takes P	K takes Kt	15. B t P (ch) (e)	K takes B
7. P to Q4	P to Q4	16. R takes R	Q takes R
8. B takes P	Kt to KB3 (a)	17. Q to Q2 (ch)	Kt to B5 (f)
9. Kt to B3	B to Kt5 (b)	18. R to KB sq	resigns (g)

NOTES.

(a) Quite sound; Black can also play. 8. P takes P as given in the synopsis, to be followed by K to Kt2, B to K2, and R to B sq. Mr. Zukertort recommends K to Kt3, but we think this unnecessary, as by either of the two defences above Black is fairly protected.

(b) Never a good move in this opening. B to K2 or Kt2 is the right move; the King being exposed requires the protection of the Bishop. If B to Kt2, in that case only Black would follow up by K to Kt3.

(c) Or BK2, Mr. Freeborough's move.

(d) Not apprehending any danger, Black thought this secure. 14. Kt takes B would not have promised well, for then 15. R takes Kt, R to B sq, 16. R takes R, Q takes R, 17. Q to K2, Q to K2, 18. R to KB sq, B to K3, 19. R to B6. This latter move Black could hardly prevent; it gives Black a bad game.

(e) A very fine view of position for a blindfold player to master by mental sight; it wins by force.

(f) 17. K to Kt2 would be still worse, for then Q to Kt5 (ch). If now K to R sq, then Q takes Kt (ch) wins; or if K to B2, then R to B sq (ch) wins.

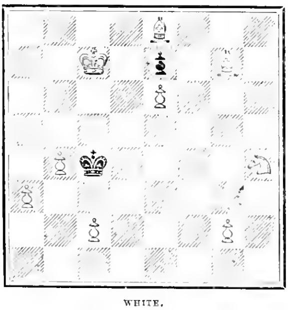
(g) Black has no resource left if 18. Q to K2, 19. R takes Kt, K to Kt2, 20. R to B6, threatening Q to Kt5 or R6 (ch), and wins.

Problem No. 38.

THE SOUTHERN CROSS.*

By J. A. Miles.

Black.



White.

White to play and mate in three moves.

SOLUTIONS.

Problem 25, by Leonard P. Rees, p. 505.

1. B to Kt5	K takes Kt	or	K to K4
2. B to B6 (ch) and Kt mates next move		2. Kt to Q6	K takes QKt. or KKt
		3. B to B4 or B6	(mate)

* Published in the late Westminster Papers.

Problem 33, by C. H. Brockelbank, p. 505.

1. K to B6	K to K5	or	K to K4
2. K to B5, and mates next move		2. R to QB4, and mates next move	

No. 34.

1. Kt to Kt3	1. K to K4
2. K to K7	2. K to Q4
3. Q to K6 (mate)	

ANSWERS TO CORRESPONDENTS.

*** *Please address Chess-Editor.*

J. A. Miles.—Many thanks for letter and problems. Your 3-mover, No. 30, very favourably received. Selections of old masters will always be welcome.

Walter Mead.—Problem received with thanks.

Constant Subscriber.—In no case whatsoever can a player be forced to take his adversary's Pawns or pieces, except if he has touched that piece or made a false move; therefore, the Pawn can play to Knight's 5.

G. W.—Solution 35, 36, and 37 correct. The mate is, as you say, known as Philidor's legacy.

C. W. S.—Solution of No. 35 correct.

J. E. L.—No. 35 correctly solved. We acknowledge all solutions received.

H. S. S.—Problems 30, 31, and 35 correctly solved.

J. M. F.—See p. 505.

H. A. N.—No. 25 correctly solved; also Nos. 33, 34, 35, 36, and 37.

Alfred B. Palmer.—Solutions 35, 36, and 37 correct.

Moleque.—35, 36, and 37 correct.

Fusee.—Solutions of Nos. 31, 33, 34, 35, and 36 correct. No. 29, if R takes B, R to Kt sq, and there is no mate, Q to Q3 is correct.

Moleque.—32 correctly solved. The mistake is ours. P. 543 it ought to be 30 (very) correct and 31 incorrect, if Q to R8, then R to B sq, and there is no mate. Please write on one side of the paper only.

John Watson v. George Wilson.

H. Vallance.—15. Q to B8 (ch), 16. K takes Q, B to Q6 discovering double check, 17. K to K sq, R to B8 (mate).

Brenton.—Solution of No. 31 correct, 32 incorrect. 1. Kt to B4. No. 25, if 1. B to Kt5, 1. K to K4, 2. Kt to Kt3 does not mate if K to K5, but 2. Kt Q6 does. 33 and 34 correct. 35 incorrect. 1. Q to B6, 1. P takes P, and there is no mate. 36 incorrect. The King retreats to Kt sq.

Fleur-de-Lis.—Solution of No. 35 correct.

NOTICES.

The First Volume of Knowledge will be published early in June next, bound in red cloth, gilt lettered. Price 10s. 6d. Vol. I. will comprise the numbers from the commencement (Nov. 4, 1881) to No. 30 (May 26, 1882). As there is only a limited number of copies, the Publishers advise that orders should be sent in without delay, to prevent disappointment.

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THE INFINITIES AROUND US.

By Pasteur.

"WHAT is there beyond this starry vault? More starry skies. Well, and beyond that? The human mind, driven by an invincible force, will never cease asking, What is there beyond? . . . . It is useless to answer 'Beyond are unlimited spaces, times, or magnitudes.' Nobody understands these words. He who proclaims the existence of an Infinite—and nobody can evade it—asserts more of the supernatural in that affirmation than exists in all the miracles of all religions; for the notion of the Infinite has the twofold character of being irresistible and incomprehensible. When this notion seizes on the mind, there is nothing left but to bend the knee. In that anxious moment all the springs of intellectual life threaten to snap, and one feels near being seized by the sublime madness of Pascal. Positivism unceremoniously thrusts aside this positive and primordial notion, with all its bearings on the life of human societies. Everywhere I see the inevitable expression of the Infinite in the world. By it the supernatural is seen in the depths of every heart. The idea of God is a form of the idea of the Infinite. As long as the mystery of the Infinite weighs on the human mind, temples will be raised to the worship of the Infinite, whether the God be called Brahma, Allah, or Jehovah; and on the floor of those temples you will see kneeling men absorbed in the idea of the Infinite. Metaphysics do but translate within us the paramount notion of the Infinite. The faculty which in the presence of beauty leads us to conceive of a superior beauty—is not that, too, the conception of a never-realised ideal? What are science and the passion for comprehending anything else, then, but the effect of the stimulus exercised upon our mind by the mystery of the universe? Where is the real fountain of man's liberty? where the true source of woman's dignity, but in the conception of the Infinite, in presence of which all men are equal?"

THE FUTURE OF KNOWLEDGE.

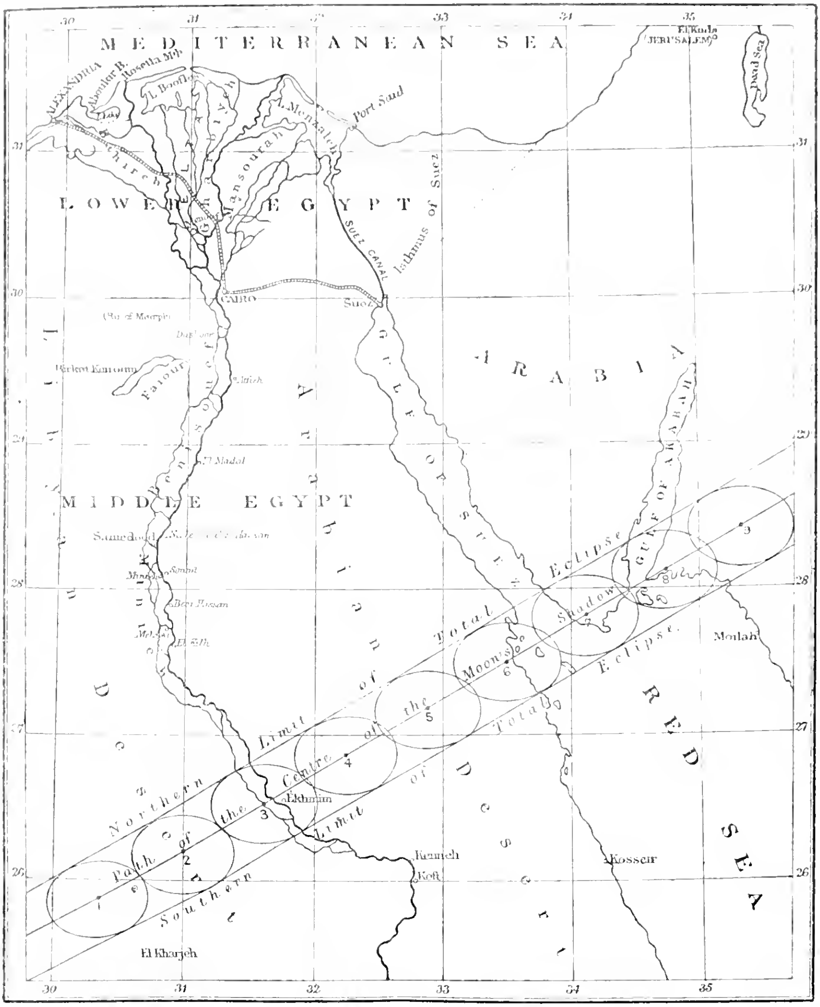
M. PASTEUR has chosen the occasion of his reception in the Academy to speculation largely upon what is in the nature of the case undemonstrable. But the truth is that the career of a great scientific discoverer suggests speculations as little demonstrable, perhaps, but of a different kind from these. Ever since thought began, mankind has wondered as to its own nature and its own destiny. It will go on wondering to the end of time, whatever new facts science may bring to light, whatever new worlds beyond the Milky Way or within the compass of a speck of dust may be revealed by telescope or microscope. It may be allowed, however, in the presence of a personality like that of M. Pasteur, or of the still greater discoverer whose loss the world is mourning, to look forward upon the future of knowledge, to ask how far all these new acquisitions will in the future modify our life, our practice, our methods of study. M. Renan, to whom a curious fortune gave the task of receiving M. Pasteur, has, in an interesting passage of his own autobiography, given it as his belief, that a century hence mankind will study very little else than physical science. The time, he thinks, will come when the historical sciences will be thrust into the background; all that they have to teach will be known, and men will feel comparatively little interest in their own past. On the other hand, the more they know of nature the more there will be to be known. Chemistry and physiology offer inexhaustible fields for research; and the truths which they reveal will prove more and more interesting to mankind. It is very difficult to say what men will think or do a hundred years hence; but it seems likely enough that this will be the tendency of study. Certainly, even now, the men of science are becoming more and more important factors in the life of all of us. They are little by little winning the fight against disease; they are giving us facts, and enabling us to found our beliefs on the sure ground of knowledge. Their influence must surely become greater and greater as time goes on; for humanity always reserves its highest honours for those who teach it to know.—*Times.*

THE APPROACHING ECLIPSE.

NOTE.—Some readers of KNOWLEDGE have been perplexed by my statement that the Editor of KNOWLEDGE had appointed Mr. Proctor Special Correspondent in Egypt, but that it was not certain whether he could go. I had no wish to mystify any of my readers. Perhaps the following statement will make all clear:—

Mr. R. A. Proctor, student of science, has been obliged, after careful consideration, to decline to accede to the earnest wish of Mr. Proctor, Editor of KNOWLEDGE, that he should go to Egypt to view the eclipse. It did not seem desirable that while KNOWLEDGE is still so young it should be left to run alone so long. Certainly, not less than six weeks would have been required for the proposed journey.

THE path of the centre of the moon's shadow across Upper Egypt during the eclipse of May 17 is shown in the accompanying map. The following table is given by Mr. Hind, superintendent of the *Nautical Almanac*, in *Nature* (Mr. Hind speaks of the eclipse of May 16, having probably become so accustomed to astronomical time that, in his mind, the morning hours of May 17, up to noon, belong to May 16; half-past eight in the morning of May 17, in common parlance, is with him half-past twenty,



May 16: but probably most of our readers are more familiar with half-past eight than with half-past twenty).

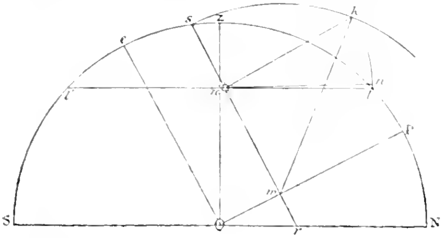
Greenwich mean time, May 16. h. m. s.	Longitude E. deg. m	Latitude N. deg. m.	Duration of totality. m. s.
18 22 30	30 18·2	25 50·9	1 9·2
18 23 15	30 58·0	26 11·6	1 10·6
18 25 0	31 37·0	26 31·9	1 12·0
18 26 15	32 15·3	26 51·8	1 13·3
18 27 30	32 52·9	27 11·2	1 14·6
18 28 15	33 29·9	27 30·2	1 15·9
18 30 0	34 6·1	27 48·9	1 17·1
18 31 15	31 11·9	28 7·2	1 18·3
18 32 30	35 17·2	28 25·2	1 19·5

Thus in longitude 31° 37′ E., latitude 26° 32′ N., a point close upon the Nile, the duration of the total phase is 1m. 12s., and the middle at 20h. 31m. 28s. local mean time. The central line crosses the Nile about a degree north of Luxor, one of the stations occupied for the observation of the last transit of Venus.

The eclipses in the map represent the outline of the moon's shadow (the umbra or total shadow), at the successive epochs mentioned in the above table. Some readers may, perhaps, be interested to learn how the shape and position of the shadow (its size being determined by the calculated duration) have been deduced from a simple process of construction, which also gives other useful information.

Draw the semicircle SZN, to represent the sky at Station 3 (where most of the observers will be) at the moment of central eclipse, N being north point, S the south, the east and west points at O, and Z the zenith, or point vertically overhead, so that OZ is perpendicular

to SN. Take arc NP of 26° 51'·8, giving P the pole, and draw Oe perpendicular to OP, representing the equator. Take es equal to the sun's northerly declination at the time of eclipse, about 19° 20': then smr, perpendicular to OP.



represents the sun's diurnal course for that declination. Open out this course by supposing it turned round radius sm into circle sk. Take sk, corresponding to arc of sun's course, from moment of eclipse to solar noon at Station 3. This, correcting 8 h. 31 m. 28s. by 3 m. 51 s. (to be added to local meantime), so that apparent time is 8 h. 35 m. 19s., corresponds to 3 h. 24 m. 41 s., or, in arc measurement, to 51° 10' 15" (which we take off as angle smk with a protractor. Then rotate sk back again, carrying k along the perpendicular k ⊙ to the sun's true place ⊙ on the sky of Station 3 at the moment of central eclipse. Obviously, then $l'n$ ⊙ l parallel to SON gives lN, the sun's altitude at the time of central eclipse, and the construction makes this 43°, which is within a minute or two of arc of the true altitude.

The elliptic shadow of the moon has, therefore, its major axis exceeding its minor axis in the same degree that OS exceeds On. (If we want this ratio exactly, we turn to a table of natural sines for sine 43°, giving 682 : 1000.)

To determine the direction of the shadow's longer axis, we note that ⊙ is very nearly due east, but a little north of it. An arc n' about n as centre, and n, (more exactly a perpendicular from n' to n, but for so small an angle the arc does as well) taken equal to n ⊙ gives $n'n$, the small angle—about 1½ degrees—by which the sun will be north of east. Thus the larger axis of the elliptic shadow lies nearly east and west, but its eastern end a little north, so that the axis is inclined about 1½ degrees to the latitude parallel through Station 3. The size of the shadow is determined by the consideration that with the determined shape and position the shadow must cover as much of the line of central eclipse as corresponds to the motion of the shadow's centre in 1m. 12s.

Similarly for the other shadows, and of course, lines touching all these ellipses (i) above and (ii) below, give (i) the northern and (ii) the southern limits of total eclipse.

DR. SIEMENS ON SOLAR ENERGY.*

By the Editor.

In this theory there is suggested a fan-like action, by which hydrogen, hydro-carbons, and oxygen are supposed to be drawn in enormous quantities towards the polar surface of the sun. During their approach they are supposed to pass from their condition of extreme attenuation and extreme cold, to that of compression, accompanied with rise of temperature, until on approaching the photosphere they burst into flame, giving rise to great development of heat, and a temperature commensurate with their point of dissociation at the solar density. The result of their combustion is aqueous vapour and carbonic acid or carbonic oxide, according to the sufficiency or insufficiency of oxygen present to complete the combustion, and these products of combustion in yielding to the influence of centrifugal force will flow towards the solar equator. . . . *So much* we may regard as possible, though much would have to be proved before it could be regarded as probable. But Dr. Siemens goes on to say that the matter thus carried towards the solar equator *will be thence projected into space.*

Now there can be nothing simpler than the considerations on which such projection into space would depend. The question whether a body moving in a particular way at any part of the sun's surface will travel outwards into space, or will not travel outwards, can be answered according to certain very definite laws. If the velocity of its motion exceeds a certain amount, the body will recede from the sun; if it falls short of that amount the body will tend to approach the sun's centre; if the body has just that velocity, then the body will neither recede nor approach. Now it suggests the idea of tremendous centrifugal tendency to say that at the sun's equator the velocity is 4·41 times the tangential velocity (at the equator) of our earth. Bodies do not fly from our earth's equator on account of the enormous tangential velocity there (more than a thousand miles per hour); but it is easy to imagine, as Dr. Siemens evidently does, that with the much greater velocity at the sun's equator there may be such a tendency as his theory requires. What is, however, the actual state of the case? Centrifugal tendency varies in the first place as to the square of the velocity: and squaring 4·41, we get 19·45: so that if our earth were to rotate 4·41 times as fast as she actually does, the centrifugal force at the equator would be increased 19·45 times. Even that would not be nearly enough to make bodies fly off at the equator. (In fact, it can easily be shown that for bodies just to become weightless at the equator the earth should rotate in 1⅓ hours, or *sixteen* times as fast as at present.) But this is only a small part of the matter. Centrifugal force not only varies as the square of the velocity, but inversely as the distance from the centre of motion. So that, as the sun's diameter exceeds the earth's about 108 times, centrifugal tendency at his equator is diminished in this degree, so far as this particular circumstance is concerned. Increasing the tendency 19·45 times and reducing it 108 times, means in all reducing it to about two-elevenths of the centrifugal tendency at the earth's equator. Yet even this is not all. Not only is the centrifugal tendency at the sun's equator less than a fifth that at the earth's equator, which diminishes by a very small part the force of terrestrial gravity, but the centrifugal tendency due to the sun's attractive force is very much greater at the sun's surface than terrestrial gravity at the earth's equator. It is roughly about twenty-seven times as great. Thus the centripetal tendency of matter at the sun's equator is very much greater (many hundreds of times greater) than its centrifugal tendency; and there is not the slightest possibility of matter being projected into space from the sun's surface by centrifugal tendency. Nor is there any part of the sun's mass where the centrifugal tendency is greater than at the surface near the equator. So that, whatever else the sun may be doing

* In No. 20, for March 17, there is an admirable *résumé*, by Dr. Carpenter, of Dr. Siemens' theory of the Conservation of Solar Energy. The theory appears to us unsound as respects both its chief requirements. We now give the reasoning which proves, we believe, first, that the solar energy could not be utilised in the way suggested; and, secondly, that, as a matter of observed fact, it is not so utilised.—Ed.

to utilise his mighty energies, he is certainly not throwing off matter constantly from his equatorial regions, as Dr. Siemens' theory requires.

This being so, the theory failing thus in a matter absolutely essential to its validity, we may feel less tempted than perhaps we otherwise might be to endeavour to overlook other difficulties, though these on careful consideration appear scarcely less decisive. It might perhaps appear a work of supererogation to consider difficulties when we have already noted an impossibility. But some perhaps will consider that, although the sun may not, after drawing to himself the matter occupying space, reject it from him in the manner supposed, he may reject it in some other manner. If so, there might still be reason for inquiring how far it is likely that the sun's rays may be utilised when falling on the matter occupying space, in the way suggested by Dr. Siemens. His idea is that solar radiation acting on the aqueous vapour and carbonic acid gas, and other compound gases supposed to occupy interplanetary and interstellar space, may dissociate such compounds, and that solar energy may thus be utilised, instead of being wasted.

Now, if the rays of heat (and light) are thus utilised within the solar domain, regarding that if we please as extending many times further than the orbit of Neptune, they have either done their work and have been completely utilised, or they have not. If they have done their work, these rays proceed no further, and the sun would therefore be invisible from any point outside his own domain. (For we must not fall into the mistake of supposing that light and heat can be considered separately in this inquiry: those solar rays which give us what we call light, give us also a large quantity of the solar heat, and the mystery of seemingly infinite waste would remain, even if we supposed that only those heat rays which are not also light rays were utilised in the way supposed. Apart from this, Dr. Siemens specially shows how the light rays act in accordance with his views.) Now, what is true of our sun is true of other suns, the stars. They also ought to be invisible outside their several domains, but as a matter of fact they are visible. If, on the other hand, the solar rays have *not* done their work in traversing what may be regarded as the solar domain, the mystery of infinite waste is not removed, scarcely even diminished, by Dr. Siemens' theory. If those other suns, the stars, are able to send across the vast distances which separate us from them, such supplies of light (to say nothing of stellar heat, which Huggins and others have measured) that by measuring it we can say that all of them are suns like our own, but many far larger and giving out much more light than he—what is the amount of work which we can suppose the stellar rays to have done on their way? If they have done much (in proportion to the total quantity which they are capable of doing), then the stars must be very much larger, brighter, and hotter than we suppose them to be, and already we regard them as the rivals, and something more than the rivals, of our sun. If they have done little, the mystery of infinite waste remains.

In the case of the Siemens' regenerative furnace, we know that the heat is utilised in the particular manner intended, not only because we find the heat so saved doing its proper work, but because we find that this heat no longer goes idly up the furnace chimney, as before. The heat cannot be doing its full work in the furnace if part goes up the furnace chimney; but also, part cannot be going up the furnace chimney if the heat is doing its full work. This, however, is what Dr. Siemens' theory requires the solar heat to do. It is to be continually utilised in dissociating compound vapours in interplanetary space, although it is continually passing beyond interplanetary space to shine through interstellar space, and to show our sun as a star to worlds circling round his fellow stars the suns. We have, in fact, the fallacy of the perpetual motion in a modified form.

We are compelled, then, regretfully perhaps, but still unhesitatingly, to give up that satisfaction which, as Dr. Siemens remarks, we should gain, could we believe that our solar system need "no longer impress us with the idea of prodigious waste through the dissipation of energy into space, but rather with that of well-ordered, self-sustaining action, capable of perpetuating solar radiation to the remotest future." Yet though not in this way, to this end all thoughtful study of the mechanism of the universe seems unquestionably to tend: not by centrifugal tendencies of the kind imagined, for none such exist; not by work which, viewed in reference to the universe as we know it, means endless production without exhaustion; but in other ways (associating perhaps our visible universe with others, permeating it as the ether of space permeates the densest solids, and in turn with others so permeated by it) there may be that constant interchange, that perpetual harmony, of which Goethe sung—

> See all things with each other blending,
> Each to all its being lending,
> Each on all in turn depending:
> Heavenly ministers descending,
> And again to Heaven uptending,
> Floating, mingling, interweaving,
> Rising, sinking, and receiving—
> Each from each, while each is giving
> On to each, and each relieving
> Each—the pails of gold. The living
> Current through the air is heaving;
> Breathing blessings see them bending,
> Balanced worlds from change defending,
> While everywhere diffused is harmony unending.

From the *Cornhill Magazine*.

Since this article appeared in the *Cornhill Magazine*, Dr. Siemens has called my attention to a letter of his in *Nature*, in which he answers the objection relating to the centrifugal force. Next week I shall endeavour to find space for his reasoning *in extenso*; but here I must content myself by noting that it amounts simply to this: That taking two equal portions of gaseous matter at equal density and temperature, and equi-distant from the sun's centre, one at the sun's pole, the other at the equator (sharing in the sun's motion of rotation), the former is drawn with greater force towards the centre of the sun than the latter,—and that, therefore, a polar inflow and an equatorial outflow must take place, provided only that space is not empty, as supposed by Laplace, but filled with either an elastic or non-elastic fluid. This reasoning shows undoubtedly that under the imagined conditions there would not be equilibrium, and therefore those conditions would not exist. Motion would take place until equilibrium was obtained. But no one familiar with the mathematics of hydrodynamics will, on consideration of the matter, *maintain* (though, by a passing forgetfulness, he might *assert*) that, even if the impossible conditions suggested by Dr. Siemens could exist for a moment, the absence of equilibrium would lead to continuous motion outwards in the sun's equatorial plane. The surfaces of equal pressure would pass from the spherical to the spheroidal form, and would for a time oscillate on either side of the form they would finally assume; but there would be no continuous motion either of inflow or of outflow. I may note, further, that Dr. Siemens' view respecting what Mairan supposed, and Laplace disproved, is not correct. His comparison also between the loss of

solar energy of rotation, due to the fan-like action he attributes to the sun, and that resulting from the tides on the earth, is unsound. However, I must defer to next week any further comments on this subject.

THE ANTIQUITY OF MAN IN WESTERN EUROPE.

By Edward Clodd.

PART II.

THE division of Palæolithic time suggested by M. de Mortillet is as follows:—*

Eolithic......	Thenaisian...	Stone split by fire.
Palæolithic.	Acheulian ...	Age of the Mammoth.
	Mousterian...	„ Cave Bear.
	Solutrian ...	„ Reindeer and Mammoth.
	Magdalenian	„ Reindeer.
		The Cave period.

The evidence in support of the presence of man in Europe in mid-Tertiary times consists of worked flints found in the *calcaire de Beauce*, a Miocene stratum at Thenay, in Loire-et-her, hence the term Thenaisian. The symmetrical form of the flakes; the "bulb of percussion," as it is called, *i.e.*, the conical projection at the end of a flint where the blow striking off a splinter is given; the rough chipping round the edges and the traces of wear and tear, are cited by their discoverer, the Abbé Bourgeois, as proofs of human origin. Moreover, he believes that they were fractured by the aid of fire, or used as "pot-boilers,"† which would be conclusive evidence, if proved. But it is not placed beyond doubt that the flints may not have come from previously-disturbed and later deposits nearer the surface, which is strewn with stone implements, and although similar finds are recorded from the Miocene beds of the Tagus, and bones with apparently designed scratches and notches have been found in the faluns of Pouaneé, the attitude of most anthropologists is to wait for additional evidence. "*Ex pede Herculem*," says the adage, "you judge Hercules by his foot," but even the foot of Tertiary man "comest in such a questionable shape" that we cannot take his measure from it. Indeed, as the foregoing table shows, M. de Mortillet passes without pause from the relics of the Thenay beds to those of St. Acheul, which are within the Post-Pliocene, Pleistocene, or Quaternary period, as it is variously called, and which are now admitted as conclusive regarding man's presence in Western Europe by every anthropologist of repute.

But before describing these in such detail as the importance of the matter demands, let us glance at the momentous changes in Europe which appear to have preceded the arrival of Palæolithic man. These may, in measure, account for the scantiness of material yet producible, and for the gaps in the sequence of geological monuments bearing on the past history of man. "If we consider," Sir Chas. Lyell remarks,‡ "the absence or extreme scarcity of human bones and works of art in all strata, whether marine or fresh water, even in those formed in the immediate proximity of land inhabited by millions of human beings, we shall be prepared for the general dearth of human memorials in glacial formations, whether recent, pleistocene, or of more ancient date. If there were a few wanderers over lands covered with glaciers, or over seas infested with icebergs, and if a few of them left their bones or weapons in moraines or in marine drifts, the chances, after the lapse of thousands of years, of a geologist meeting with one of them must be infinitesimally small."

At the close of the Pliocene Age, the land area was greatly enlarged by slow elevation. The German Ocean, which during that period had covered East Anglia, was "high and dry." As evidenced by the forest-beds traced from Cromer to Kessingland, oaks, firs, yews, birches, and smaller trees abounded; alders flourished in the congenial swampy land; water-lilies blossomed on the rivers, from whose deposits the smacksmen on the Dogger Bank dredge up to-day vast numbers of bones of mammals then wallowing in the slime and roaming through the jungles—huge elephants, rhinoceroses, hippopotamuses, cave bears, wolves, (co temporaries, be it remembered, of man), and even "several large estuarine and marine mammalia, such as the walrus, the narwhal, and the whale."* But, as testified by strata superposed upon the Norfolk forest beds, the temperature gradually declined, until an arctic cold prevailed; the land once more sank beneath the "azure main," and the long, though intermittent, reign of the Ice Age set in. The effects of this in the rounded hills of our island, the *roches moutonnées* of the continent (so called from their resemblance to sheep lying down); in the striated or grooved and polished rock-surfaces; in the erratic blocks—"foundlings," as the Swiss happily name them—deposited in districts far from their parent rocks, as, for example, the occurrence of Scandinavian boulders on the plains of Saxony; in the mounds of sand and gravel, and the deposits of "till" or clay crammed with stones of all sorts and sizes and scantily charged with derived and broken fossils, were long the puzzle and problem of geology, and the source of numberless legends. They were referred to every cause except the true one, until Agassiz, after long study of glacial action in Switzerland, proved them to be due to the mechanical effects of ice. What brought about such alterations of climate as to swathe the northern hemisphere in a vast ice-sheet at one period, and to clothe it within a few degrees of the pole with the vegetation of temperate climes in another period, is explained to the satisfaction of most competent judges by Dr. Croll's theory.† That is to say, at certain periods, irregular in their recurrence, the earth's orbit becomes much more elliptical, and its distance from the sun correspondingly greater. If, when this period of greatest ellipticity happens, the incidence of the seasons has been changed by the precession of the equinoxes,‡ the summer would be too brief to undo the work of the long winter, and ever-increasing accumulations of snow and ice would result. In the course of thousands of years these conditions would be reversed, and the climates of northern and southern hemispheres change places.

Such is, in brief, the explanation of those remarkable conditions which either beset or immediately preceded palæolithic man, and, only staying to remark that, with subsequent upheaval of the land, Britain was once more joined to the Continent, we may pass without further break of story to the sure ground where his "works follow him."

More than fifty years ago, many of the bone-caves of

* "Cf. Matériaux pour l'Histoire de l'Homme." Second Series. Vol. II., p. 545.

† In the absence of earthen or metal pots, we find the practice of dropping red-hot stones into vessels of skin, wood, or bark, widespread among ancient and modern savages.

‡ "Antiq. of Man," 4th ed., p. 246.

* Lyell: "Antiquity of Man," p. 258.

† "On the Physical Cause of Change of Climate during Geological Epochs." *Phil. Mag.* August, 1854, and, for further treatment, "Climate and Time."

‡ See Knowledge, No. 11, p. 218, for an admirably clear explanation of this complex movement by the Editor. Cf. article by Mr. Burr, "*Intellect. Obs.*," vol. iii, pp. 354, *et seq.*

England and Belgium had been explored, but it was not until 1847 that the Torquay Natural History Society embodied the results of their labours in Kent's Hole, in a paper which was sent to the Geological Society. What obtuseness to the momentous revolution in current beliefs as to the antiquity and primitive state of man which these and like discoveries involved, the Council of that learned body displayed, is shown in this laconic entry in their Quarterly Journal, "On Kent's Cavern, near Torquay. In this paper an account was given of some recent researches in that cavern by a committee of the Torquay Natural History Society, during which the bones of various extinct animals were found in several situations."

Nor did they manage these things better in France. In the same year (1847) M. Boucher de Perthes called attention to the discovery of some rudely-shaped flint implements in pits which were being worked for sand and gravel in the Somme valley, near Abbéville. They had been found at intervals during the preceding six years in such positions and so far below the surface as to convince him that they were not later, but probably much earlier, than the deposits in which they were embedded, and in which were also found bones of the mammoth, woolly-haired rhinoceros, and other extinct animals. M. de Perthes argued that these worked flints had been fashioned by man, and witnessed to his high antiquity and low level of culture. But he was met with the reply that these so-called tools and weapons were either natural fractures or forgeries, and an account of similar finds of "instruments en silex" in the Drift at St. Acheul (hence the term Acheulian), near Amiens, which was published by Dr. Rigollot in 1855, met with the same reception.

It seems strange to us, with whom the "Origin of Species" has for some years been a canonical work, that, until within the last quarter of a century, even the masters in our scientific Israel were so fettered by traditional opinions concerning man, that they deprecated any resistance to these, so that the investigation which he had for a long period extended to the earth beneath him, and for a still longer period to phenomena above him, was applied to his kind and its place in the succession of life last of all.

A dozen years passed before *savants* on both sides of the Channel confessed themselves mistaken. In 1858-9, some English geologists, stimulated by discoveries in Brixham cavern, examined M. de Perthe's collection of implements, and the beds in which they were said to have been found. "In addition to being perfectly satisfied with the evidence adduced as to the nature of the discoveries, they had the crowning satisfaction of seeing one of the naked flints still *in situ* in its undisturbed matrix of gravel, at a depth of seventeen feet from the original surface of the ground."* An impetus was thus given to further research, and not only were discoveries of similar implements (presumably, from their general resemblance of form, of the same age, and shaped by the same race of men), made in England in beds of gravel, sand, and clay, for the most part on the slopes of our existing river-valleys, but it was ascertained that flint implements had been disinterred at the end of the last century from the Waveney Valley, in Suffolk, only to be, as it were, reinterred in the Museum of the Society of Antiquaries. The earliest known find of a flint in the drift was in the Thames Valley, probably at the close of the seventeenth century. It is said to have been found with the tooth, or, according to another account, the skeleton, of an elephant, near Gray's-Inn lane, and is preserved in the British Museum. It would be easy to convert this paper into a dry catalogue of discoveries, and to avoid that prosaic result, it suffices to say that implements of stone,—leaf-shaped flakes, removed from flints by blows or pressure, and apparently intended as knives and scrapers; pointed weapons analogous to lance or spear-heads; oval or almond-shaped weapons, with cutting edge all round, have been found by thousands in the drift of England and the Continent. This river-drift is formed of alluvial deposits bought down by that unresting, yet unhasting action of rain and flood which is for ever deepening the bed over which the waters flow. Since the time when the men of the Acheulian period lived in France, the Somme has cut down its valley one hundred feet—a result which requires an enormous antiquity for the flint implements found in the undisturbed gravels. The bottom of that valley has yielded polished stone weapons and other remains further illustrating the vast lapse of time between the Ancient and Newer Stone Ages—vast, even after making full allowance for a more rapid action of rain and flood in the Quaternary period than now.

* Evans's "Ancient Stone Implements," p. 178.

THE AMATEUR ELECTRICIAN.

ELECTRIC GENERATORS (*Continued*).

In the previous article we described the first principles of magneto-electricity, and got so far as to say that when an electro-magnetic coil is made to pass across the poles of a permanent magnet, currents of electricity are induced in the coil. These currents are reversed in direction every time the coil changes its position in relation to the magnet, that is to say, the current induced as the coil approaches the magnet being in one direction, the current induced after the coil has passed, and as it recedes from the magnet, will be in the opposite direction. Commutators or current reversers are used, by means of which these opposite currents are sent in one common direction through the external circuit.

The electro-magnetic coil, however, need not necessarily be of the orthodox form. M. Gramme, about 1870, designed a coil (which, by-the-way, is called the armature of the large or field magnets, consisting of a ring of soft iron, with insulated copper wire wound round it in sections. The object in view was to have a part of the coil always passing through the magnetic field, and so to be constantly producing electric currents, instead of only once or twice in each revolution. For an amateur, however, the Gramme machine is comparatively difficult to make.

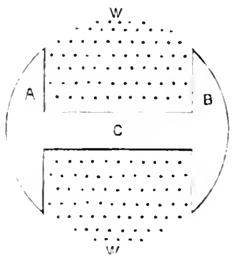


Fig. 1.

We prefer, therefore, to describe in detail a machine with what is know as "Siemen's" armature, being much easier to make, and quite as efficient. This armature

is represented by Figs. 1 and 2. Fig. 1. is a vertical section (full size), while Fig. 2 is a longitudinal section a little less than half-size. The dimensions here given are such as will produce a small instrument of great service for purposes where only a few cells are otherwise required. Larger machines can be described subsequently.

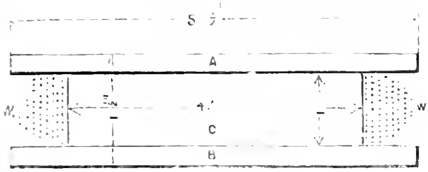


Fig. 2.

The armature is simply an electro magnet with the core flattened out, and its extremities extended so as to form pole-pieces of a segmental section. The core, C, in our machine is four inches long and one inch wide, the thickness being not more than three-eighths of an inch. The pole-pieces, A and B, are five and a-half inches long, quarter of an inch thick in the thickest part, and turned up so as to form parts of a circle of one and a-half inches diameter. The armature may be made in three pieces, and fastened together by means of wrought-iron screws or rivets; or, what is better, it may be cast in one piece, great care being taken to ensure that the iron is perfectly soft, and possesses no coercive force—that is to say, it must not retain any magnetic polarity after a current of electricity has passed round it. It must also be pointed out that when in working order, the armature has to revolve between what are called the magnetic pole-pieces bored out cylindrically, the greatest effect being obtained when the armature revolves in a circular boring only sufficiently large to allow it to move without touching. Consequently, the more true we make the curves of the pole-pieces, the more effective will the apparatus become. For the benefit of those of our readers who have not the facilities for turning out such work, we have deposited a pattern with a very good firm of iron-founders, who will send the casting, either in the rough or finished state.*

Well insulated copper wire should be very carefully wound round the core, so as to fill up the hollow portions of the casting, and form as nearly as possible a circle with AB, as illustrated by WW in Figs 1 and 2. It is recommended to use number 24 BWG, silk-covered (price 3s. 3d. per pound). Both extremities of the wire may be brought to one end of the armature. Before winding on the wire, gun-metal caps should be fitted ready to be fixed on to the armature. Fig 3 represents one of these caps in vertical section. Holes should be drilled in the face of each cap, and corresponding screw-threads tapped into the pole-pieces of the armature, so that perfect rigidity may, on screwing together, be ensured. The caps should also be furnished with projections (about half-an-inch thick and an inch in length), whose function is to act as a spindle. A small pulley wheel, say an inch in diameter, should be driven (or cast) on to one spindle projection, and a little ebonite tube over the other. In this latter cap two small holes should be made near the spindle, and through them the ends of the armature wire should be passed. What becomes of them we will say hereafter.

When working, the armature will revolve at the rate of 1,500 or so revolutions per minute, and it is therefore essential that everything should be true. The wire has naturally a tendency to fly out and will do so, unless two or three small grooves are made in the pole-pieces A, B, in

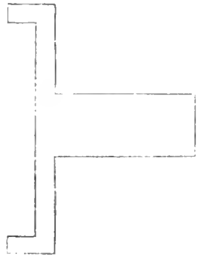


Fig. 3.

which a few turns of silk thread, or any equally tenacious non-magnetic substance, are wound, passing round the armature, and so securing the wire. It is as well, perhaps, to remind our readers that they should make sure that the entire armature is evenly balanced. This they can easily find out. We will leave our description of the other portions of the apparatus till next week.

* Readers wishing for these castings, can get them by sending to Mr. Benjamin Slater, Wellington Foundry, Charles-street, City-road, London, E.C., for KNOWLEDGE casting, No. 1. rough, 6d.; or finished, 3s.

PHOTOGRAPHY FOR AMATEURS.

By A. Brothers, F.R.A.S.

PART V.

THE solution required for developing the negative is composed of protosulphate of iron 1 oz., glacial acetic acid 1 oz., alcohol 1 oz., and water (rain or distilled always to be understood in making up solutions) 20 oz. In hot weather more acid and less iron may be used. The iron should be dissolved before the acid and alcohol are added. Filtration is not necessary if the solution be allowed to settle.

When the plate is removed from the camera slide or carrier, it presents the same appearance as when placed in it (the film of iodide of silver in the collodion is creamy white); no image whatever is visible; it is *latent*, and requires to be "developed." Several agents are used for this purpose, pyrogallic acid and protosulphate of iron being most commonly employed. Development with pyrogallic acid is very slow as compared with the iron, and for that amongst other reasons the iron is mostly used. The light has caused a chemical change, and has so modified the silver compound that when the reducing agent is poured over the plate the silver is thrown down in a metallic state on those parts which have been acted upon by the light, in exact proportion as it has been reflected by the object which has been copied. This will be seen to be the case when the negative is examined by transmitted light; the lightest parts will appear most opaque, and the deepest shades show only bare glass. That the image is formed of metallic silver may be proved by gentle friction on the dry surface of the negative, as, after removal of the powdery surface, the polished silver will be found beneath.

In developing the picture, only sufficient solution should be used to cover the plate, and this, of course,

is determined by the size of the glass used. The developing solution must be caused to flow evenly over the plate by pouring it on at the lower edge, and at the same time by so accommodating the position of the plate that the solution will flow in one sweep to the top, and care should be taken to allow as little as possible to run off. The reason for this is that the silver is required to form the picture, and if washed away the image may be weak. If the proper exposure has been made, the image will soon appear, and its development must be carefully watched, in order to avoid too much intensity. The solution must be kept in gentle motion while on the plate, and may be returned to the glass cup while the plate is examined, and if the development has not been sufficient, it must be again poured on. Experience alone can determine this, and it will quickly be gained. What is required is sufficient intensity to give the proper light and shade *in the print on paper*, which is the object in view in taking the negative. Usually, when iron is used as the developing agent, the image is not sufficiently *dense* for printing from, and, when this is the case, it must be *intensified*. The solution for this must contain 15 grains of pyrogallic acid, 5 grains of citric acid, and water 5 ounces; and it is used by taking sufficient of the solution to cover the plate and a few drops of silver solution (20 grains, for water 1 ounce). This is used in the same way as the developing solution, and the effect on the negative must be watched to avoid over-intensity. After use, the developing and intensifying solutions may be put into any convenient receptacle when the silver they contain may be recovered in a metallic state.

The negative image is still veiled by the unreduced iodide of silver, and this can be removed by pouring over the plate a weak solution of potassium cyanide which will clear the plate, leaving only the metallic image. As potassium cyanide is a dangerous poison, hyposulphite of soda is preferred by some operators; its action is not so energetic as cyanide, but it answers the purpose equally well. A saturated solution should be used—that is, sufficient of the hyposulphite of soda must be put into the water until it will dissolve no more, the surplus crystals may remain in the bottle, and the solution may be repeatedly used.

The negative must be carefully washed after each of the operations we have now described, and if hyposulphite of soda be used for fixing, the washing must be more thorough than when cyanide is used.

If allowed to dry spontaneously, the plate must be turned face to the wall, or it may be at once dried over a spirit lamp, or by a fire; care being taken to avoid dust. In the experimental stage, the negatives need not be varnished before they are printed from. Before varnishing, make the negative so hot that it is just bearable when touched on the back of the hand, then pour on varnish as if it were collodion, returning surplus to stock bottle. Dry by fire, and the negative is complete.

THE Imperial Commission of German Scientists, who will come to America to observe the transit of Venus on Dec. 6 next, will establish posts of observation in Charleston, S.C., and in Hartford, Conn. The Legislature of the latter State has adopted a resolution extending its courtesies to the distinguished visitors, and granting permission to erect on the State Capitol grounds a temporary building for their scientific apparatus.

A NATURALIST says there is an "ant town" in the Alleghany Mountains. It consists of sixteen or seventeen hundred nests, which rise in cones to a height of from two to five feet. The ground below is riddled in every direction with subterranean passages of communication. The inhabitants are all on the most friendly terms, so that if any one nest is injured, it is repaired by help from the other nests. Foreign ants of the same species are not tolerated.

HANLAN AND TRICKETT.

BY AN OLD CLUB CAPTAIN.

I WAS able to study the action of these two scullers under favourable conditions on Monday, hoping to find evidence bearing on the question of rowing styles. But the superiority of Hanlan was manifestly not due entirely, or even chiefly, to any differences such as I touched on in my discussion of the styles in vogue at the two Universities. Yet the characteristic feature of Hanlan's rowing illustrates the principles I then considered. This feature is his marvellously scientific method of sliding; for he so uses the slide as to combine the effect of slide, swing, and arm work, thus bringing arms, back, loins, and legs into simultaneous action, so that his boat seems as if driven from a catapult. Then, so perfectly is each stroke given that there is not the slightest perceptible dip, and therefore no force lost in vertical pressures, and as little loss of way as possible between the strokes. One feature in Hanlan's build surprised me. I had been told that his muscular development is singularly well-proportioned, that there is no abnormal or unusual show of any muscles, but that all the principal muscles of the body are well developed. Also when I met him at Toronto, I had failed to recognise the peculiarity to which I now advert—viz., a most abnormal development of the deltoid, as compared with the biceps muscles. I venture to say that Hanlan is an anatomical marvel in this respect.

THE NAVAL AND SUBMARINE EXHIBITION.

THIS exhibition, which formed one of a course of what may be called "trade exhibitions," at the Agricultural Hall, Islington, and which has been held during this month, was, in the opinion of those old enough and able to judge, by far the best of its kind which has taken place since the Great Exhibition of 1862. Certain it is that the processes and appliances exhibited by some 500 or 600 firms are fraught with extremely great interest, not only to those in the various trades represented, but also to the general public. It is doubtful if anything can be conceived of greater importance than the saving of life at sea. Numerous systems having this object in view were offered to the public, embracing life-buoys, belts, boats, &c., and in addition to the ordinary boats, there were to be seen a number of devices, more or less ingenious, for compactly showing such vessels, yet in such a manner that they could be lowered in an almost inappreciably small space of time—time, in fact, which could be counted by seconds. In one case, the boat, made of canvas-like material, &c., could be doubled up longitudinally, and placed by the side of a ship, occupying only a fifth of the space it would fill when laid open. There were also several arrangements for releasing the boats from the davits, the best, perhaps, being that which allowed the hooks to drop away as soon as the boat touched the water.

Diving apparatus occupied a very prominent place; the most important exhibit in this department being that of Messrs. Fleuss, Duff, & Co. This apparatus is as useful for searching expeditions in mines or other places filled with noxious gases as it is for ordinary diving purposes. No pipes are used in this system, but the diver, carrying with him a small vessel holding a supply of oxygen gas sufficient to last for four hours, breathes his own breath over and over again, the exhaled air being passed through a filter containing caustic soda, which robs it of its poisonous exhalations.

Asbestos is a substance which receives a daily-increasing attention, and was exhibited in a great number of applications. It is expected—and it is to be hoped the expectation will not prove an unfounded one—that asbestos will very shortly be practically demonstrated to be one of the, if not *the*, best of electric insulators. Specimens were shown in which it took the place of ebonite, at something like one-tenth the cost. As was known very many centuries ago, it is a mineral which is fire-proof, and it is not to be wondered at that great efforts are being made to demonstrate its non-inflammability when used as a paint.

Another series of exhibits of very great interest were the materials devised for speedily, very efficiently, and economically removing paint. Altogether, the Exhibition may certainly be described as one of the greatest successes of the day.

CHARLES DARWIN.

THE death of Charles Darwin, which, even at the age of seventy-three, appears premature, will send a thrill of grief through the whole civilised world. No man during the present century has been better known, more quoted and misquoted, appreciated and scoffed at, than the author of that "epoch-making" work, "The Origin of Species;" and yet the man who thus set the world ablaze has been content to lead the life of a true philosopher—allowing friends and foes to say their say, and leaving it to his works themselves to justify praise and to refute calumny. Unambitious and unassuming, he has never thrust himself before the public, nor sought for honours and emoluments. He worked for the love of science and of truth, careless of his own reputation if only he could impart to others that which his own mind had grasped so firmly and analysed so accurately. As a naturalist, not even his greatest enemies will deny him the meed of praise. No other man could have drawn so much knowledge from a single scientific voyage, and the works consequent upon his connection with the expedition of the *Beagle* would have stood out as monuments of vast genius and unparalleled industry, even had he never written those better-known and much-criticised books which have made his name the war-cry of opposing factions.

The great thinkers of the day have long ago made up their minds as to the truth of Darwinism, although Darwin himself would have been the first to admit that the theory he advanced was still imperfect. Having given the bold, broad outline, he has left it to other workers to fill in the details; and even if, in so doing, it should be found necessary to efface a line here and there, he would have been the last to object to such effacement if it should prove desirable in the cause of truth. And meanwhile, the great and childlike philosopher, who thought not the lowly worm and clinging plant beneath his notice, has passed away quietly and unostentatiously as he has lived, few even knowing of his death till a full day after it had taken place. Nevertheless, his loss will be deeply and widely mourned, and the gap left by it in the ranks of science will long remain vacant, for in the present generation who could be found to fill the place so long occupied by Charles Darwin? As a stranger, I can testify to his great courtesy in replying to queries which must often have seemed frivolous; but the truly great man is always tolerant, and willing to give freely to others knowledge acquired with much pains and labour, and in this respect Darwin was truly great, and never despised or rejected anything placed before him which had the slightest scientific value.

It is not necessary here to give a list of his works; they must be well-known, at least by name, to all readers of KNOWLEDGE, and those who know them by name only would do well to make themselves familiar with their contents before they condemn the greatest philosopher and most profound thinker of the age, who, in his quiet country home, has worked out problems in zoology, botany, and geology, which otherwise would have remained, as the riddle of the Sphynx, fatal to all who attempted their solution.

A. W. BUCKLAND.

M. PASTEUR.

FOR thirty years M. Pasteur has carried on the most minute and elaborate researches into the lowest forms of life, and his discoveries, in the opinion of many, have established beyond all reasonable doubt the great fact that there is no such thing as spontaneous generation. He is the foremost representative of the "germ theory" of disease, and has absolutely proved in certain departments, and left it a matter of sure inference in others, that animal maladies may positively be traced to the presence of minute organisms in the body. There has been fierce controversy on these matters. There still are some vigorous opponents who refuse to be converted, such as Dr. Charlton Bastian, who held debate with M. Pasteur at the Congress last year; but there is no question as to which way the balance of opinion now lies, if, indeed, it is not incorrect to speak of the germ theory as being any longer within the sphere of opinion. The great advance that it has made towards certainty during the last few years is primarily due to the work of M. Pasteur. He did not, of course, invent the theory. It is in its outlines as old as the beginnings of scientific medicine; and in a somewhat advanced form it is as old as the last century. But M. Pasteur has given it at once a width and a universality that it lacked before, by his researches into the nature of fermentation and his microscopic studies of disease. It might be thought that beer was too everyday a subject for the investigations of one of the profoundest observers of our time; but M. Pasteur's work on beer has not only made the fortune of the brewers who were wise enough to read him, but has revealed the most important truths as to the mysterious process of fermentation. Wine and silkworms have also attracted his attention; so have chickens and sheep. It was, indeed, with the diseases of these two last that he was concerned in the memorable address last August. Chicken-cholera and splenic fever are mysterious and, it had been thought, incurable diseases. To M. Pasteur they have proved neither mysterious nor incurable, for he has found out the two facts, so important in themselves, so immeasurably important in their bearings on all similar diseases, that these forms of sickness are both caused by the presence of minute alien organisms in the body of the animal, and that they can be cured or prevented by a process analogous to vaccination. Vaccination, indeed, which has heretofore been regarded as a certain but inexplicable safeguard in one disease alone, is now in a fair way of being scientifically explained, and, as a consequence, of being proved useful in innumerable cases hitherto thought to be beyond its reach. Some of our readers will remember the statistics which M. Pasteur gave last year of the effects of the vaccination of sheep according to his method. May we not suppose that a similar cure is about to be discovered for the other plagues, whether of human or of lower forms of life, which are one by one being brought within the scope of the germ theory? The researches of Dr. Koch with regard to tubercular consumption, which Professor Tyndall explained in our columns a few days ago, are a case in point. Who can say whether in a few years, or in the next generation, at all events, it may not be the practice to vaccinate for consumption, as we now vaccinate for small-pox?—*Times.*

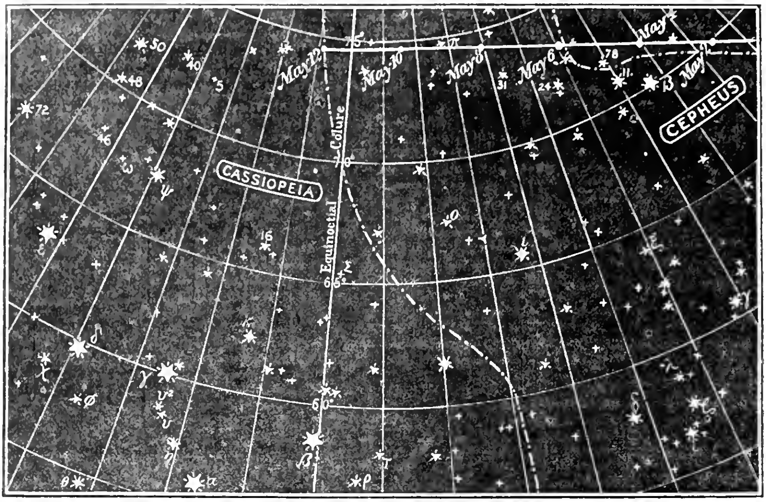
DEVELOPMENT IN FOOT-RACING.

THE winner of the six-day "go-as-you-please" contest, which began in New York, Feb. 27, made the unparalleled record of 600 miles. The second in the race covered 577 miles, beating every previous score save his own of 582 miles made in this city a year ago. The winner, Hazael, was on the track a few minutes short of 106 hours.

The scores made by the winners of the various six-day contests that have taken place since 1878 stand as follows:—

		Miles.
O'Leary...	Astley Belt, London, March, 1878	520¼
Rowell ...	Astley Belt, New York, March, 1879	500
Weston ...	Astley Belt, London, June, 1879	550
Corkey ...	First race, Championship of England, 1878 .	521½
Brown ...	Second race, Championship of England, 1879	542
Brown ...	Third race, Championship of England, 1880..	553
Hart	Rose Belt, New York, September, 1879	540
Murphy ...	O'Leary Belt, New York, October, 1879 ...	505⅛
Hart	O'Leary Belt, New York, April, 1880	565
Rowell ...	Astley Belt, London, November, 1880	566
Panchot ..	O'Leary Belt, New York, March, 1881 ...	541⅓
Hughes ...	O'Leary Belt, New York, January, 1881	568½
Fitzgerald	Ennis Race, New York, December, 1881	582
Hazael ..	Contest at Madison Square Garden, March, 1882	600

In the last race, Rowell, who broke down, ran on the first day 150 miles in 22½ hours, the first 100 miles being covered in 12⅓ hours.—*Scientific American.*



THE COMET.

We give this week the path of comet Wells to the end of next week. We had prepared a map from the orbit given in the circular of the *Science Observer*, Boston; but this was incorrect, and we instructed the engravers not to proceed with the map. The path here shown is taken from the positions given by the superintendent of the *Nautical Almanac*.

THE NEBULA IN ORION.

PROF. HENRY DRAPER'S PHOTOGRAPHS OF THE SPECTRUM.

The nebula in Orion is, for many reasons, a very interesting body. It is an immense mass of glowing gas, and presents the same condition that, according to the nebular hypothesis, the solar system did before any planets had been formed. Among these points of interest none is greater than that bearing on the chemical question of the non-elementary character of the so-called elements. If we examine the spectra of the sixty-eight elementary bodies found on the earth, and group them together, the resulting map contains thousands of lines; if we look at the spectrum of the sun, a considerable proportion of these lines are found there; if we observe a certain series of stars, the number of lines diminishes, till in such stars as Vega only about a dozen lines are seen; and finally, if we turn our spectroscope to the true nebulæ, only three or four lines remain. Such observations indicate that we are tracing back a process of evolution of the elements, and that from extreme simplicity complexity is being evolved. The evolution of the elements proceeds in the same way as the evolution of organised structures, the heterogeneous from the homogeneous.

The investigation of the compound nature of the elements has for some years attracted the attention of advanced scientific men, and various attempts have been made to solve the question. Among these we may now mention those of Professor Henry Draper, who has looked at the problem from the astronomical point of view. As the main hope for the dissociation of our so-called elements is from the influence of heat, it is obvious that in the heavenly bodies, where the temperature is far higher than any we can attain here, we may detect such decomposition. For this reason, for many years, Dr. Draper has been prosecuting this research, and step after step of advance has been attained.

Inasmuch as hand-work is inadequate for the correct and ready mapping of spectra, it was plain that if photography could be applied, great advantages would arise. Accordingly, Dr. Draper has constructed a series of instruments of greater and greater delicacy for the successive steps of this research, and beginning with spectra of the sun many years ago, has photographed the spectrum of star after star, even down to the tenth magnitude, and, finally, during the past month of March he succeeded four times in photographing the spectrum of a nebula—the nebula in Orion. Such photographs require careful study before all that they contain can be explained, but they constitute an important advance. One result, however, is obvious, hydrogen declines to be decomposed, and maintains unimpaired its position as an element. Curiously enough, in the same month, the celebrated English physicist, Huggins, has also obtained a photograph of this same nebular spectrum.

Dr. Draper has also taken photographs of the nebula itself, so as to watch for changes in it, and observe whether the process of aggregation into stars can be detected. Collated with the photographs of the spectrum, they show clearly evidences of such condensations.—*New York Times.*

[Dr. Draper writes to me as follows:—"I do not get the line at λ 3,730, of which Dr. Huggins speaks in the April number of the *American Journal of Science*, but, on the other hand, I photograph *h* (λ 4,101), and a couple of other lines in its vicinity which he does not get. Moreover, I have found two curious condensations (in the nebula) which give a continuous spectrum, and indicate either compressed gas or liquid or solid. These just precede the trapezium, and do not show themselves as stars on the photographs of the nebula (without spectroscope)."—R. A. P.]

LITTRÉ.—Littré's faith in Positivism arose from the mental quietude it offered on the great metaphysical questions. Negation as well as doubt incommoded him. Auguste Comte extricated him from both by a dogmatism which abolished all metaphysic. Following this doctrine, M. Littré said to himself, "Do not trouble yourself either with the origin or end of things, with God, the soul, theology, or metaphysic." What quietude for this ardent mind, ambitious of traversing every field of knowledge. This quietude, however, has been misconstrued, and appearances have been deceptive in representing M. Littré as a staunch and resolute atheist. The religious creeds of others were not indifferent to him. "I know too much," said he, "of the sufferings and difficulties of human life to wish to deprive anybody of the convictions which bear him up in every trial." He no more denies the existence of a God than the immortality of the soul. He excludes the consideration of the notion from the mind, because he is proclaiming the impossibility of scientifically attesting it. For my part, regarding the words progress and invention as synonymous, I ask what is the new philosophical or scientific discovery which can remove such great anxieties from the mind? They seem to me essentially everlasting, for the mystery enveloping the universe, of which they are an emanation, is itself essentially everlasting.—"Pasteur's Address before the Academy of Science."

WEATHER REPORT, FOR WEEK ENDING SATURDAY, APRIL 29.

ABERDEEN.

Day of Week.	Sun.	Mon.	Tues.	Wed.	Thurs.	Fri.	Sat.
Thermometer in Shade. Max. / Min.	52 / 40	55 / 40	54 / 40	53 / 39	47 / 36	49 / 38	48 / 39
Thermometer in Shade. Day's Range	12	15	14	11	11	11	9
Barometer. Inches / 100ths	29 ·30	29 ·13	29 ·19	29 ·37	29 ·67	29 ·25	29 ·23
Wind. Direction	S W	W	N W	N	N W	S S E	N N W
Wind. Force (0 to 12)	1	1	6	8	2	6	5
Weather. Beaufort Scale*	b c	c	b c	c	b	o q p	r
Rainfall. 100ths of inch	22	7	11	1	11	2	37

LIVERPOOL.

Day of Week.	Sun.	Mon.	Tues.	Wed.	Thurs.	Fri.	Sat.
Thermometer in Shade. Max. / Min.	58 / 47	51 / 46	53 / 42	49 / 44	52 / 41	53 / 41	55 / 41
Thermometer in Shade. Day's Range	11	8	11	5	11	12	14
Barometer. Inches / 100ths	29 ·11	29 ·43	29 ·12	29 ·38	29 ·74	29 ·06	29 ·37
Wind. Direction	E	W	W	N N W	S	S W	W S W
Wind. Force (0 to 12)	3	5	3	3	1	2	4
Weather. Beaufort Scale*	m	c	o	b	b	b c	o
Rainfall. 100ths of inch	39	3	1	0	0	9	5

VALENCIA.

Day of Week.	Sun.	Mon.	Tues.	Wed.	Thurs.	Fri.	Sat.
Thermometer in Shade. Max. / Min.	56 / 45	56 / 46	53 / 41	52 / 40	51 / 40	54 / 43	53 / 37
Thermometer in Shade. Day's Range	11	10	12	12	11	11	16
Barometer. Inches / 100ths	29 ·48	29 ·76	29 ·41	29 ·63	29 ·71	29 ·41	29 ·27
Wind. Direction	N W	W N W	E N E	E N E	W S W	N W	N N E
Wind. Force (0 to 12)	6	6	2	1	5	7	3
Weather. Beaufort Scale*	c q	c m	r m	o	b c	c q p	r q
Rainfall. 100ths of inch	74	3	30	3	6	58	35

LONDON.

Day of Week.	Sun.	Mon.	Tues.	Wed.	Thurs.	Fri.	Sat.
Thermometer in Shade. Max. / Min.	59 / 44	59 / 47	59 / 40	54 / 38	53 / 37	53 / 41	57 / 39
Thermometer in Shade. Day's Range	15	12	19	16	16	12	18
Barometer. Inches / 100ths	29 ·26	29 ·50	29 ·53	29 ·31	29 ·76	29 ·23	29 ·53
Wind. Direction	S	S W	S W	N W	W N W	S W	S
Wind. Force (0 to 12)	7	3	2	3	2	2	3
Weather. Beaufort Scale*	o q p	b c	b c	c	b m	o	o
Rainfall. 100ths of inch	31	21	10	91	0	15	10

LYONS.

Day of Week.	Sun.	Mon.	Tues.	Wed.	Thurs.	Fri.	Sat.
Thermometer in Shade. Max. / Min.	71 / 53	62 / 42	59 / 44	61 / 46		52 / 41	
Thermometer in Shade. Day's Range	18	20	15	11	—	11	—
Barometer. Inches / 100ths	29 ·93	29 ·96	30 ·00	29 ·63		29 ·82	—
Wind. Direction	W	S S E	S	S S W	—	W	—
Wind. Force (0 to 12)	1	1	1	1	—	1	—
Weather. Beaufort Scale*	o	b	o	b		o	
Rainfall. 100ths of inch	0	20	0	1		0	

* WEATHER.—*Beaufort Scale* is, b. blue sky; c. detached clouds; d. drizzling rain; f. fog; g. dark, gloomy; h. hail; l. lightning; m. misty (hazy); o. overcast; p. passing showers; q. squally; r. rain; s. snow; t. thunder; u. ugly, threatening; v. visibility, unusual transparency; w. dew.

NARCOTIC INDULGENCES.

OPIUM EATING AND SMOKING, AND MORPHIA-CRAVING.

BY M.D.

OF late years medical men have become familiar with a new form of opium-eating—namely, "Morphia-Craving," designated by the Germans "Morphium-Sucht." Morphia is one of the "Alkaloids," or essential principles of opium; it possesses powerfully narcotic properties, it allays pain, and soothes the nervous system. Owing to its concentrated virtue, it has within the last few years come largely into use by the mode in injection beneath the skin. A small syringe, graduated for the measurements of the dose, is fitted with a fine tube of the size and sharpness of a needle. The injection of a dose, suited to the emergency of the case, is followed by almost instant relief of the most intense pain, and even if sleep be not induced, the most agreeable sensations of relief supervene. So seducing is the sense of ease and rest, under the influence of this mode of administration that, once experienced, the desire for its repetition recurs frequently, until it becomes a hard matter to resist or to overcome the craving for the drug. The solution, inserted into the loose tissues below the skin, is rapidly almost instantaneously absorbed into the current of the blood, and thus carried to the seat of pain.

We know of instances where patients have purchased for their own use the injection syringe, and have practised the use thereof for years, the breaking off of the habit having been found impossible. It is therefore not without some anxiety, and a deep sense of more than ordinary responsibility, that the medical man avails himself of so invaluable a therapeutic means. The effects of the prolonged use of the subcutaneous injection of morphia is to blunt all the nervous energies: to demoralise, in fact, the powers of the mind, and to make the supplementary indulgence in alcoholic stimulants an equally irresistible craving.

Opium-eating is prevalent in several districts in England. In Norfolk and Lincolnshire it is not casual or rare, but popular, habitual, and common, and supposed to be "protection against ague." A writer in the *Medical Times and Gazette* (July 19, 1873) says:—"Any one who visits such a town as Louth or Wisbeach, and strolls about the streets on a Saturday evening, watching the country people as they do their marketing, may soon satisfy himself that the crowds in the chemists' shops come for opium. They have a peculiar way of getting it. They go in, lay down their money, and receive the opium pills in exchange without saying a word. For instance, I was at Wisbeach one evening in August, 1871, went into a chemist's shop, laid a penny on the counter. The chemist said, 'The best?' I nodded. He gave me a pill-box and took up the penny, and so the purchase was completed without my having uttered a syllable."

Opium, there is little doubt, is largely and habitually consumed by persons in England. In the northern counties, more especially in Lancashire, there is a nostrum extensively sold under the name of Godfrey's Cordial, which contains a large quantity of opium. It consists of a decoction of sassafras-wood, with some treacle, and the addition of opium in uncertain quantities. This villanous compound is sold in large quantities, and used by mothers engaged in factory work to keep their infants asleep and quiet whilst they are from home. The mortality among infants in these districts is frightfully large.

It would be a needless occupation of your space to repeat the stories of opium smokers and eaters in China and other parts of the world, but one point should be noticed. The quantity used at each smoking "bout" is very small—exceedingly so to begin with. Taken in the solid form in small doses, as done by some of the runners and carriers of India, it is found to be more sustaining and far less injurious than alcoholic stimulants. That it is a deadly poison cannot, however, be controverted by such facts. Two wrongs don't make one right.

It certainly was a strange oversight that in the last Act of Parliament restricting and regulating the sale of poisons, opium, in a crude or solid state, was omitted from the schedule, while paregoric, a very mild opiate, was included, and cannot be sold without statutory precautions; while opium pills, or powdered opium, may be purchased without restrictions. The writer knows an instance in which a young man bought in a druggist's shop, without any questions asked, enough opium to kill himself. At the inquest, the druggist escaped censure, because the drug was not entered on the schedule of the Act for the Regulation of the Sale of Poisons.

In illustration of the danger of the reckless use of opium, I would add the following incident:—

I was one evening hastily summoned by a person who told me that my son was in a fit in an adjoining street. I hurried off in the direction indicated, in a state of the most painful anxiety, as he had never previously had a fit. On my way, I met two policemen with

my son on a stretcher, and I accompanied them to the nearest station. My son was totally unconscious, and his countenance had the paleness of death. His limbs were flaccid; his breathing slow, regular, and performed without effort; his pulse was soft and slow.

The report to me by the constables was, that they had found him in this state, seated on the pavement, propped up against a wall. The impromptu and inevitable verdict of most of the police was "Drunk." There was, however, no smell of alcoholic liquor in his breath; neither was his physical state like that of a man "in drink." One of the constables, more intelligent and cautious than his fellows, pointed out these features, and said "He is not drunk, but he's drugged." I hesitated to accept this explanation, since he, being a medical student, I thought, would not easily be "hocussed" in that manner. Nevertheless, the sequel proved the man was right in his diagnosis.

As I was well-known in my professional character at the station, I was allowed to remove my son home. He was placed in bed and kept quiet. In the course of about eight or nine hours he woke up, and was at first quite at a loss as to his "whereabouts." After an attack of vomiting, he recovered his faculties, and gave the following account of his illness.

He and a fellow-student, discussing the subject of opium-smoking, in a thoughtless moment agreed to experimentalise upon themselves. They were both tobacco-smokers. They placed each a small pinch of powdered opium in their pipes with the tobacco and smoked away for a short time, when they began to feel agreeably intoxicated. They went to an adjacent tavern and asked for coffee; whilst this was being prepared they fell asleep on sofas. After some time they were ejected from the inn as being the worse for liquor. They wandered out in the street; my son remembered getting into an omnibus, but forgot everything afterwards until he came to himself at three o'clock in the morning. Upon close inquiry, I learnt that the conductor of the omnibus when he got to his journey's end turned his passenger out, and saw no more of him. His fellow-smoker could give no account whatever of his doings until he found himself in bed, in his lodgings, the next morning. In these two young men the effect of the inspiration of opium fumes was a dreamless sleep, with some of the phenomena of somnambulism. A slightly stronger dose would, doubtless, have sent them into that sleep from which there is no waking.

The narrative here given may, perhaps, warn others from such experimenting, and it should also serve as a caution against opium smoking to any extent at all.

SEAL "FISHERIES."

A LARGE part of the world's supply of seal-skins for furs comes from the Prybilov Islands, off the coast of Alaska. On two of these islands the seals congregate in vast numbers early in spring, and remain for three months. With the exception, I understand, of one island off the coast of Japan, the fur seal is nowhere else seen in the North Pacific, and it is said that the seals on the coast of Alaska are colonies which migrated suddenly from the other island. Be this as it may, the habits of the seal prove that it has a remarkable attachment to localities. The sealing is farmed by the United States Government for, I think, £50,000 a year to the Alaska Commercial Company; and an agent of the United States resides on the principal island and superintends the annual slaughter of a stipulated and selected number of victims. By this precaution there is little danger, from man's inconsiderate rapacity, of the fur seal being exterminated in the North Pacific. Some years ago I heard Mr. Bryan, the Government agent, describe to the California Academy of Sciences, the fur seal trade carried on under his supervision. With only a variation of a few days in point of time, the males arrive in a great shoal that agitates the sea as far as the eye can scan, and, hobbling across the beach, establish themselves at "home" on the ledges of rock, the strongest males always taking possession of the best places. Next day, or, at the outside, on the second day after the arrival of the males, the females are discerned on the horizon, and ere they have emerged from the waves, the males have rushed down to the beach to welcome them, which they do with much roaring and quarrelling among themselves. Fighting for possession of the females is legitimate warfare, and in such a contest the greatest number of females fall to the most powerful males, some of which secure five or six companions. A day after the female lands, she gives birth to her young (one or two), and hardly have the offspring begun to suckle when she receives the embraces of the male. During their stay on the islands, combats of great ferocity occur among the males, and the older animals exhibit scars and gashes which render their pelts of little value to the furrier. A short time before the seals go to sea again, the males and females separate, and are to be seen extended in long lines apart from each other along the beach. The young stay with the females, and go to sea with them. It is a most curious fact that, during the whole of the three months on the islands, neither male nor female enters the water. They abstain entirely from food throughout the whole of the period, and it is almost unnecessary to say that, although they arrive in plump condition, they return to their fishing very lean. The animals are killed, and not skinned alive. Males alone are killed, and these only at three years of age. After that age, their combats materially reduce the value of their skins. As soon as the males have separated from the females, the fur-providers' operations begin. Day after day, groups of the males are surrounded and cut off from their companions, and a grand drive of the game takes place towards the curing establishments. On the way thither, the males which are unsuitable are allowed to slip seaward between the ranks of the drivers. On reaching the slaughtering-ground, a blow or two from a club on the head kills the animal. The skins are removed with the greatest care, in one particular way, and are given to the dressers, who scrape and rub down the inner parts, leaving them soft, and remove also the outer rough hair, which completely covers the inner fur. Some precautions of curing having been taken, the skins are periodically shipped by the company's steamer to San Francisco. There they remain some time in the company's warehouse in Sansome-street, but are all ultimately sent to London to be dyed; and, as I have been informed by the officials of the company, the dyeing operations are a secret in the possession of one family in London. It thus happens that seal-skins in the United States, on re-admission to the country, have to pay duty as a manufactured article, and are much more costly than in England. Occasionally, dyers in San Francisco experiment to get at the secret of the dyeing in London, but no one has succeeded. Some dyeing, I believe, is performed in Germany, but it is possibly by a branch of the London establishment. D. J. McR.

A DOG GOES OVER NIAGARA FALLS ALIVE.

A LARGE dog lately survived the passage over Niagara Falls and through the rapids to the whirlpool. He was first noticed while he was within the influence of the upper rapids. As he was whirled rapidly down over the falls, no one imagined but that that was the last of him. Shortly afterwards, however, he was discovered in the gorge below the falls vainly endeavouring to clamber up upon some of the *débris* from the remains of the great ice bridge which recently covered the water at this point, but which had nearly all gone down the river. The news spread rapidly through the village, and a large crowd gathered on the shore. Strenuous efforts were made to get the struggling animal on shore, for an animal which had gone safely over the falls would be a prize worth having, but without success. Finally the dog succeeded in getting upon a large cake of ice, and floated off upon it down towards Suspension Bridge and the terrible whirlpool rapids. Information of the dog's coming was telephoned to Suspension Bridge village, and a large crowd collected on the bridge to watch for the coming wonder. In due time the poor fellow appeared upon his ice-cake, howling dismally the while, as if he appreciated the terrors of his situation. An express train crossing the bridge at the time stopped in order to let the passengers witness the unusual spectacle. Round and round whirled the cake, in a dizzy way, and louder and more prolonged grew the howls of the poor dog. As the influence of the whirlpool rapids began to be felt, the cake increased in speed, whirled suddenly into the air, broke into two, and the dog disappeared from view. No one thought that he could possibly survive the wild rush through the rapids. When, therefore, word was received that the dog was in the whirlpool, still living, and once more struggling vainly to swim to land, it was received with marked incredulity. This story was substantiated by several trustworthy witnesses. It seems incredible that an animal could go through the upper rapids, over the falls, through the gorge, through the whirlpool rapids, and into the whirlpool itself, a distance of several miles, and still be alive. The poor animal perished in the whirlpool.—*Scientific American.*

"In two instances dogs have been sent over the Falls and survived the plunge. In November, 1836, a troublesome female bull terrier was put in a coffee sack by a couple of men who had determined to get rid of her, and thrown off from the middle of Great Island Bridge. In the following spring she was found alive and well about sixty rods below the Ferry, having lived through the winter on a deceased cow that was thrown over the bank the previous fall. In 1858, another dog, a male of the same breed, was thrown into the rapids, also near the middle of the bridge. In less than an hour he came up the Ferry stairs, very wet and not at all gay. He was ever after a sadder if not a better dog."

"NIAGARA."—BY GEORGE W. HOLLEY.

TO CORRESPONDENTS

Letters to the Editor.

[The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with their writers. All communications should be as short as possible, consistently with full and clear statements of the writer's meaning.]

All Editorial communications should be addressed to the Editor of KNOWLEDGE; *all Business communications to the Publishers, at the Office, 74, Great Queen-street, W.C.*

All Remittances, Cheques, and Post-Office Orders should be made payable to Messrs. Wyman & Sons.

**** All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.*

All Letters or Queries to the Editor which require attention in the current issue of KNOWLEDGE, *should reach the Publishing Office not later than the Saturday preceding the day of publication.*

(I.) Letters to have a chance of appearing must be concise; they must be drawn up in the form adopted for letters here, so that they may go untouched to the printers; private communications, therefore, as well as queries, or replies, to queries (intended to appear as such) should be written on separate leaves.

(II.) Letters which (either because too long, or unsuitable, or dealing with matters which others have discussed, or for any other reason) cannot find place here, will either be briefly referred to in answers to correspondents, or acknowledged in a column reserved for the purpose.

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

"God's Orthodoxy is Truth."—*Charles Kingsley.*

Our Correspondence Columns.

POPULATION OF THE EARTH: A CURIOUS PROBLEM.

[391]—A correspondent, James Connal, asks how much space would be required on the earth, on the assumption that, beginning with a single pair, the human race had multiplied during 5,000 years at the rate of 30 children to each pair, sons and daughters being born alternately, and each husband and wife being at the time of marriage respectively 21 and 20 years old—there being also no deaths. The problem is too difficult "for any use," as Americans say; but it may be roughly dealt with thus:—Children being born from the twenty-first to the fifty-first year of husband's life, and from the twentieth to the fiftieth of the wife's, we shall obtain a fair mean estimate, rather under than over, if we take 30 children for each pair at the age of 40 (nineteen or twenty being really born before and the rest after 40). On this assumption the population of the world in 5,000 years (or 125 times 40) would be

$$2 + 30 + 450 + \&c. \text{ to } 126 \text{ terms}$$

$$= 2\left[\frac{15^{126}-1}{15-1}\right] = \frac{1}{7}(15^{126}) \text{ approximately.}$$

Now log. $15 = 1{\cdot}1760913$, and multiplying by 126

$$\begin{aligned} \log.\ (15^{126}) &= 148{\cdot}1875038 \\ \log.\ 7 &= 0{\cdot}8450980 \end{aligned}$$

log. (number representing population) $= 147{\cdot}3424058$ $=$ log. 2199915, followed by 141 digits, which digits we may represent without appreciable inexactness by cyphers. Now, assuming that ten persons could stand on a surface one yard square, 30 millions could stand on a square mile, and on the entire earth, whose surface is about 200,000,000 square miles, some 6,000 millions of millions. Dividing the number just obtained by this, we get 3666526, followed by 125 cyphers. It would require this inconceivable number of worlds like our own for the population on the assumptions made.

Let us see how large a single globe would suffice to give standing room to this population, ten to the square yard.

Such a globe must have a diameter exceeding the earth's as the cube root of the above quantity exceeds unity, or, roughly, as 7 followed by 43 cyphers exceeds unity. (In reality it will be larger considerably, but this is near enough in such a problem as the present.) Now the diameter of the orbit of Neptune, roughly, exceeds the earth's diameter nearly 700,000 times. Hence the diameter of the required globe exceeds the diameter of Neptune's orbit more than a hundred billions of billions of billions of times.

Supposing the farthest star visible in the great Rosse telescope to lie some thirteen or fourteen millions of times farther from us than the nearest, which lies about 70,000 times farther than Neptune, the distance of that star, 1,000,000,000,000 times farther than Neptune (or a light-journey of some 10 millions of years), would be but the

100,000,000,000,000,000,000,000,000th

part of the radius of such a globe as would be required to hold the population we are considering. A sphere having a radius equal to 100 millions of years' light-journey would not suffice even to *contain* so many human beings.

COD SOUNDS AND SCIENTIFIC PRIVILEGE.

[392]—Though I am glad to see Mr. Mattieu Williams' exposition of his views upon the subject of Cod Sounds, as it enables one to understand the way in which his (as I take it) misconception of the nature of that structure has been brought about, yet I am very sorry that any clumsiness of expression upon my part should have led him to think that I am an upholder of that paltry spirit of scientific exclusiveness which he with such vigour and justice condemns. Versatility in my eyes ranks next only to accuracy in a scientific man, and I agree entirely with Professor de Morgan that the ideal scientist should "know something of everything, and everything of something," indeed my own studies in biology have been undertaken mainly with a view to enlarging my grasp of another science from which I have borrowed the *nom de plume* under which I write.

This notwithstanding, I trust Mr. Williams will pardon my pointing out that, for instance, Pasteur's splendid acheivements "in the domain of the biologist" were not published as the recollections of work done in his student days twenty or thirty years previously. Since the days when Mr. Williams was a student of natural history, &c., at Edinburgh, biology has made vast strides, partly by the mere increase of the number of observers, but mainly by the aid of absolutely new methods of research with refinements in appliances undreamed of forty years ago. The grand generalisations of Darwin have, besides, directed inquiry along lines entirely new. Did Mr. Williams acquaint himself with the writings of modern Comparative Anatomists, and then perform one careful dissection of a "Cod-fish," I feel sure he would come to a very different conclusion as to the nature of the "Sound." As, however, it would be unreasonable to ask him to recommence the study of so large a subject after such a lapse of time, I will just give an account of the appearances presented in my recent dissections of the fish under discussion.

I would point out that, as a preliminary, the blood-vessels were injected with a brightly coloured fluid which made them so distinct as to leave no room for doubt as to their nature.

Just under the throat the heart is situated, and from it proceeds forwards a large vessel, the *ventral* aorta; this subdivides into a number of branches which distribute blood to the gills. After traversing the minute capillary vessels of those organs, the blood is carried into a corresponding set of large vessels, which, after giving off one or two unimportant branches, unite to form a medium trunk, the *dorsal* aorta, which almost immediately sends branches to the stomach, intestine, and various glands. From the point whence these arteries are given off, the aorta bends up and runs in direct contact with the underside of the backbone down to the tail. Lying beside the aorta are two large veins.

Beneath these blood-vessels is a large bag (the sound) with fibrous saccular walls, the dorsal (upper) wall of which is very thin and delicate, and the ventral (lower) and lateral walls exceedingly thick and strong. This bag extends through almost the entire body-cavity, and is quite shut. It terminates in front in two blind prolongations which lie loosely among the nerves and blood-vessels just behind the head. Within it (besides air, which I have always found) is a body known as rete mirabile, a closely-packed arterial network, which receives its blood from a very small branch of the aorta. Between the sound and the backbone are the kidneys, elongated bodies of a deep-red colour, which in the dead fish, at any rate, contain a great deal of blood. They are divisible into three regions—a thick *head kidney, not in contact with the sound*, extending from the hinder part of the skull backward to the *mid-kidney*, a much thinner portion, occupying the middle region of the body-cavity; and a very thick portion, the coalesced right and left *hind kidneys*, passing back beyond the hinder end of the body-cavity. The kidneys are supplied with blood by a number of small branches of the aorta, and their ducts (the *ureters*) pass right through the sound, perforating both its upper and under walls, and carrying side by side with them *a small artery*.

When the coloured fluid was injected into one of the large arteries, it passed freely into the aorta (the dorsal one I style simply *the aorta*) and all its branches, even to the most delicate; yet *not one drop found its way into the cavity of the sound.*

The foregoing facts I can personally testify to.

Professor Hæckel classifies the true bony fishes (*Teleostei*) as *Physostomi*, or those in which the air-bladder communicates throughout life with the alimentary canal by an open duct; and the *Physoclisti*, in which the duct, though open in the young immature fish, is closed in the adult. To the latter group (which is further characterised, according to Professor Huxley, by the presence of a rete mirabile in the air-bladder) the cod belongs.

To sum up the case, I contend:—

1. That there is *air* within the sound, which, unless produced by *post mortem* changes, would by all analogy certainly cause the death of an animal if in its aorta.

2. There is also within it a rete mirabile which *might* be mistaken for a blood clot, but I have never found any blood.

3. There are, between the sound and the backbone, the kidneys, which, in Mr. Williams' student-days, *may* have been regarded by anatomists as mere clots of blood, and it is obviously to them that he refers as the "great dorsal blood clots," agreeing as they do exactly in distribution with the said "clots." It is quite contrary to my experience to find blood forming clots in *arteries*, though they are almost invariably found in the *veins* of dead animals, and it is more than doubtful if such definitely distributed ones could form in a large sac like the cod-sound.

4. The head kidney lies in front of, and therefore must be outside, the sound.

5. There is no communication between the circulatory system and the sound, as is proven by injection.

6. The sound has walls made up entirely of fibrous tissue, and therefore is not comparable to human arteries, which have muscular walls, as have those of all other animals. It could not supply any force for the propulsion of the blood, being merely *elastic*, and not possessing *contractility*.

7. There being no *systemic* heart in these bony fishes, the *dorsal* aorta cannot proceed from the *bulbus arteriosus*, as suggested by Mr. Williams.

8. The "sound" of the cod originates as an "off-shoot from the upper part of the digestive canal," just as the air-bladders of other fishes do, and therefore is the precise homologue of those structures; and, whatever its true function may be, it certainly plays no part in the circulation of the blood. Its strength is no obstacle in the way of comparison with undoubted air-bladders, as the carp (a much smaller fish) has an air-bladder almost as thick, with an open duct; and in the extinct genus, *Cœlacanthus* (a Ganoid), we actually find a *bony* air-bladder. OLD FOSSIL.

BREAK FOR TWO-WHEELED VEHICLE.

[393]—I have a small pony, about 9 hands high, that runs in a small two-wheel ladies' basket carriage. He is a very useful and sagacious pony, but very frightened of going down hill, for fear he should fall with the vehicle on him. I am advised by our country cousins about here to whip him, and make him run down-hill. I think this advice both cruel and unwise. I think my pony knows as well as I do that he might fall. In order to mend matters, can any of your numerous correspondents say if they have had any experience with a break applied to a two-wheeled vehicle? and should be obliged by a description.

I am quite aware that breaks to four-wheeled carriages are common, but with two wheels the difficulty is, that wherever the break is placed on the periphery of the wheel, the effect is to press down the shafts on the pony's back. By applying the break to the nave of the wheel, some objection to this is removed, but I am told that it strains the wheels. And, also, should the break be on both wheels—that is double? W. H. C.

A PASSAGE IN "IN MEMORIAM."

[394]—I consider it to be utterly impossible for the allusion to be to Longfellow. Goethe is no doubt the poet referred to. Longfellow does *not* sing "in divers tones," whereas Goethe does. It appears to me that what was in Tennyson's mind at the time of writing was the manner in which Faust, in the second part of the drama, finally works out his own salvation. The allusion is probably one in spirit, rather than in letter, though it is possible that in the "conversations with Eckermann" *some* passages may be found to throw light upon it. Tennyson himself has, I believe, stated that the reference is to Goethe.

Next as to dates of publication. "In Memoriam" appeared in May, 1850 (though written years before), and the introduction is dated 1849. Longfellow's poem is avowedly based upon a passage in St. Augustine's "De Ascensione" (*De vitiis nostris scalam nobis facimus, si vitia ipsa calcamus*), shows here and there slight and unusual traces of Tennysonian mannerisms, and forms part of the First Flight of "Birds of Passage." So far as I can remember, this series was published about 1858, probably as an appendix to the "Miles Standish" volume. This much may be gleaned from internal evidence. The poems appear to be chronologically arranged. Those on the death of the Duke of Wellington and on Florence Nightingale, for obvious reasons, could not have been written earlier than 1852 and 1855 respectively. I cannot give the date of "The Two Angels," but it may easily be fixed by the event it refers to—the birth of one of Longfellow's children and the death of Mrs. Lowell on the same day. "Victor Galbraith" *may* have been written as early as 1846, and "The Jewish Cemetery" probably sprang from the poet's visit to England. I think that "Daybreak" appeared about 1856 (or somewhat earlier) in one of the magazines edited by Charles Dickens, but I have no present means of ascertaining. The series closes with "The Fiftieth Birthday of Agassiz," which is dated May 20, 1857, just seven years after the publication of "In Memoriam." I believe, therefore, that the resemblance between the passages in question is purely accidental, but that if it be otherwise, Longfellow, not Tennyson, must be held to be the copyist. I may add that the so-called parallel passages from Petrarch and Milton need hardly be taken into consideration.

GEORGE E. DARTNELL.

Answers to Correspondents.

*** *All communications for the Editor requiring early attention should reach the Office on or before the Saturday preceding the current issue of* KNOWLEDGE, *the increasing circulation of which compels us to go to press early in the week.*

HINTS TO CORRESPONDENTS.—1. *No questions asking for scientific information can be answered through the post.* 2. *Letters sent to the Editor for correspondents cannot be forwarded; nor can the names or addresses of correspondents be given in answer to private inquiries.* 3. *Correspondents should write on one side only of the paper, and put drawings on a separate leaf.* 4. *Each letter should have a title, and in replying to a letter, reference should be made to its number, the page on which it appears, and its title.*

R. OUSELEY. If your communication were not quite so long! It is so difficult to abridge satisfactorily. No; the gentleman to whom you refer is not my son, nor have I a son old enough for the rank of colonel, unless thirteen years be considered a suitable age in India.—C. J. BROWN. The answers to correspondents will not be abolished, but reduced in quantity. I am glad to think that *some*, at any rate, among readers have not misunderstood what was meant—in almost every case—for good humour. Several have taken for downright severity what I have meant for good-natured fun. I do so thoroughly enjoy banter (I mean when I am bantered myself), that I cannot quite understand how others can be hurt by it. To give an instance,—one correspondent, who really has some very wild ideas about stone implements—which he regards as natural formations—describes my jocular objections to his views as "worthy only of a palæolithic savage." You are right in thinking R. A. Proctor and the Editor of KNOWLEDGE one and the same individual. I never supposed it would be thought otherwise, when I mentioned that the Editor of KNOWLEDGE (R.A.P.) wished R. A. P., student of science, to go to Egypt to observe the eclipse—which task, by the way, the latter has been obliged, greatly to his regret, to decline.—CURIOSITY. Since noticed. Letters had been too multitudinous to be got through.—G. H. P. Egyptian centres, area, period, and duration of totality given this week.—J. S. Thanks for pointing out that the doggrel rhymes about the borrowing days do not occur in the "Complaint of Scotland," a prose work. Does it not seem as though the observed coldness of those three days, which in the sixteenth and seventeenth centuries were the first three days of April, affords a sufficient reason for the conceit that March had borrowed these days from April? in other words, that though April days in name they are March days in quality.—DECIMAL. The rule for placing the decimal point in division of decimals, simple and recurring, is given, I think, in most books on arithmetic. It is not very concise, but is simple enough. I have forgotten it. But in practice one does not need any rule of the kind. I simply set off the decimal point of division as many digits to right or left as may suffice to leave only one digit on the left of it; and then set the decimal point of dividend as many digits in the same direction, putting in zeros if any are wanted. It then becomes obvious where the decimal point of quotient must fall. Thus consider the following cases:—

First, 127·412)15·315(; I write this, or conceive it written, thus, 1·27412)·15315(. Here manifestly the quotient will begin ·1.

Secondly, 167·531)15·315(; I write this 1·67531)·15315(, and manifestly the quotient here will begin ·09.

Thirdly, 167·531)·015315(; I write this 1·67531)·00015315(, and manifestly the quotient will begin ·00009.

Fourthly, 127·412)1531·5(; I write this 1·27412)153150(, and manifestly the quotient will begin 1, followed by five digits before the decimal point.

Fifthly, ·0167531)1531·5(; I write this 1·67531)153150(, and manifestly the quotient will begin with 9 followed by four digits before the decimal point. The plan is not affected when either divisor or dividend or both contain recurring decimals. This is not, I must admit, an answer to your question for an exact (and you suggest) concise rule. But it is far better to have a common-sense plan, the *rationale* of which is obvious, than a mere verbal rule, the terms of which may be forgotten when the rule is wanted.—A FELLOW OF THE GASTRONOMICAL SOCIETY. I believe that for persons with your symptoms, Hanwell, Bethlehem, and Earlswood, are equally open; but as "great wit to madness nearly is allied," so that entire absence of wit precludes the idea of madness, Earlswood seems more suitable than the others.—A SUBSCRIBER. Certainly no new force is obtained by using coals to produce electricity, only a different form of force.—J. B. There is no correspondence between the Scandinavian deities and those of the Greek mythology. If Thor as the Thunder God corresponds with Zeus, Odin as Thor's father would correspond with Saturn; but Odin is in many respects akin to Jupiter. Sater is regarded as the Anglo-Saxon deity for Saturday; but probably only from verbal kinship.—JAS. SMITH.—There is no such rigid arch. Tresca's experiments show conclusively that at a depth of less than twenty miles, the pressures are so great that the hardest solids would behave as fluids. He found steel perfectly plastic, and, as it were, viscous, under a much smaller pressure.—G. W. L. Soon I hope to deal with the reappearance of Biela's comet in another form.—IGNORAMUS. A misprint for earth, as you say.—J. TINSLEY. I do not know the exact size of Lincoln's Inn Fields. It is about 750 or 760 ft. from north to south, but less from east to west; the Great Pyramid's base is 760 ft. square. The area of the base of the Great Pyramid is about $13\frac{1}{4}$ acres, the area of Lincoln's Inn Fields about 12 acres.—ALGERNON BRAY. The fraction represents the exact chance; but the question belongs to the more difficult departments of probabilities.—A. McD. Even in the temperate zone there might occur a glacial irruption. It is evident the mammoth was suited to bear cold. Adhémar's theory has no scientific standing.—VOLO SCIRE. The co-efficient of expansion for iron will not give the law of increase in length of iron wire. Your question how to make an artificial bead resembling black pearl, I cannot answer. Possibly Lieut.-Col. Ross may be able to.—JOHN REID. There is no proof that as rock substances cool they continually diminish in bulk. Some substances would unquestionably behave like ice, floating on the molten matter. The point has been considered by Sterry, Hunt, Mallet, Dana, and others (myself, for instance). But of most rock substances (as distinguished from metals) it seems to be true that the crystalline products resulting from their slow cooling are of greater specific gravity than the fused rocks.

ELECTRICAL.

W. H. and others. There are several good text-books on electricity, but for the untechnical student, Deschanel's "Electricity and Magnetism" is perhaps the best. The Leclanché is the best form of battery for physiological purposes.—H. W. B. 1. The arc in the 150,000 candle-power Brush lamp is about 1·25 in. 2. Quadruplex has not been applied to either of the Transatlantic cables.—P. I. Had one-tenth, or even less, of the current generated in Edison's dynamo passed through the body of the gentleman who placed his hands on the terminals of the machine, the result would doubtless have been fatal. It should be remembered that the current generated by the machine is of comparatively low tension, being what is called a large-quantity current (see our eighth article on Electric Exhibition), and the external circuit is proportionately low, therefore, when the hands are placed on the terminals, a branch circuit of 2,000 or 3,000 ohms resistance is offered to the current. The joint resistance of 1,000 lamps, at 100 ohms each (omitting the resistance of the connecting wires) is by calculation 0·1 ohm. A little reflection will show that from $\frac{1}{20000}$th to $\frac{1}{30000}$th of the current will be all that can go through the body of the experimentalist. The labourer at Hatfield was killed because a current of high tension was used to overcome the high resistance of the lamps, &c., the body in this case offering a comparatively lower resistance, and forming an easy means of escape for the current. This would have been avoided had there been two leading wires, instead of using the earth in place of one of them. Furthermore, it should be made imperative that all conductors carrying large currents should be well insulated. 2. We are already pledged to an article on "Electrical Measurement."—B. J. P. The best thing you can do is to set to and make a little coil, or a medical magneto machine. You will not have much chance of getting a shock otherwise, unless you are prepared to expend a considerable amount of money on batteries. You might get a little shock by placing the bell in circuit, and so getting a series of pulsations. 2. You appear to be a little in error, or your query is more than I can fathom. Depositing steel by electricity is out of the question.—H. W. B. I should say that such an accumulator might give good results at first, but, from a chemical point of view, would soon run down through local action. I will experiment and publish the result.—F. W. The universal galvanometer has not so far answered expectations, and you cannot do better than get a *good* tangent galvanometer (wound for quantity and intensity with an optional shunt), and a set of Wheatstone bridge coils.—E. KNOWLES. Get an ordinary kite, place a small metal rod on it, and in connection with the iron a wet hemp or cotton thread. At the lower extremity of the thread tie a dry silk thread. Fasten a key or any suitable piece of metal on the lower part of the cotton. Send your kite up, holding it by means of the silk, and sparks can then be obtained from the lower piece of metal. Before, however, you try an experiment on your body, it would be advisable to make arrangements with a respectable undertaker.

THE TELESCOPE.

R. F. S. As your primary object is probably to examine lunar and planetary detail, and to obtain a knowledge of the more remarkable celestial objects, in the shape of double stars and nebulæ, you will certainly obtain more for your money in the shape of a reflecting than of a refracting telescope. A really first-class 3-inch achromatic refracting telescope, mounted on a firm stand, and furnished with two astronomical eye-pieces, is scarcely procurable under £15 or £16; whereas the same sum will purchase a $5\frac{1}{4}$-inch silvered glass reflector, mounted on a plain equatorial stand (without divided circles), the respective grasp of light of these instruments being nearly in proportion of 9 to 25. It is beyond our province to recommend individual makers, but our Advertising Columns may be consulted with advantage.—JULIUS must not accept every statement in Guillemin's "Heavens" precisely as Gospel. What, however, the writer probably meant to convey (*loc. cit.*) was that, in our climate, 6 inches is as large an aperture as can be profitably employed for ordinary work on average nights. We are occasionally favoured with definition which renders much larger apertures not only available but desirable. Moreover, nothing is easier than to stop down the object-glass of greater diameter on a bad night. But having said this, we must add that the number of *first-class* 6-inch objectives is greatly in excess of that of object-glasses of larger apertures of similar excellence, the difficulty of actually achromatising very large ones being seemingly insuperable. We have ourselves never looked through an objective exceeding 9 inches in diameter which was absolutely and in all respects satisfactory, and we have seen more than one big one which was very indifferent indeed.—H. D. will fail to divide ξ Ursæ Majoris in a 3-inch telescope, unless the object-glass be really a high-class one. But the components of that star are now some 2″ apart, instead of 1″, as our querist appears to imagine; and we must further add that the absolute limit of the dividing power of the finest 3-inch objective in the world is 1·52″. Secondly, an inferior object-glass will not show a trace of Jupiter's satellites on the disc of the giant planet; nor will the best one do so, save just after their ingress on to, or just prior to their egress from, his limb. Thirdly, Argelander's 11·6 magnitude is the *minimum visibile* of the aperture of which we are speaking. Argelander's scale had a scientific basis. Smyth's scale was—well, it was Smyth's scale.—Rev. E. H. can *not* make "a really useful astronomical refractor, object-glass from 3 to 4 inches" for any sum approaching £3 or £4, for the simple but sufficient reason that a high-class 3-inch object-glass alone in its cell costs £6, and a 4-inch £15. Our correspondent should write to the dealers in second-hand instruments who advertise in KNOWLEDGE for their catalogues. Excellent telescopes, by known makers, may often be obtained in this way at reasonable rates.—"Antarctic" may measure the power of any eye-piece on any telescope whatever, reflecting or refracting, by the aid of a little instrument known as the Dynamometer, one very simple form of which is the invention of the Rev. E. L. Berthon, of Romsey, in Hampshire. All he has to do is to focus the telescope on a star, and then, in the daylight, turn it up to the sky. Withdrawing the eye now some ten inches or so from the eye-piece, a little circle or disc of light will be seen. This is an image of the object-glass or mirror diminished in the exact proportion of the magnifying power. All we have to do, then, is to measure the diameter of this spot of light, and to divide the effective aperture of the mirror, or objective, by the result; the quotient will give the magnifying power of that particular eyepiece. Thus, suppose that the aperture of "Antarctic's" mirror is $6\frac{1}{2}$ inches, and the little spot of light is found to measure 0·0252 inch, then, dividing 6·5 by ·0232, we get 280 as the magnifying power. All methods of calculation based on obtaining the foci of the component lenses of an eye-piece are very operose, and uncertain to boot.

Our Mathematical Column.

FAIR BUT UNWISE BETTING.

By the Editor.

BEFORE considering other orders of chance problems, it will be well to consider the relation between the mathematical chance of an event and the moral value of expectations depending upon it. For convenience, let us do this with special reference to wagers upon events more or less probable, such as races, matches, and so forth.

If the chance of an event is $\frac{r}{n}$, the chance of its failing to happen is $\frac{n-r}{n}$. Comparing these two chances, we get the ratio r to $n-r$, in which r represents the number of favourable cases, and $n-r$ the number of unfavourable cases. The technical expression used to indicate this relation is that the odds are r to $n-r$ on the event (that is, in favour of it), if r is greater than $n-r$; or $n-r$ to r against the event, if r is less than $n-r$.

Suppose now that in an urn there are ten balls, of which three are white and seven black; then the chance of drawing a white ball is $\frac{3}{10}$, and the chance of failing to draw a white ball is $\frac{7}{10}$; while the odds against drawing a white ball are 7 to 3. And if two persons, A and B, were to wager on the event, A to win if a white ball were drawn, and B to win if a black ball were drawn, then, that the wager should be strictly fair, the sums respectively wagered by A and B should be in the proportion of 3 to 7. It will be clear that this proportion is fair, if we remember the real fact as respects wagers, that when once a wager has been laid, even though the betters keep the wagered sums in their pockets till the issue is decided, the case is precisely the same as though those sums were added together to form the *prize* for the winner. In the present case, supposing A to wager £3 against £7 of B's, the prize for the winner is £10; and as A's chance is $\frac{3}{10}$, the price he should pay for it is three-tenths of £10—that is, £3—while B's price for his chance should be seven-tenths of the prize, or £7.

But there is another way of viewing the matter. Suppose A and B to go on betting upon the same event, A always backing the white and B the black, the drawn ball being returned after each wager had been decided; then, in the long run, the number of times that A and B would be respectively successful would be in the proportion of 3 to 7, as nearly as possible—the more nearly the longer the backing continued; and it is clear that, to equalise their chances, the money gained by A and B respectively, when successful, must be in the proportion of 7 to 3.

Here, then, we have the mathematical principle on which all wagers should be based, if they are to be fair,—viz., that the sums respectively staked by the bettors must be proportioned to their respective chances of success.

But although bets made on this principle are strictly fair as between the parties to the wager, yet it is a mistake to conclude that a man's chances of loss or of gain are equal, when he stakes his money on fair wagers.

For, in the first place, his property is not increased in the same proportion if he wins an even wager, as it is diminished if he loses. Thus, suppose his property to be £1,000, and that he wagers £500 against £500, the chances of success and failure being equal. If he loses, his property is halved; but it is not doubled if he wins; and in like manner it may be shown that, whatever he stakes, the effect of success is not equivalent to the effect of failure.

It might seem, however, that if a person always wagered a sum bearing a very small proportion to the property he has at first, he would be safe from serious loss in the long run. Supposing, for example, that a person, A, has £1,000, and repeatedly wagers £1 against £1 on equal terms, it might seem as though he would never be much richer or much poorer than at starting. Now, even if this were so, it would be an argument against betting, since it would show the uselessness of fair wagering. But, as a matter of fact, a belief in the "long run" is one of the most fatal delusions which a bettor can entertain. It may be shown—and, indeed, will be found to follow from the principles to be enunciated in these papers—that the chance of absolute ruin, in such a case as we have imagined, increases with the number of wagers. The *ratio* of money lost to money won in such a series of wagers approaches, indeed, more and more nearly to equality the greater the number of wagers; but the extent of the *difference* between the two sums is likely to be greater the longer the process of wagering is continued. Thus, in a hundred wagers there would be nothing very wonderful if A lost or won as many as fifty-five wagers, in which case he would have lost or won £10; whereas in a million wagers it would be utterly improbable that he would lose or win so many as 550,000 wagers; the numbers of won and lost wagers would probably be much closer; but it would be unlikely that they would be so close as 500,500 and 499,500; yet *if they were no closer, and the balance were against* A, his £1,000 would be lost, and his wagering put an end to. It is calculable that the odds are greatly in favour of the numbers not being so close as 500,500, and 499,500, and it is obvious that the balance is as likely to be against A as in his favour. So that what he in effect would risk by entering on so long a series of wagers would be this, that *in all probability* his whole property would be as if risked on a single contingency, in which the chance of success or failure was but one-half. No one would think of risking his whole fortune on the toss of a halfpenny; nor would any one care to agree that his whole fortune should be thus risked, *if* in drawing a ball out of a bag of twenty, of which but one was white, he failed to draw the white ball. Yet a person who makes a series of small wagers, trusting to the "long run," is no whit better circumstanced (if he only continues wagering long enough) than one who has agreed to so daring a venture as the latter; while the longer his wagering is to last, the more nearly does his case approach that of the former. For the complete investigation of the subject of wagering, I would refer the reader to the chapter on the "Risks of Loss and Gain" in De Morgan's admirable, though somewhat dry, treatise on probabilities; but the following general principles may be enunciated, as containing the essence of the whole matter:—Better small wagers and many than large wagers and few; better few small wagers than many small wagers; better yet, no wagers at all.

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[30]—T. F. asks for the solution of equations—

$$x^2+y=11 \quad \text{(i)}$$
$$y^2+x=7 \quad \text{(ii)}$$

The equations, of course, reduce to a biquadratic in x or y, having one obvious root, and so reducing to a cubic, the solution of which does not belong to elementary algebra. The four roots are all real, as is indeed obvious if we consider that (i) and (ii) are equations to two parabolas having their axes at right angles, and intersecting in four points. In the only sense in which the equations can be regarded as suitable for our "young readers" as we wrote, their solution is very easy, because $x=3$ and $y=2$ are obvious solutions, so that $x-3$ will be a factor of the biquadratic in x, or $y-2$ a factor of the biquadratic in y, according to the line followed in obtaining an equation with one unknown. Or we may write (i) and (ii) thus—

$$x^2+y=3^2+2 \text{ and } y^2+x=2^2+3$$

whence obviously $x=3$ and $y=2$.

The Muscular Force of a Crocodile's Jaw.—A strange kind of experiment has been lately made in Paris by Drs. Regnard and Blanchard, viz., the measurement of the power exerted by the masseter muscle in a crocodile (a muscle passing from the cheek-bone to the lower jaw). Ten live crocodiles of the species *C. galeatus* or *siamensis*, that had been sent in large cases from Saigon to M. Paul Bert, afforded the opportunity for such experiments. Some of these animals were as much as 10 ft. in length, and weighed about 154 lb. The difficulty of managing such creatures in the laboratory was, of course, considerable. The crocodile was fixed with ropes on a heavy table; the lower jaw kept in contact with the table by a cord, while the upper was raised by means of a cord attached at the extremity, and passing up to a beam overhead. A dynamometer was inserted in this cord, and was affected when the animal was stimulated with an electric current. In this way a crocodile of about 120 lb. weight gave an indication of about 308 lb. (140 kilogrammes). The application of the cord at the end of the snout was necessary, but unfavourable, seeing the application of the force is thus at the end of a long lever, and there is at least five times more space between this point and the insertion of the masseter muscle than between the latter and the joint of the jaw, the fulcrum. Hence the massoter really produces a force five times that indicated by the dynamometer, or about 1,540 lb. (700 kilogrammes). This extraordinary force, it should be remembered, was that of an animal somewhat weakened and at a low temperature. The force (of about 308 lb.) is really applied at the end of four large teeth that project beyond all the others, and considering the surface here represented, the authors estimate the pressure, while the bite is executed by the extremity of those teeth, at nearly 400 atmospheres. Making similar experiments with an ordinary sporting dog, they obtained in the dynamometer a pressure of about 72 lb.; while the effect at the insertion of the masseter was about 360 lb. The pressure at the point of the canine teeth would be about 100 atmospheres. It is calculated that the crocodile is about one-third stronger than a dog of the same weight would be.—*Times.*

Our Whist Column.

By "Five of Clubs."

Play Second Hand (Trumps).

The play second hand in trumps differs in several respects from the play in plain suits. This partly depends on the circumstance that the lead in trumps is somewhat different, as we have seen, from the lead in the other suits; partly on the absence of risk from ruffing; and partly on the card turned up, and its position with respect to second player. Then, also, the critical nature of trump play has to be considered. When trumps are led, second player knows that there is at least sufficient strength, either in the leader's hand, or between the leader and his partner (if the latter has signalled), to justify the expectation that between them they may get out all the trumps held between second and fourth players, and bring in a long suit. A defensive, or at any rate a waiting game, has therefore generally to be played.

The principal differences in detail, between trump and plain suit play, second hand, are these:—

From Ace, King, and one or more small cards in trumps, it is generally better to play a small card second hand, to give partner a chance of making first trick. A small card is played for a similar reason in trumps, from King, Queen, and more than one small one; if, however, you have the ten also, play it. From Ace, King, Queen, and a small one, Queen is played in Trumps, as well as in plain suits, because second player is so strong that he should play a forward game.

From Ace, Queen, ten, in trumps, ten is played, instead of Queen as in plain suits. This gives partner a chance of making the trick; and should he fail and third hand make it, on the return of the trump lead two tricks are certain.

From Ace, Knave, ten, one or more small ones, the smallest is played in plain suits, because the first player cannot hold both King and Queen. But in trumps (see Leads) he may hold both these cards, and it is therefore better to play the ten.

From a honour and one small card, only play the honour if it is important to stop the trump lead. If your partner has turned up King or Ace, and you hold Queen and a small one, you of course play the small one.

Second Hand Second Round.

In the second round of a suit it is nearly always best, if you hold the winning card, to play it, unless, of course, you know third hand to be very weak in the suit. When your adversaries' trumps are exhausted it is sometimes better to pass the second round of a long suit, if the third round is assured, and there is a fair chance of the suit being established third round. In trumps second round, it is often better to keep back the winning card if you have numerical strength in trumps and a good plain suit.

If in second round second hand holds second and third best cards, he should, of course, play the third best. If you hold third best, and have reason to believe your partner holds the best (and leader second best) you may often with advantage play the third best, and so save your partner's best. If, however, you are long in the suit, you very likely lose by this, for your partner's best card is probably single, and so falls on a trick already won.

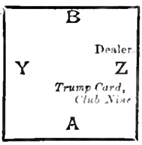
Illustrative Game.

Ace is usually played second hand on King; but occasionally, with Ace, Knave, and others, if you are strong in trumps you may pass the King, on the chance of making the tenace. It is, however, very seldom good play to do this.

The following game illustrates the importance of clearing your partner's suit when it is established, and you hold the best card and but one small one. With two small ones, *B*, second round, would not have been justified in taking his partner's trick.

THE HANDS.

A.
Clubs—K, 3.
Hearts—A, Q, Kn, 9, 8, 6.
Spades—Q, 7, 3.
Diamonds—5, 2.



B.
Clubs—A, 10, 8, 5, 4.
Hearts—K, 5, 2.
Spades—2.
Diamonds—10, 9, 7, 4.

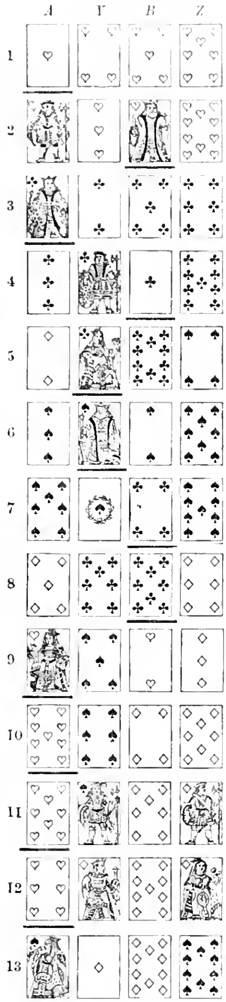
Y.
Clubs—Q, Kn, 7, 2.
Hearts—4, 3.
Spades—A, K, Kn, 6, 5.
Diamonds—A, K.

Z.
Clubs—9, 6.
Hearts—10, 7.
Spades—10, 9, 8, 4.
Diamonds—Q, Kn, 8, 6, 3.

Score.—*A B*, 0; *Y Z*, 0.

THE PLAY.

Note.—The underlined card wins trick, and card below it leads next round.



REMARKS AND INFERENCES.

1. Both *Y* and *B* begin to signal, both with good reason, but *Y* with the better, for he not only has four trumps two honours, but an excellent hand outside trumps.

2. *A* has led from Ace, Queen, Knave, and two at least. *Y*'s signal is completed; but *B* sees his opportunity, takes the trick, (leaving his partner with the suit established), and leads the penultimate trump. A bad partner would have saved his King (knowing the Queen with *A*), and lost the chance of making a great game.

4. *B* having five trumps, and knowing *Y* cannot have less than four trumps two honours (*Y* being one who never signals without good cause), knows that *A*'s Club 3 cannot be the lowest of three left in hand; but it is the lowest Club; therefore *A* can have no more. For if he had had two he would have played the higher. The inference happens to be of no subsequent use to *A*, but it is well to note it, as inexperienced players often lose by failing to notice just such points as these. *Y* makes a similar inference, being sure that *B* would not have led trumps, after opponent's signal, from less than five trumps, one honour. *Y* should have played the seven, on the chance that ten may lie with *Z*. As the cards lie, it would have made no difference in the result.

5. Having second and third best trumps left, *B* leads the second best and draws *Y*'s Queen. *Y* does well to take the trick, having nothing to gain from getting out more trumps.

6. *Y*, of course, resorts to his long suit; but unfortunately,

7. *B* has but one card of the suit, and trumping the second round (*Z* discards penultimate diamond),

8. Draws out the last of *Y*'s trumps,

9. Brings in his partner's long suit, and

10, 11, 12, 13, *A B* make five by tricks.

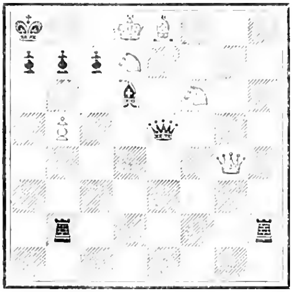
W. F.—Would it not be apt to perplex if Queen with two small cards, or Knave with three small cards, were said to be singly guarded? Of course, Queen with two guards is not much better guarded than King with one. (She is a little better guarded, because of the chance that if in first round the trick falls to a small card, second round may draw Ace and King together.) But that is met by keeping the expression "Queen guarded," for Queen two small cards, "Knave guarded" for Knave, three cards, and so on. A single card is a guard, though an imperfect one, for a Queen.

H. W.—No; certainly *A* cannot take back the card first played, unless it would have been a revoke. Not only so, but the card which he threw down to replace the first can be called. If a player having renounced finds, before the trick is turned and quitted, that he has a card of the suit, he can take up the card played in error, and follow suit; but the card so played may be called.

Our Chess Column.

Problem No. 39.
By the late William Bone.

Black.



White.

White to play and mate in four moves.

SOLUTIONS.

Problem 35, by Leonard P. Rees, p. 525.

1. Q to QR8, and mate next move.

Problem 36, p. 525.

1. R. to B8 (ch)	1. K to Kt 2
If K takes R, Q to K8 (mate)	
2. Q to Kt6 (ch)	2. B takes Q
3. P takes B (ch)	3. K takes P.
4. B to Q3 (mate)	

Problem 37.

1. Q takes P (ch); K to R sq. 2. Kt to B7 (ch); K to Kt sq. 3. Kt takes RP (ch); K to R sq. 4. Q to Kt8 (ch); R takes Q. 5. Kt to B7 (mate).

REVIEWS.

Chess and Mathematics. (From "Waifs and Strays.") By Captain Hugh A. Kennedy.

Tom Moore, the poet, mentions in his diary, as a very strong objection of Bishop Warburton's to mathematical pursuits, "that in making a man conversant with studies in which certainty is the result, they unfit him (or at least do not prepare him) for sifting, and balancing (what alone he will have to do in the world) probabilities; there being no worse practical men than those who require more evidence than is necessary."

Now that circumstance, which so grave an authority as Warburton pronounces to be an inherent deficit in mathematics, as regards practical mental training, is precisely reversed in Chess, and constitutes, therefore, the principal value of the game as a mental exercise and preparative for the contentions of actual life. In the study of mathematics, there is always an inevitable result, to be reached by a fixed and consecutive train of reasoning which admits of no deviation. In Chess, on the other hand, although the result, *i.e.*, the winning or drawing of the game, is inevitable, yet the ways of accomplishing it are almost infinite; and the mode of reasoning—save in the openings and many of the endings, which can be acquired from the books—consists in a perpetual sifting of probabilities. It is true that Chess in its nature admits of the same determinate certainty as mathematics, but as it is also true that absolute perfection of play is possible only by the exercise of a degree of prescience and a depth of skill, both in combination and calculation, of which the most happily-organised brain must ever fall immeasurably short, this perfection, as far as concerns us, cannot be said to have any existence.

Chess Trees. By Thomas Long, B.A., T.C.D. Tree 1—Philidor's Defence.

The whole of this opening is on one foolscap folio, in what the author calls "map and pedigree" form. In our opinion, the form adopted by Cook in his "Synopsis" is much plainer and simpler, and we can see nothing in Mr. Long's plan but unnecessary elaboration. On examining the sheet we found it to contain exactly the same matter and variations as is given in the "Synopsis," with hardly anything novel. There are a few transpositions of moves or slight additions, which do not, however, materially affect the opening. There are twenty-two variations, and we will trace them to Cook's giving the number of the variations in that book.

"Chess Trees" variation ...	AB	C	D	EF	H	I	K
Cook's "Synopsis" variation	17.	7.	8.	5.	6.	4.	3.
"Chess Trees" variation ...	L.	M.	N.	O.	PQ.		R.
Cook's "Synopsis" variation	2.	1.	12.	11.	10.		9.
"Chess Trees" variation ...	S		TU.		W.		X.
Cook's "Synopsis" variation	18.		16.		13.		15.

Seeing, then, that the form in which this opening is given is less clear than Cook's, and the contents the same, we turn to the question of price; the book, when finished, will cost about seven or eight times the price of the "Synopsis." In the latter book there are thirty-four different openings, with over 600 variations, for which a charge of 3s. 6d. is made. Mr. Long asks the chess-public to give 1s. for one opening containing twenty-two variations.

ANSWERS TO CORRESPONDENTS.

**** Please address Chess-Editor.*

Muzio solution of Nos. 35 and 36 correct: in the Evans' play 8. P to Q4, your best move, or, better still, 6. P to Q3.

H. S. S.—Solutions of 33 and 34 correct.

M. Oldwyd.—No. 35 correctly solved.

Steady Reader.—Your former solution no doubt correct, but we did not receive it.

H. A. N.—Problem received with thanks.

Alfred B. Palmer.—No. 38 correctly solved, and in good time.

S. M.—You are right in a measure; Black would not lose the piece if he did not proceed with B to B4, but if he did not intend to push the attack, why institute it at all? after 15. B takes B. 16. Q takes B; 16. Kt to R3. 17. P to KB4. White has clearly a better game.

Contents of Knowledge *No.* 26.

NOTICES.

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KNOWLEDGE
AN ILLUSTRATED
MAGAZINE OF SCIENCE
PLAINLY WORDED—EXACTLY DESCRIBED

LONDON: FRIDAY, MAY 12, 1882.

CONTENTS OF NO. 28.

SCIENCE AT THE ROYAL ACADEMY.

IN art exhibitions, such as those at Burlington House, in the Grosvenor Gallery, and the like, there are always many paintings so hideous in subject or in treatment, that it may be questioned whether the pain they cause to the artistic eye does not go far to counterbalance the pleasure which the better works are calculated to produce. This is the case even in great national collections, where only the finest works are supposed to be gathered together. There is not a room in the Louvre where there are not paintings and sculptures absolutely painful to contemplate, and annoying even when we try, so far as possible, not to see them. But in annual exhibitions of the works of a great number of living artists, the hideous paintings and sculptures sometimes preponderate to such an extent that a visit to such collections is an actual punishment. Last year, for instance, there were so many bad paintings, and some of them so very bad, so many ill-chosen subjects, and some of them so very faithfully (and therefore disgustingly*) rendered, that the many beautiful works of art which were exhibited were quite insufficient to counteract the painful impression produced by the others, and one found oneself wondering whether the worst of the rejected pictures could have been very much worse than some which were exhibited.

This year the general impression produced by the pictures and sculptures at the Royal Academy is far pleasanter. There are several very bad works, and a few which are positively execrable; but they do not destroy the pleasurable effect produced by a general survey, nor are they so obtrusive as to disturb the artistic mind by their mere presence, during the careful study of the better works.

In these columns, however, we do not intend to consider chiefly the artistic beauty of the various works exhibited at the Royal Academy. Our purpose rather is to note where artists have either failed from want of scientific knowledge, or have availed themselves of such knowledge (or of the close observation of nature which is the basis of scientific knowledge), to produce effects, the truth and beauty of which are at once recognised, even by those who do not understand the secret of the artist's success.

It may be urged at the outset, and is, indeed, often urged by artists themselves, that they represent what they see, and that it is not for others to question the scientific accuracy of this or that portion of a painting or sculpture, when they have not before them the landscape or model from which the artist worked. The truth, however, rather is that the artist endeavours more or less successfully, according to his skill, to represent what he sees. Even where what he is attempting to represent is unchanging, he often fails in his attempt to represent it correctly. But in many cases, the artist is obliged by the very nature of his work to represent not what he sees, but what he has seen—some attitude or expression necessarily fleeting, some aspect of nature necessarily lasting for too short a time to be reproduced save from memory. From a want of knowledge, or from failure to make sufficiently careful observations, the artist may overlook some essential characteristics of what he wishes to represent. He may combine incongruous elements in the delineation of facial or bodily expressions, he may represent a natural feature true enough in itself, in combination with another equally true in itself, which could not possibly be seen at one and the same time as the other. He may not, to use Macaulay's illustration, "mix August and January in one landscape"; but the same sort of reasoning may apply to his less glaring incongruities which Macaulay applied in the other case, and Horace, earlier in the passage which every schoolboy (as Macaulay would say) knows by heart. "Would it be a sufficient defence of such a picture to say that every part was exquisitely coloured, that the green hedges, the apple-trees loaded with fruit, the waggons reeling under the yellow sheaves, and the sun-burned reapers wiping their foreheads were very fine, and that the ice and the boys sliding were also very fine?" It is no better defence to say of Mr. Pettie's picture of Monmouth before James II. (30, Room I.), that from the knee downwards Monmouth's left leg is well drawn, while the rest of the body is, perhaps, placed as it might be if a man with bound arms tried to wriggle along the floor, when it is absolutely impossible that, with the body so placed, the lower part of the left leg could be seen as it is, unless it had been twisted round by main force through some forty-five degrees round the axis of the limb, to the dislocation of bones and the rending of muscles. To take another example from another department of painting:—The blue sea is charmingly represented in Mr. Brett's picture, "The Grey of the Morning" (506, Room V.). We have seen the sea as blue as that, and though it is not quite so level as the sea usually is (Mr. Brett's seas seldom are), the effect considered in itself is very pleasant. Again, the lower cumulus clouds, showing through the grey mist, are well presented, and in effective contrast with the "lily white clouds," which have "got up early and peeped over the wall." But neither Mr. Brett nor any one else has ever, except, perhaps, in a dream, seen that rich blue sea in the grey of the morning. These two characteristic features of his chief painting this year are charmingly represented, and in two different pictures would have been admirable, but in one and the same painting they are not admirable at all.

In figure painting, as in sculpture, anatomy is the science which has first to be considered in estimating the truthfulness of the artist's work. Every sculptor *must* thoroughly study anatomy, not the anatomy of the body at rest only, but of the body in motion; and every painter of the human figure should do so, for the eye alone cannot be

* The "Fishmonger's Stall," in 1879 was an "awful example."

trusted. If we begin our examination of the works at the Royal Academy with reference to this point, we find, even in some of the works of the best artists, errors which are difficult to account for. For instance, in Sir F. Leighton's charming "Day Dreams" (56, Room I), the outer carpal bone of the right hand is incorrectly represented. We do not say that it is not often so shaped. On the contrary, the deformity, for such it unquestionably is (though a slight one), is common enough. Possibly Sir F. Leighton's model, however beautiful in other respects, had a wrist thus malformed; but there was no occasion to reproduce the defect in an ideal painting. In his "Wedded" (71, same room), the lady's unhappy expression seems justified by the exceedingly unsatisfactory anatomical development of her young spouse, the muscles of whose limbs are without depth or fulness, and little more than surface markings. His more pretentious "Phryne at Eleusis" (307, Room III.) is open to anatomical exception, especially with regard to the lower extremities. But the artistic objections to this painting are much more serious than the scientific ones. If Phryne had really so charming a face, but so matronly a form, and flesh so strangely coloured, she would never, unless singularly unwise, have resorted to the expedient she actually adopted to disarm her judges at Eleusis. We take it the real Phryne relied on the perfect harmony of her proportions, the perfect delicacy and tenderness of every outline and every tint, to plead in her favour, not on fulness of development, or the mere evidence of a fine constitution (apart from a most abnormal complexion). Sir F. Leighton's Phryne, however, would have found her judges merciful if she had been content to let them assume that her figure was as perfect as her face.

(To be continued.)

OLD MAY-DAY.

THE day on which these lines appear—May 12—is Old May-day. We must not forget this in reading what our early poets say of the charms of May. The month of May has been shifted since the days of those poets. When Chaucer, for example, spoke of

> the sixth morrow of May,
> Which May had painted with his softè show'rs,
> This garden full of leavès and of flow'rs,

he meant the time of year corresponding to our 14th, not to our May 6. And again, in choosing May for the name of the naughty lady whom Sir January took to wife, Chaucer was assigning emblematically to May 9 the qualities described by the author of the "Menagiana." The reader will remember that Budgell, in the *Spectator* (No. 365), referring to this description of the kindly warmth infused into earth and its inhabitants during the month of May, expressed the opinion that "the observation is as well calculated for our climate as that of France," adding wickedly, "and some of our British ladies are of the same constitution as the French Marchioness of S., who told the author of the 'Menagiana' how dangerous she found the month of May." In his "Il Pastor Fido," Guarini describes May as—

> Bella madre di fiori,
> D' erbe novelle e di novelli amori,*

The May-day of the *Spectator's* time was that which I call Old May-day, viz., May 12, for it corresponds with May-day at the time when our Bradley "robbed the people," as they thought, "of their eleven days." But if the Old Style had continued till now, our present May 13 would be May-day, for the Julian Calendar caused the dates slowly to pass away from the seasons they had originally corresponded with. It is worthy of notice, however, that the May-day of the last century, though coming later in the true year—that is, the year of seasons—than May-day of Chaucer's time (our May 9), was not likely to be a warmer or brighter day, for from about May 11 to May 14, there usually occurs a singular, and as yet unexplained, "cold snap." On the average, May 9 is at least one degree Fahrenheit warmer than May 12, though our almanacs in giving the mean temperature make no note of this.

The change of style has altered the seasons notably since the days of Spenser, Shakespeare, and Milton. Then Midsummer fell at the very end of June, Midwinter at the very end of December. In one sense this accorded better with the actual changes of temperature than our present arrangement; for Midsummer is not the hottest nor Midwinter the coldest part of the year. The weather was far more likely to be cold and bitter on Old Christmas Day—our Twelfth-night—than it is on our present Christmas Day, when, indeed, the weather is as often soft and warm as bleak and cold. The last week of December was as apt in Shakespeare's time as in the second week of our January to be a time—

> When icicles hang by the wall,
> And Dick the shepherd blows his nail,
> And Tom bears logs into the hall,
> And milk comes frozen home in pail.
> When all aloud the wind doth blow,
> And coughing drowns the parson's saw,
> And birds sit brooding in the snow,
> And Marion's nose looks red and raw;
> When blood is nipp'd and ways be foul,
> And nightly sings the staring owl.

The old May-day ceremonies would show that a marked change had taken place in our seasons, if the May morn of former days fell in the same part of the year of seasons as our present May-day. Chaucer tells us, in his "Court of Love," that early on May-day "forth goth al the court, both most and lest, to fetche the flowris fresch, and braunch and blome." Even Kings and Queens rose early on May-day, to fetch green boughs or May boughs. This is probably the "rite of May" referred to in the "Midsummer Night's Dream,"—"No doubt," says Theseus, "they rose up early to observe the rite of May." On our May-day not many trees are green, but a few days at this season make a great difference in the aspect of the woods and fields, so that on Old May-day folks who rise early, and are not troubled by fears of the "great unpaid," might collect a goodly number of "flowris fresch; and braunch and blome."

Our poets, despite the effect of the change of style, which has thrown back May-day a full fifth nearer the spring equinox than it was in Shakespeare's time, still sing in the same strain of the merry month of May and of the delights of its opening days. But May-day is only poetically "the merriest, maddest day of all the glad New Year." The change of style has practically wrought a change of weather. It has brought down the Maypoles. A few chimney-sweeps may still carry round their Jack-in-the-Green; but the green is usually evergreen, not the fresh growth of the new May. We cannot wonder at the

* This line does not refer in the remotest way to the Derby or to new favourites.

melancholy fate of Tennyson's Queen of the May, if, on one of our modern May-days, she—

"Danced about the Maypole and in the hazel copse,
Till Charles's wain came out above the tall white chimney-tops."

Someone should have explained to the merrymakers that the sun had not yet attained a mid-day altitude which could justify these gambols in the hazel copse—that, in fact, they would probably result in what Sidney Smith calls "our British constitutional coughs, sore throats, and swelled faces." His advice would have been scouted, no doubt, and he would have been looked upon as a bore and a nuisance; but he would have been right all the same. Indeed, speaking seriously, in these days, when many, especially the younger folks, look back longingly on old customs, and occasionally try to revive them, it may sometimes be well to inquire even into such dull, dry details as the effects of the change of style in modifying the relations of dates and seasons.

CRYSTALS.

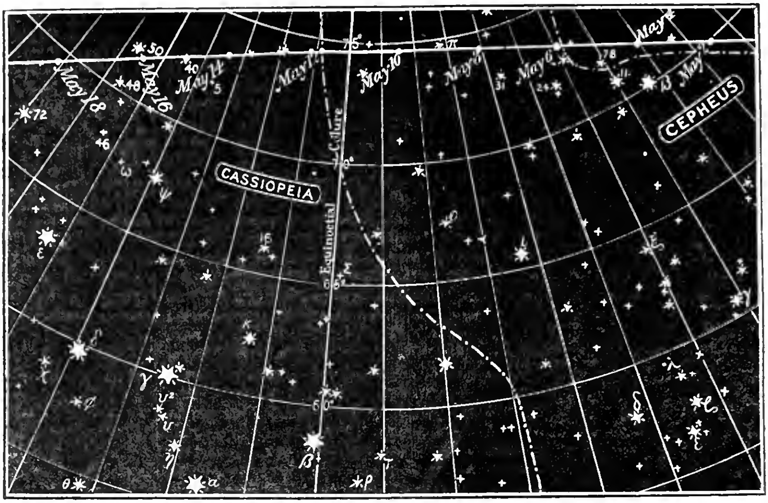
By William Jago, F.C.S., Assoc. Inst. Chem.

THE object of this and the succeeding papers of this series is to place before the readers of KNOWLEDGE an account of the Chemistry and Chemical Physics of different subjects of general interest in as plain and interesting a manner as is possible to the writer. It must not be supposed, however, that the result will be a regular and succinct chemical treatise; those requiring such can find them in abundance, and of all prices and qualities. The writer would rather treat his science so that they who run may read, appealing, he hopes, to those who, though busy in the shop, the field, the mart, have not ceased to take an interest in Science for her own sake. A very clear and interesting account of the properties of matter in the gaseous state, which appeared in one of the earlier numbers of KNOWLEDGE, led to an account of its behaviour in the solid condition being chosen as the subject of this introductory paper.

Let us start with the inquiry, "What is a crystal?" and probably at the outset we shall be met with an answer which, curiously enough, selects as its typical example the very substance of all substances that a chemist instances as the type of non-crystallised bodies. Nine persons out of ten will tell us, in response to our query, "A crystal is anything which is very clear, *like glass*." Go to the chemist or geologist and ask him whether a piece of that black, shiny lava poured out of a volcano is crystalline, and he will say, "No, it is a specimen of the glassy (vitreous) type of rock." Here, then, is one point at which the chemist at once joins issue with the popular idea. Let us in the next place ask him for his definition of a crystal, and we learn that almost every solid substance known has a tendency to arrange itself in a definite geometrical form, and that it is then said to be crystallised. This change of bodies from the condition in which their particles are arranged irregularly, to that in which each occupies a stated position, forming part of one harmonious whole, is frequently accompanied by remarkable changes in the appearance and properties of the substance. Carbon, known to every one in the humble and familiar guise of charcoal, crystallises and becomes a diamond; and further, this depends on its particles arranging themselves so as to form the figure known as an octahedron; with a change of the conditions, the same elementary substance varies the crystallised form it assumes, and instead of the hard and brilliant gem, we have produced six-sided plates of soft and metallic-looking graphite, or black lead. Nature carries on within her laboratory these wonderful changes, giving us little or no idea of what she is doing unless we watch and question her closely; her secrets are, however, readily revealed to him who questions aright. Owing to the value and beauty of the diamond, efforts have been made, from the days of the alchemist downwards, to obtain it artificially; these efforts have, however, met with but little reward. Nature has one element of success in her experiments which we can never have, and that is time. The diamond, sparkling in a lady's ring, no bigger than a pea, has, in all probability, taken for its growth not days or months, but years, reckoned by thousands, or perhaps hundreds of thousands! How, then, shall we imitate her? In this particular instance, probably in no way; but there are fortunately other substances known which crystallise more readily, and with these we may hope to have success. Most of the metals assume, under certain conditions, a crystalline form, and those particularly which are found native occur frequently as crystals. The Latrobe nugget, at present in the Natural History Museum, is a magnificent instance of crystals of gold; it consists of natural golden cubes, welded, as it were, together in one mass. Among the metals, bismuth is remarkable for its tendency to crystallise, and by following the directions given, a crystalline mass of bismuth is readily obtained. Take about a quarter of a pound of the commercial metal and melt it either in a small clean iron ladle or over a Bunsen lamp in a porcelain crucible; when quite melted, set the ladle or crucible on a cold metal surface. Let it remain perfectly still, and watch the bismuth carefully, until it is seen to solidify round the edges, then quickly pour out the metal still remaining liquid, and you have the whole of the interior lined with more or less perfect cubical crystals of bismuth. There is one striking peculiarity about these crystals, however. They are but skeleton crystals; the lines forming the edges of the cubes are there, but there is a depression in each face of the crystal evidently not as yet filled up. The growth of the crystal was arrested by pouring out the still liquid metal, and there we have not only shown us the shape of bismuth crystals, but also the manner in which the crystal grows.

For purposes of comparison, try now to make sulphur crystals. To do this, melt down roll sulphur in the ladle or crucible, using, however, a very gentle heat, and not prolonging it beyond the point at which the whole of the sulphur is melted; allow to cool in the same manner as with bismuth, wait until a crust has formed over the surface, and then immediately bore two holes through with a red-hot wire, the one for the liquid sulphur to run out, and the other to admit air. Pour out the sulphur still remaining liquid, and cut carefully round the upper crust with a penknife, remove it, and the whole of the interior is interlaced with delicate needle-shaped, amber-like, crystals of sulphur. Here, then, are two substances, of widely different appearance and properties, both possessing in common this property of crystallising, but with each there is a definite shape. Further experiment and observation teach us that the form of a crystal is as characteristic of a body as any other property it possesses. In the next paper the writer purposes to give further directions for the preparation of crystals, and hopes to add sketches of crystals as viewed by the microscope.

IN an early number, probably the next, an important series of papers by Miss Amelia B. Edwards, the eminent authoress and Egyptologist, on the question, "Was Rameses II. the Oppressor of the Hebrews?" will be commenced.



THE COMET.

We continue the path of the comet Wells to the end of next week.

POPULATION OF THE EARTH.

By the Editor.

Several correspondents ask whether a computation, such as that made in letter 391, p. 575 might not be based on conditions more in accordance with known facts. I suggest the following:—From the Registrar-General's return just presented to Parliament, it appears that for an estimated population of rather more than 26 millions in England, the deaths were, 491,813; the births, 883,508—an increase of 391,695,—say 390,000 for 26,000,000 (the real increase *slightly* greater). This is at the rate of 39 for 2,600, or 3 for 200—that is $1\frac{1}{2}$ per cent. Suppose now that 5,000 years ago the human population of the globe were one million (which, considering that according to every Egyptologist of standing, the Pyramids were built long before that time, and that 30,000 lives were *expended* in building them, must be considered a very moderate allowance), then, what would be the present population of the globe if there had been an increase of $1\frac{1}{2}$ per cent. per annum during the last 5,000 years?

The problem thus presented is very simple. The rate of increase per annum is from 1,000 to 1,015, so that we have merely to multiply one million (or whatever the population was 5,000 years ago) by $\frac{1015}{1000}$ five thousand times. Without logarithms this would be rather a difficult task; but with the aid of logarithms (see our papers on that subject) it is sufficiently easy:—

$$\log.\left(\frac{1015}{1000}\right) = \log.(1015) - \log.(1000)$$
$$= 3{\cdot}0064660 - 3$$
$$= 0{\cdot}0064660$$

Multiplying by 5000 we get,

$$\log.\left(\frac{1015}{1000}\right)^{5000} = 32{\cdot}330000$$
$$\log.\ 1{,}000{,}000 = 6$$

log. (earth's population under the assumed conditions) .... = 38·330000

Whence the earth's present population would be

213,800,000,000,000,000,000,000,000,000,000,000,000

if, being only one million 5,000 years ago, it had increased during the last 5,000 years at the same rate that the population of England has increased during the last year. If, 5,000 years ago, the population of the earth had been but 10, we should have to strike five digits from the above.

But, to show how very far the conditions attained in a civilised country are from those under which life could possibly exist for any length of time (as science measures time) on the earth as a whole, let us suppose that only 4,000 years ago (and even the believers in the Great Pyramid as a stone Bible will allow so much), the earth's population did not exceed 10. Then we have, to determine the earth's present population, this calculation:—

$$\log.\left(\frac{1015}{1000}\right)^{4000} = 25{\cdot}864000$$
$$\log.\ 10 = 1$$

log. (earth's population under the assumed conditions) ... ... = 26·864000

whence the earth's present population would be

731,140,000,000,000,000,000,000,000.

Now we have seen that with ten persons to the square yard (rather close crowding) there would be 30,000,000 to the square mile, and to the entire surface of the earth (land, water, and ice) 200,000,000 times as many, or

6,000,000,000,000,000.

Wherefore, even at the rate of increase which has taken place in this much maligned old country during the past year (of course the "good old times" were ever so much better), the earth's population, beginning with only 10,

four thousand years ago, would now suffice to closely pack the surfaces of about

122,000,000,000

such globes as this earth on which we live, or the surface of one large globe having a diameter exceeding the earth's nearly 5,000 times, or the sun's much more than 400 times. Yet in science 4,000 years count almost as nothing.

When we remember the rate of increase which is customary among those whose lives are easy—this being certainly much nearer 10 than $1\frac{1}{2}$ per cent. per annum—we see that, so far as the benefit of the greater number of surviving persons at any epoch is concerned, it is, on the whole, perhaps as well that during the last 4,000 years there have been certain destroying agencies at work to prevent the population of the earth from increasing so fast as it otherwise might have done.

It may, perhaps, be interesting, before we conclude, to inquire what would have been the actual average increase per cent. per annum if the population of the earth had increased from 10 four thousand years ago, to 1,500,000,000 now. If r be the increase per cent. per annum, we have

$$\log. \left(\frac{100+r}{100}\right)^{4000} = \log. (1{,}500{,}000{,}000)$$
$$= 9{\cdot}1760913$$

whence, since log. 100 = 2 we have

$$4000 \log. (100+r) = 8000 + 9{\cdot}1760913$$
$$= 8009{\cdot}1760913$$
$$\log. (100+r) = 2{\cdot}0022940$$
$$100+r = 100{\cdot}5304,$$

or the average rate of increase, on the assumptions made has been little more than $\frac{1}{2}$ per cent. per annum. As it is certain that the population of the earth 4,000 years ago consisted of many millions, the real average rate of increase must have been very much less even than this.

NIGHTS WITH A THREE-INCH TELESCOPE.

By "A Fellow of the Royal Astronomical Society."

The constellation Bootes, at which we now arrive (Map, p. 474), will be found a very mine of objects of interest by the incipient observer. We will begin by turning our instrument, armed with a power of 160, upon ϵ, a star which Struve well described as "pulcherrima" (or most beautiful). So viewed it will be seen as in Fig. *b*, p. 511.

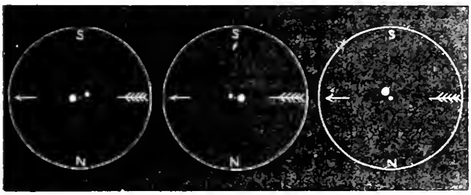


Fig. 27.—π Böotis. Fig. 28.—44 Böotis. Fig. 29.—39 Böotis.

the larger star being yellow, and the companion a bluish-green. π Böotis, an interesting and easy pair, when viewed with a power of 160 will be found to present the appearance shown in Fig. 27. ξ Bootis, is a little closer and somewhat more unequal pair, the colours of the components, moreover, being more strongly contrasted than in the case of the previous star. It is shown in Fig. *a*, p. 511. It is a wide and easy pair, which it is needless to figure. 44 Bootis, shown in Fig. 28, as seen with a power of 160, is interesting from the contrasted colours of its components. It is not numbered in the map, p. 474; but is one of two small stars forming a triangle with β and θ Bootis in it. Nor is our next object, 39 Bootis, numbered; but it is the north-western of the pair of stars in the map, and will be found in the sky, a little above and to the right of 44. In this, again, the colours are prettily contrasted. Its aspect as viewed with the same power as the preceding objects is represented in Fig. 29. κ Bootis, on the confines of Canes Venatici, is a wider and much more unequal pair. It is shown in Fig. 30. On a line drawn from Spica Virginis to ζ Bootis, and about 11° south (and a little east) of Arcturus, will be found the very pretty and interesting double star which we have

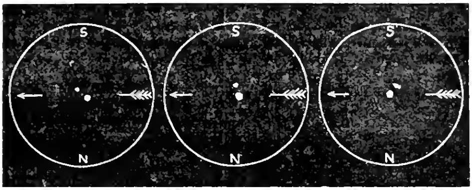


Fig. 30.—κ Bootis. Fig. 31.—P. xiv. 69. Fig. 32.—1 Böotis.

drawn in Fig. 31. It is 69 of the fourteenth hour of Piazzi's catalogue. The difference in colour of the components of this pair will at once strike the observer. He will, though, probably be puzzled to say exactly what the colour of the smaller star is, very discrepant conclusions on this subject having been arrived at. Some $8\frac{1}{2}°$ to the west, and just to the north of Arcturus, we shall find a very beautiful object, the star 1 Böotis, shown in Fig. 32. At the first glance the student will observe two stars, nearly of the same magnitude, and wide apart. It is the upper, or southern one of them, to which our attention must be directed. Looking at it carefully, we shall note the minute blue star shown in our figure to the south and very slightly to the east of its primary. We have omitted the second large star of which we have just spoken from our diagram, inasmuch as, using the scale to which it is drawn, such star would be just out of the northern, or lower, portion of the field. Finally, the student may, if he likes, look at ζ Böotis with the very highest power at his command; but, under the most favourable circumstances, he will only succeed in so far converting it into an egg-shaped object as to show that it is not single. Such are a few of the most easily-identifiable objects in this constellation. The number of purely telescopic double stars is very large indeed, but their necessary absence from our map of reference, and the impossibility of recognising them without an equatoreal provided with graduated circles, renders the mere mention of them here sufficient.

PHOTOGRAPHY FOR AMATEURS.

By A. Brothers, F.R.A.S.

PART VI.

The advantage of the negative form of photograph is that from it we are enabled to obtain an almost unlimited number of copies on paper, having the lights and shadows, as in nature. The paper used for the purpose may have a plain *matt* surface, that is without gloss, or it may have a surface of albumen. The plain paper is used

when the photograph is to be coloured. Both kinds may be purchased ready for use, excepting that it is not sensitised.* The necessary appliances, such as printing-frames, porcelain dishes, and other articles, which will be named in due course, it is assumed will have been supplied in the "outfit." We now require a solution of nitrate of silver, and this must be prepared in the proportion of 40 or 50 grains to each ounce of water. The quantity required will depend on the size of the pieces of paper to be prepared—say a quarter of a sheet; the dish, therefore, must be somewhat larger, and solution of silver must be poured in to the depth of about half-an-inch; then take the paper by two corners, and place it on the solution so that it will fall down evenly. After a few moments, with a piece of horn or wood, lift the paper by one corner, so that it may be seen that there are no air-bubbles, which, if any are present, may be removed by a puff of breath, or they may be touched. The paper is to be then replaced, and left for about three minutes—not longer. It may then be lifted slowly, and, after draining the surplus solution (this may be facilitated by drawing the paper over the edge of the porcelain dish) the sheet may be suspended to a line by American "clips," or it may be dried at once by fire-heat. Cut the paper, which must be quite dry, to the size required. Place the negative face, or picture side, upwards in the pressure-frame; put the paper carefully on the negative, then put on the back of the frame, the paper being pressed close by springs or screws, care being taken not to apply more pressure than is necessary to keep the paper and negative in perfect contact. The frame is then to be placed in strong daylight; diffused light is preferable to the direct rays of the sun.

While we suppose the printing is proceeding, we may say that the strength of the silver solution should not be allowed to fall much below 10 grains to the ounce, otherwise the prints will be weak. A good plan is to keep a stock of full strength always ready, and always fill up the bottle of solution to replace the portion used. If it should be found that the silver solution becomes discoloured by use, the colour may be removed by the addition of kaolin or China clay; shake well after each time of using the bath, and when the kaolin has subsided, the silver solution will be found to be quite clear, and may be poured off for use when required. Filtration is sometimes desirable. If the surface of the silver has any scum upon it, this is a sure sign that the bath should be filtered, but the scum can sometimes be removed by drawing the edge of a piece of blotting-paper over the solution.

The strength of the solution of silver may be determined with sufficient accuracy by means of the *argentometer* sold for the purpose.

The progress of the print must be watched. It will be noticed that the paper projecting beyond the edges of the negative has changed to a dark tint. If the negative be a *dense* one, this darkening may be allowed to proceed until the paper is nearly black. The frame should now be removed out of the full daylight, and one-half the back of the frame opened, when, on turning back the print, it will be seen whether the printing is deep enough, and it requires some little experience to determine this. The print should be rather over printed, as the fixing will reduce it to some extent. If not printed deep enough, the finished print will appear weak and unsatisfactory. When as many prints as are required are ready, they should be put, one at a time, into water, in order to remove the excess of chloride of silver; after a few minutes' washing, the water may be poured into a jar for the purpose of recovering the silver, and then, after one or two changes of water (if the second washing water appears milky, it still contains silver), the prints are ready for the next operation—toning.

If the prints were to be *fixed* without toning, they would not have a pleasing colour. We now require another solution, which contains chloride of gold. This salt is supplied in small glass tubes containing 15 grains. The tube may be broken and thrown into a bottle containing 15 ounces of water; there will therefore be one grain of gold to each ounce of water. The use of this solution of gold is for the purpose of changing the colour of the print; the change produced being a combination between the gold and silver in the paper, the gold giving a better colour to the finished print, and the gold-toned print is also more permanent.

There are many kinds of toning-baths, but the one which gives good results, is easy to make and use (it may be used as soon as made); it consists of one ounce of the solution of gold, and, say, 20 ounces of water poured into a porcelain dish. A small piece of *litmus* paper put into the solution will show that it is acid. A lump of carbonate of soda (common washing soda) may be put into the solution, and stirred about until the litmus-paper shows that the solution is slightly alkaline. The prints may now be put into this alkaline gold solution, face downwards, a few at a time, and they must be kept in constant motion, to insure equal toning. As soon as the prints present a pleasing colour, they may be removed into a dish of clean water, and when all are toned, they are ready for the next operation—fixing. It may be remarked here that one grain of gold will be sufficient to tone a sheet of paper; therefore the proportion to be used for each batch of prints must be regulated accordingly.

The *fixing* is effected by means of hyposulphite of soda, in the proportion of 3 ounces of the salt to 20 ounces of water. The prints must be left in the soda solution about 20 minutes, and they must be kept in motion for a time, to insure even action in fixing. When fixed, the prints must be removed to a dish of water, and the water must be frequently changed during the first half-hour. The prints may now be left during the night in the water, and the following day, after several changes, they may be placed between clean cloths or blotting-paper, and if they have been previously *trimmed*, they are ready for mounting.

* If preferred, paper may be purchased ready sensitised, but we assume the amateur will wish to prepare his own.

THE COMING TRANSIT OF VENUS.

By R. A. Proctor.

I PROPOSE to give a short and simple account in these pages, in the next few weeks, of the circumstances under which transits of Venus occur, explaining the general principles on which the determination of the sun's distance by observation of Venus in transit depends. But, as many questions have been addressed to me respecting the places where the whole transit—its beginning and its end—will be seen, it appears well to give at the outset, for immediate reference, a chart showing from what parts of the earth the transit can be seen wholly or in part.

The following preliminary explanation may be useful, however:—

Venus circuiting around the sun in a smaller orbit than the earth, and completing a circuit in 224·7 days, whereas the earth takes 365¼ days, passes between the earth and sun at intervals of about 583·9 days, or a year and seven months. If she travelled in the same plane as the earth, she could not thus pass between the earth and the sun

without seeming to cross the sun's face centrally. But as her path is inclined to the plane in which the earth travels, she is sometimes slightly on one side, sometimes slightly on the other side of that plane, when she is passing between the earth and the sun, so that sometimes she passes above, or north of the sun, sometimes below, or south of him, and is not seen crossing his disc. When, however, it so chances that Venus comes between the earth and the sun at or near the time when she is crossing from one side to the other of the earth's plane, there occurs a transit. This can only happen, of course, at two certain times in the year—viz., at or near the time when the earth is crossing the line through the sun, along which the two orbits—the earth's and Venus's—intersect. One of these times in the year is about June 8, the other about Dec. 7, and a transit of Venus never occurs except at or near one of these dates.

Now, when one of these transits occurs, certain parts of the earth are suitably placed for seeing, either the whole transit, or the beginning or end of it, while from certain positions of the earth no parts of the transit can be seen.

Let us see how such places are determined :—

Suppose that *e* (Fig. 1) represents the earth, *ss'* representing the sun. In reality, both are enormously exaggerated in dimensions compared with the distance separating them. Suppose a cone, *sks'*, enclosing the earth and sun, in the way shown, to travel round with the earth ; and also another cone, *sls'*, touching both the earth and the sun,

Then the transit is over : (and no more transits of Venus will be seen till the year 2004.)

While Venus is between 2 and 3, she is visible on the sun's disc from every part of the earth's surface, turned sunwards, and the portion whence she is thus seen at some time or other is of course more than a hemisphere, seeing that when Venus is at 2, a hemisphere of the earth is turned sunwards, and a different hemisphere when Venus is at 3.

Now, in the large chart there are shown :—

(1) The semi-circles bounding the region (approximately a hemisphere) whence the beginning of the transit is seen. These are marked "transit begins at sunrise," "transit begins at sunset."

(2) The semi-circles bounding the region (approximately a hemisphere) whence the end of the transit is seen. These are marked "transit ends at sunrise," "transit ends at sunset."

Over the region common to both (1) and (2), the whole transit is seen, except in the small region (shaded in the chart), where, though both the beginning and end are seen, a part of the mid-transit is not seen. Over the region belonging to neither (1) nor (2) no part of the transit is seen, except in two small regions (shaded in the chart), whence, though neither the beginning of the transit nor the end is seen, a part of the mid-transit is seen.

Over region 1, a series of dotted lines are drawn, showing how much the Ingress of Venus, or the beginning of the

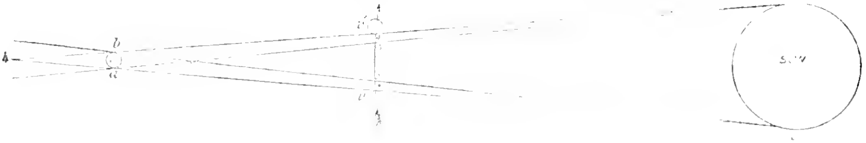


Fig. 1.

but on opposite sides of the vertex *l*. Now, imagine Venus to come along, gaining on the earth, and passing (as she does when there is a transit) through the double cone, at the points 1, 2, 3, and 4 ; 1 being the point where she first reaches the outer surface of the exterior cone ; 4, the point where she finally leaves that cone ; while 2 and 3 are the corresponding points for the inner cone.

Now, a little consideration will show that when Venus reaches the point 1, the transit begins, but only for that particular point where the line from *s'* to 1 produced touches the earth. *There* the transit begins earliest of all. As Venus passes from 1 to 2, transit begins for different places on the earth, until when Venus is at 2, transit begins from just that point where the line from *s'* to 2 produced touches the earth. The face of the earth turned towards the sun during the short time (less than half-an-hour), during which Venus passes from 1 to 2, does not change much ; so that one may say that over a hemisphere of the earth the beginning of the transit is seen, but for about half of that hemisphere the beginning is seen earlier and over the other half later than from the earth's centre (if we could imagine an observer, stationed at that inconvenient spot, to be able to watch the transit).

After passing 2, some six hours elapse before Venus comes to 3, when the end of the transit occurs at its very earliest for the station where *s*3 produced touches the earth ; and then, as Venus passes from 3 to 4, the end is seen at different stations on the earth ; until, finally, when Venus reaches 4, the end is seen at its very latest from that point where the line *s*4 produced touches the earth.

transit, is hastened or delayed (Ingress Accelerated or Retarded, marked "I A so many minutes," or "I R so many minutes").

Over the region 2, a series of dotted lines show how much the Egress of Venus, or the end of the transit, is hastened or delayed (E A or E R).

Then, over the region whence the whole transit can be seen, a series of heavy lines show how much the duration exceeds (+) or falls short of (−) the average.

The places where the duration is greatest and least are marked by heavy black dots, beside one of which is marked the maximum amount of shortening of the duration. (The other being on a part of the earth whence none of the transit can be seen, is not so marked.)

The map is very easily interpreted :—

Thus, in England, say London, we see that only the first part of the transit can be seen, and not much of that, the transit beginning shortly before sunset : ingress is retarded about five minutes. At New York the whole transit can be seen : ingress is retarded nearly eight minutes, and egress accelerated rather more than seven minutes, duration falling short of the mean nearly fifteen minutes.

And similarly for any other places where either the beginning, the end, or the whole transit can be seen.

Reviews.

"A RIDE ACROSS THE CHANNEL."*

Although Colonel Burnaby's balloon ride was not intended for scientific observation, this book has interest for the student of science, besides being a graphic account of a balloon ride. The greatest height attained by Colonel Burnaby in this ride was about two miles, and at that elevation the temperature was four degrees below the freezing point, while at an elevation of 500 feet, a few minutes before, the temperature had been 18° in the shade. The account of the balloon's descent suggests, as usual, that balloonists might with advantage devote some of that ingenuity to devise safe ways of bringing a balloon to rest on the ground, which has hitherto been fruitlessly expended on attempts to guide the balloon through the air. As a scientific statement, Col. Burnaby's assurance respecting the effect of the balloon's apparition on hens is open to question. "Thank Heaven I have seen it," cried a middle-aged female. "It passed over my house like the dome of a cathedral; and all my hens are still in convulsions of fright at its appearance." On which the advocate of Cockle's Antibilious Pills gravely assured her that "the apprehensions of her hens would not diminish, but rather increase, their laying powers." He should have added that all eggs thereafter laid by those hens, would be marked with a balloon in full career "like the dome of a cathedral."

FLOWERS IN MAY.

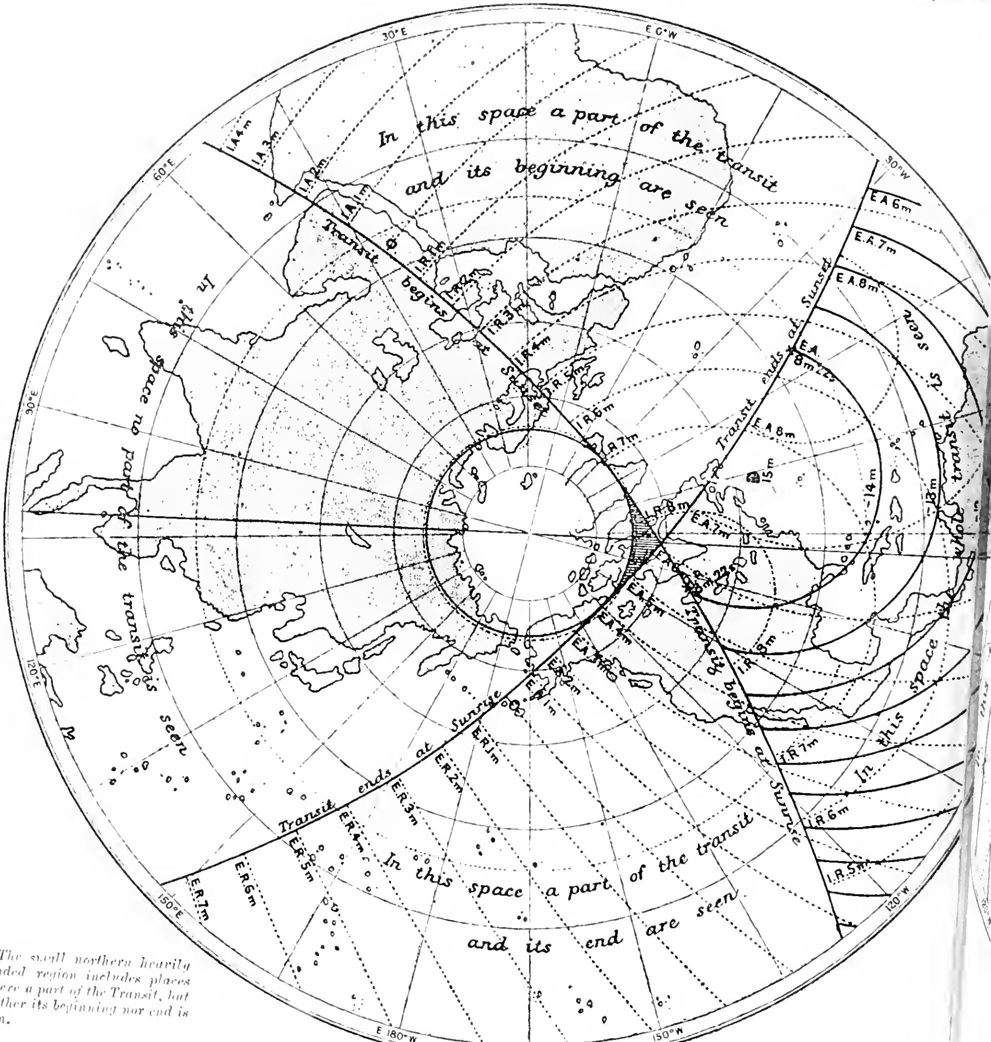
At the opening of May, by far the commonest buttercup in our meadows is the bulbous species (*Ranunculus bulbosus.*) It grows almost everywhere. Buttercups as a group may be always easily recognised by pulling out the petals, when they will be seen to have a small hollow scale near the base. The bulbous kind is known both by the way its calyx is turned back tightly against the stalk, and by the rough bulb formed by the lower part of the stem. As the month wears on, the tall meadow buttercup (*R. acris*) becomes commoner in the fields: its calyx grows in the normal fashion, enclosing the petals, and the middle division of its leaves starts from the same point as the outer ones. A third species found almost as universally is the creeping buttercup (*R. repens*), exactly like the last in most respects, but with rooting runners and the central leaflet on a separate stalk, apart from the two-side leaflets. The water crowfoot (*R. aquatilis*) has white flowers and finely-divided submerged leaves with larger floating ones: it is common in shallow, muddy water. The ivy-leaved crowfoot (*R. hederaceus*) differs from it only in the absence of the submerged leaves; it creeps on mud beside the water. Of the rose family, two or three little potentillas may be found abundantly. They have yellow flowers, and may be roughly recognised by their double calyx. The tormentil (*P. tormentilla*) has only four petals; it grows on high, windy places. The cinquefoil (*P. reptans*) has five petals and five divisions to its strawberry-like leaf. Silver-weed (*P. anserina*) has similar flowers, but many little leaflets arranged in two rows on each side of a long stalk. Both these are road-side weeds. Herb-bennet (*Geum urbanum*) much resembles the potentillas in flower and in its double calyx, but is a taller and weedier plant, with little hooked fruits forming a sort of rough burr. It is common in hedgerows. The hedge itself blossoms now too; its bawthorn bushes are also roses by family; notice their single calyx. The pinks are another family well represented this month. As a rule, most of the small white flowers growing in ordinary situations, with a single capsule in the centre of the flower, filled with seeds arranged centrally around an axis, are almost sure to be pinks. The smaller kinds have the calyx in separate pieces. Of these, mouse-ear chickweed (*Cerastium vulgatum*) has five split petals, and a capsule funnily cocked up at the end and opening in ten teeth. It grows everywhere. Common chickweed (*Stellaria media*) has flowers much the same in appearance, but its caps ule opens in five valves, and it may easily be recognised by a single line of hairs running down one side of the stem. Sandwort (*Arenaria trinervis*) looks very like the last, and can only be discriminated by its petals, which are entire instead of being two-cleft, and by the absence of the line of hairs. The larger pinks have the calyx united into a sort of cup or tube. Two of them are common this month—red campion (*Lychnis diurna*), which is pink, with scentless flowers; and white campion (*L. vespertina*), which is white and scented. Ragged Robin (*L. flos-cuculi*) is very like the first-named, but has much-divided petals, and a less swollen calyx. The veronicas are another group that can be well studied in May. They have blue flowers, the petals united into a tube at the base, four lobes to the corolla, and only two stamens. These peculiarities will at once distinguish them from any other English plants. Some of them have the flowers arranged in leafless spikes starting from the axils of the leaves. Two such may be found in May: *V. chamædrys*, the germander, with hairy leaves and two lines of long hairs, one on each side of the stalk; and *V. beccabunga*, brooklime, with smooth leaves and hairless stem. The first haunts road-sides, the second running streams. Another set of Veronicas has the flowers solitary in the axils, not in spikes. One such, with little shining leaves and tiny white blue-streaked blossoms, growing among grass in fields, is the thyme-leaved speedwell (*V. serpyllifolia*); another, with ivy shaped leaves, is the *V. hederifolia*; a third, with the upper leaves reduced to mere long, lance-like bracts, is the wall veronica (*V. arvensis*). Two others, very common in fields, are Buxbaum's and the procumbent speedwell. They may be known from the others by their broad, toothed leaves, not ivy-shaped, and by their upper leaves like the lower ones, only smaller, but they are harder to distinguish from one another. Buxbaum's (*V. Buxbaumii*) has a capsule twice as broad as long; the procumbent speedwell (*V. agrestis*) has it about the same breadth as length. We have only room for one other family, the orchids, known by their spur and their tuberous roots, as well as by their curious twisted ovary. The green-winged orchis (*O. morio*) may be recognised by its green-veined sepals; it is a southern plant only. The military orchid (*O. militaris*), with a handsome spike of purple-red flowers, and a long, two-cleft centre lobe to the lip, belongs only to the counties around London. The male orchis (*O. mascula*), with a pair of spreading sepals, as if winged for flight, is over early in the month. The spotted orchis (*O. maculata*) with lobed tubers and a very dense spike, lasts through the whole of May. Altogether, several hundred plants flower in May, and of these at least a hundred and fifty are common everywhere, so that it is necessary to make a selection; but whoever masters these five groups to start with, will have done a good month's botanical work.

* "A Ride across the Channel, and other Adventures in the Air." By Col. Fred Burnaby. (London: Sampson Low, Marston & Co. 1882.) One Shilling.

Solar Apparatus.—It will be remembered that M. Mouchot, a short time ago, devised an apparatus for utilisation of solar heat, and that M. Pifre made some important improvements on it. Very different views have been taken as to the practical utility of such an apparatus. Some help towards a right judgment now comes from Montpellier, where a French Government Commission has been experimenting with the apparatus for a year (1881). Another commission has experimented at Constantine, in Algeria, but the results are not yet published. The apparatus was of the known form—a concave mirror, with blackened boiler in the axis, surrounded by a glass envelope. The steam from the boiling water was condensed in a coiled tube cooled by water. The weight of water distilled in an hour indicated the amount of heat utilised; and observations with an actinometer from hour to hour showed the amount of incident heat. The rates of these two quantities was a measure of the economic efficiency of the apparatus. The temperature and moisture of the air, &c., were also carefully noted. The number of days of observation was 177, and of observations 930, and water was distilled to the amount of 2,725 litres. Without entering much into numerical detail, we may state that while the heat utilised in the most favourable circumstances per square mètre per hour would be about equal to that utilised from 240 grammes of coal (supposing about a half to be utilised)—even the half of this is not attainable in our climate. The sun does not shine continuously enough for practical utilisation of the apparatus. In very dry and hot climates, the possibility of utilisation would depend on various circumstances, such as the degree of difficulty of procuring fuel, the price and facility of transport of solar apparatus, &c. We note in the report (by M. Crova) that the efficiency of the apparatus is not proportional to the heat intensity of the solar radiations, and hardly ever varies in the same sense. The absolute quantity of heat utilised, on the other hand, depends essentially on the temperature of the air; the higher this is, and the less consequently the cooling, the greater the amount of heat utilised.—*The Times.*

CHART OF THE TRANSIT OF 1882

Showing the lines of equal acceleration and retardation of ingress an

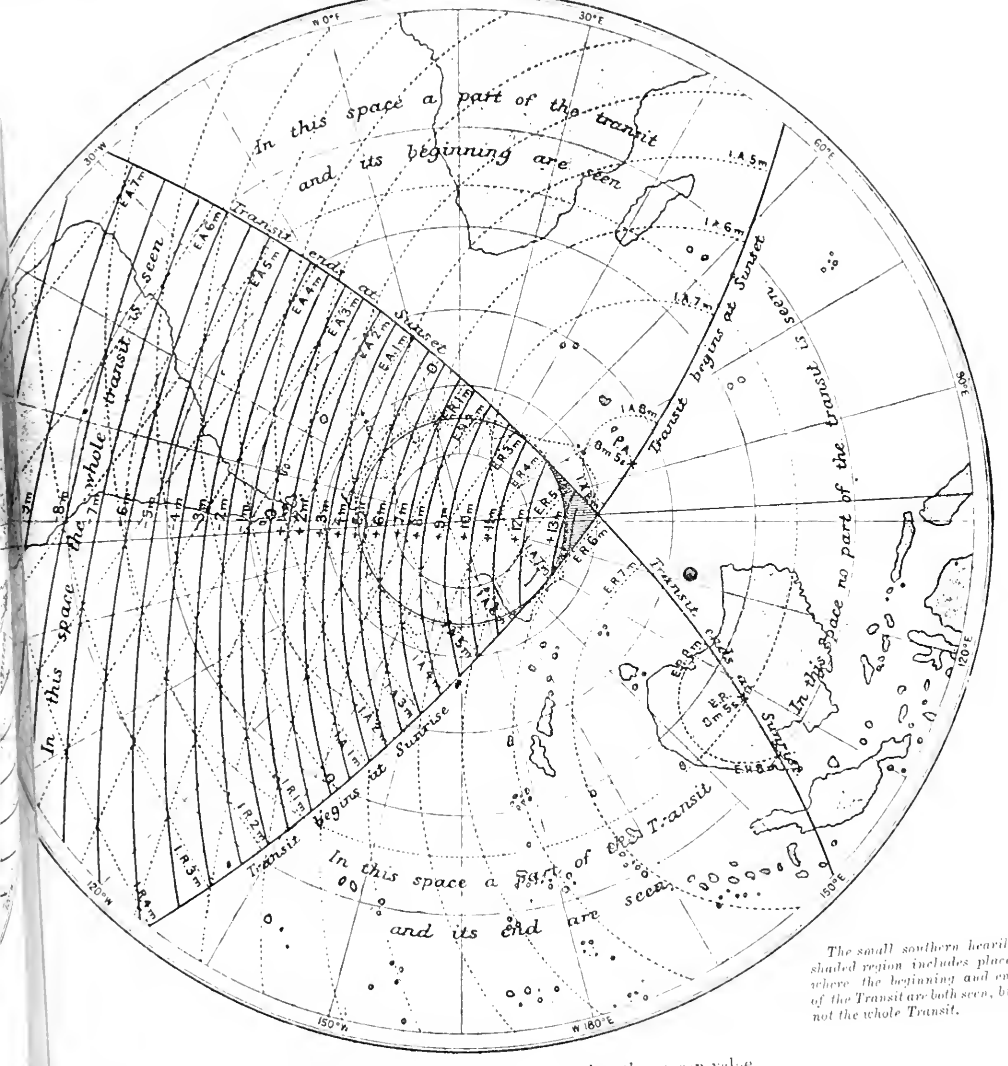


The small northern heavily shaded region includes places where a part of the Transit, but neither its beginning nor end is seen.

Abbreviations.—I.R. 5m. = Ingress Retarded 5m.; E.A. 6m. = Egress Accelerated 6

IE STEREOGRAPHIC PROJECTION,

; and also the lines of equal duration (——) for external contact.



The small southern heavily shaded region includes places where the beginning and end of the Transit are both seen, but not the whole Transit.

Duration 7m. greater than mean value; − 8m. = Duration 10m. less than mean value.

WEATHER REPORT, FOR WEEK ENDING SATURDAY, MAY 6.

ABERDEEN.

Station. Day of Week.	Sun.	Mon.	Tues.	Wed.	Thurs.	Fri.	Sat.
Thermo'ter in Shade. Max.	50	55	51	52	55	54	56
Thermo'ter in Shade. Min.	39	35	42	36	45	35	40
Day's Range	11	20	9	16	20	19	16
Barometer. Inches. 100ths	29 ·31	29 ·53	29 ·60	29 ·88	29 ·61	29 ·80	29 ·83
Wind. Direction	N W	S	S	S S E	W S W	N N W	S S E
Wind. Force (0 to 12)	6	1	6	2	1	3	3
Weather. Beaufort Scale*	c	b	b c	b	b c	b	o
Rain-fall. 100ths of inch	3	0	12	4	30	0	0

LIVERPOOL.

Station. Day of Week.	Sun.	Mon.	Tues.	Wed.	Thurs.	Fri.	Sat.
Thermo'ter in Shade. Max.	47	50	52	58	57	54	55
Thermo'ter in Shade. Min.	34	38	42	46	45	42	43
Day's Range	13	12	10	12	12	12	12
Barometer. Inches. 100ths	29 ·61	29 ·51	29 ·73	29 ·83	29 ·69	29 ·84	29 ·92
Wind. Direction	W S W	S S E	S S E	E S E	W	W	N
Wind. Force (0 to 12)	3	3	2	2	2	2	2
Weather. Beaufort Scale*	b	o	b c	m	o	b c	b
Rain-fall. 100ths of inch	105	9	14	2	16	0	20

VALENCIA.

Station. Day of Week.	Sun.	Mon.	Tues.	Wed.	Thurs.	Fri.	Sat.
Thermo'ter in Shade. Max.	49	61	54	57	54	56	57
Thermo'ter in Shade. Min.	36	37	42	45	36	36	48
Day's Range	13	24	12	12	18	20	9
Barometer. Inches. 100ths	29 ·51	29 ·22	29 ·70	29 ·73	29 ·78	29 ·88	29 ·90
Wind. Direction	S W	S W	W	N N E	—		N W
Wind. Force (0 to 12)	6	7	3	3	0	0	1
Weather. Beaufort Scale*	c p	q p c	b c	o p	b	b c	c
Rain-fall. 100ths of inch	34	59	10	12	0	0	38

LONDON.

Station. Day of Week.	Sun.	Mon.	Tues.	Wed.	Thurs.	Fri.	Sat.
Thermo'ter in Shade. Max.	54	56	59	61	71	58	65
Thermo'ter in Shade. Min.	37	41	43	47	51	46	49
Day's Range	17	22	16	14	20	12	16
Barometer. Inches. 100ths	29 ·80	29 ·72	29 ·92	29 ·82	29 ·65	29 ·86	29 ·88
Wind. Direction	S W	S S W	S S W	E N E		S W	S W
Wind. Force (0 to 12)	1	4	3	3	0	2	1
Weather. Beaufort Scale*	b c	b c	b c	o	g r m	o	t
Rain-fall. 100ths of inch	21	12	1	0	15	12	8

LYONS.

Station. Day of Week.	Sun.	Mon.	Tues.	Wed.	Thurs.	Fri.	Sat.
Thermo'ter in Shade. Max.	64	—	54		76	63	65
Thermo'ter in Shade. Min.	46	—	46		57	48	45
Day's Range	18	—	8	—	19	15	20
Barometer. Inches. 100ths	38 ·10	—	30 ·11		29 77	29 ·96	30 ·[illegible]
Wind. Direction	E		E S E		S W	W	S S E
Wind. Force (0 to 12)	1		1		1	1	1
Weather. Beaufort Scale*	o		r		o	o	b
Rain-fall. 100ths of inch	8	—	35		0	51	0

* WEATHER.—*Beaufort Scale* is, b. blue sky; c. detached clouds; d. drizzling rain; f. fog; g. dark, gloomy; h. hail; l. lightning; m. misty (hazy); o. overcast; p. passing showers; q. squally; r. rain; s. snow; t. thunder; u. ugly, threatening; v. visibility, unusual transparency; w. dew.

Letters to the Editor.

[The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with their writers. All communications should be as short as possible, consistently with full and clear statements of the writer's meaning.]

All Editorial communications should be addressed to the Editor of KNOWLEDGE; *all Business communications to the Publishers, at the Office, 74, Great Queen-street, W.C.*

All Remittances, Cheques, and Post-Office Orders should be made payable to Messrs. Wyman & Sons.

**** All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.*

All Letters or Queries to the Editor which require attention in the current issue of KNOWLEDGE, *should reach the Publishing Office not later than the Saturday preceding the day of publication.*

(I.) Letters to have a chance of appearing must be concise; they must be drawn up in the form adopted for letters here, so that they may go untouched to the printers; private communications, therefore, as well as queries, or replies to queries (intended to appear as such) should be written on separate leaves.

(II.) Letters which (either because too long, or unsuitable, or dealing with matters which others have discussed, or for any other reason) cannot find place here, will either be briefly referred to in answers to correspondents, or acknowledged in a column reserved for the purpose.

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

"God's Orthodoxy is Truth."—*Charles Kingsley.*

Our Correspondence Columns.

SCREW-DRIVER TUBES.

[395]—I am afraid screws would not take to my friend Mr. Ellis's spiral tubes as kindly as ulcers do, even though it has since been extended (as he doubtless knows) so that you can drill a hole in a tooth—and the cheek behind it—in no time, with your foot, by a drill at the end of a long spiral of that kind, which will work round any bead. But I do not write merely to say that, but to add to my original mode and simple slate-pencil tube, that it may be made of steel, and have three slits, besides the opening in the lower 2 inches of it, and the pieces may then be flattened and bent outwards a little, enough to hold the largest screw-head. A ring slipped over them will bring the fangs together, enough just to fit smaller screws. Whether you see your screw or not, you can very soon feel if it is going crooked—the feeling is too enraging to mistake for a moment. I observe (in both senses) your notice about brevity of correspondence.

EDM. BECKETT.

CONSERVATION OF SOLAR ENERGY.

[396]—I give now in full Dr. Siemens's letter referred to at p. 566 last week, omitting only those references to Mr. Archibald, to whom he was replying, which do not belong to the letter, regarded as a reply to my own reasoning in the *Cornhill Magazine*, and at pp. 565-566 of KNOWLEDGE.

Dr. Siemens's letter, then, runs thus:—[Mr. Proctor] has missed the principal point of my argument concerning solar fan-action. I showed pretty clearly I thought that solar gravitation would affect the inflowing and the outflowing currents equally, and that centrifugal action must determine motion in the equatorial direction in a space filled with matter. But, to put the problem into a mathematical garb, let us consider the condition of two equal masses, m_p and m_a, both at the radius R from the solar centre, the one opposite either pole, and the other opposite the equatorial region. The moment of gravitation of both these masses will be represented respectively by $\frac{gm_p}{R^2}$ and $\frac{gm_a}{R^2}$, and supposing both masses to be gaseous, and of the same chemical composition and temperature, they will represent equal volumes, say one cubic foot.

These conditions being granted, we may put—

$$\frac{gm_p}{R^2} = \frac{gm_a}{R^2},$$

but the mass m_a is subject to another force, that produced by tangential motion, which shall be represented by v, and the centrifugal

force resulting from this motion by ϕv; [Why put it vaguely thus, writing "function of v," when we know what the centrifugal tendency is, viz., $\frac{m_a v^2}{R}$?]

the moment of gravitation towards the sun will then be reduced to $\frac{gm_a}{R^2} - m_a\phi v$, and the latter factor being a positive quantity we have—

$$\frac{gm_p}{R^2} > \frac{gm_a}{R^2} - m_a\phi v.$$

This inequality of attractive moments must determine motion toward the sun in favour of $\frac{gm_p}{R^2}$, and this condition holding good for any value of g and R, it follows that the polar inflow and equatorial outflow must take place, provided only that space is not empty, as supposed by Laplace, but filled with either an elastic or non-elastic fluid.

To put it in another way, Mr. [Proctor] imagines that, in order to determine an outflow from the sun, it is necessary for the centrifugal moment $m_a\phi v$ to exceed the moment of gravitation $\frac{gm_a}{R^2}$, whereas according to my view, the value of the former determines only the rate of outflow, but is immaterial as regards the principle of action. The projection of dust is entirely dependent upon the outflowing current. I leave it for Mr. [Proctor] to determine for himself the velocity of current necessary to move a particle of dust of given size and weight away from the sun in opposition to its force of gravity, which I am well aware is twenty-seven times that of the earth on its surface.

The gaseous current is of course produced at the expense of solar rotation, but this expenditure of energy is relatively much smaller than that lost to our earth through tidal action, and may be neglected for our present purposes. It is, moreover, counterbalanced by solar shrinkage, as explained in my paper.

C. WM. SIEMENS.

[I fear Dr. Siemens' way of treating this question is but too correctly described by himself as putting it in a mathematical garb, for there is only the garb of mathematics, not the thing itself, in the above discussion, and even the garb is not quite correct. For instance, in mathematics the term "moment" is not used as in Dr. Siemens' letter, nor in any way even approaching to his use of the term. One can tell, of course, very clearly what Dr. Siemens means, and therefore, perhaps, it is unimportant whether he correctly expresses his meaning or not—except as showing that discussions of this kind are somewhat outside the usual course of his inquiries and reading (and also, what was, however, already known, that the Editor of *Nature* has no very profound mathematical knowledge). Turning, then, to Dr. Siemens' meaning, I note that, in the first place, proving that a cubic foot opposite the pole and another opposite the equator are unequally attracted towards the sun's centre, by no means suffices to prove that either will move in any particular way. It would be the tendencies of *neighbouring* cubic feet we should have to consider, not those of two cubic feet hundreds of thousands of miles apart. Inflow of a mass of vapour opposite either pole would depend on the state of the gaseous matter immediately below it, and it can very readily be shown that the pressures which would exist opposite the polar regions, and the consequent resistance to inflow, would be greater, not less, than at equal distances in equatorial directions. But the chief objection to Dr. Siemens' reasoning (I was about to call it specious, but it is not so) lies in this, that he considers a certain consequence which would not even follow at all, as though it not only would certainly follow, but having followed, would leave things as they were, so far as the circumstances causing inflow and outflow are concerned. Under the impossible conditions he describes, equilibrium would be unstable (though he does not, as I conceded for the sake of argument last week, prove this), and movements tending to restore equilibrium would accordingly take place; but Dr. Siemens assumes, in effect, that there will be no tendency towards equilibrium, but that the forces tending to produce motion will remain all the time unchanged. It is as though having shown that the water forming the hollow of a wave tends to rise, one were to assume that it will rise for ever.

What Sir John Herschel said of the theory that the Zodiacal light is "a solar atmosphere in any proper sense of the word" is true of Dr. Siemens's supposititious atmosphere, "the existence of such a gaseous envelope propagating pressure from part to part subject to mutual friction in its strata, and therefore rotating in the same or nearly the same time with the central body, and of such dimensions and ellipticity, is utterly incompatible with dynamical laws." The case is certainly not strengthened by reasoning which, while endeavouring to show that the more aggregated parts of the supposed atmosphere have such a figure as is attributed to the outer corona and the zodiacal, assumes nevertheless the possibility of equal densities at equal distances opposite the polar and equatorial regions.

Mairan's views involved rather an excess than a deficiency of centrifugal tendency, and what Laplace did was to show not that a solar atmosphere would extend no further than a certain distance under any conditions, but that no such atmosphere could, beyond a certain distance, share in the solar rotation, without being entirely freed from any tendency sunwards. This does not seem to be what Dr. Siemens supposes to have been Laplace's reasoning, seeing that his views would be rather supported than opposed by such freeing of gaseous matter to travel outwards.

Again, there is all the difference in the world between the effect attributed to solar rotation in constantly expelling gaseous matter throughout an enormous extent of space around the sun, and that tidal action which affects the earth's rotation, not by the actual motion of the ocean, but by the mere transmission of wave states.

However, the points touched in the two preceding paragraphs are relatively insignificant.—RICHARD A. PROCTOR.]

CONSUMPTION.

[397]—Anything coming from so brilliant an intelligence as that of Professor Tyndall deserves attention. In this instance, at any rate, he has cut before the point. His conclusions do not justify his premises, his premises his conclusions. They remind one of a statement of his made a few years back, that the air of a sick chamber, by passing through cotton wool, might be made pure as the air of the Upper Alps, forgetful that the carbonic acid of respiration could not be thus eliminated, and the atmosphere in so far rendered pure. Professor Carpenter, going on some statements of Professors Villemin and Klebs, informed us that tubercle was owing to a micrococcus or microphyte, a little berry or little plant. Now, we are asked to believe that it depends on a little stick or *bacillus*, as Professor Koch terms some presumed organism. These inquirers, however, are led away on a wrong scent. If there be a micrococcus or *bacillus* met with in tubercle, it is an occurrence entirely fo[illegible], and in no way essential to the production of tubercle itself. I have had as much to do with tubercle as most persons, and I never saw any micrococcus, any *bacillus*. Tubercle is wholly unorganised, in fact, a *caput mortuum*, consisting of the unoxidised carbonaceous waste, not excreted by reason of insufficient congress with the oxygen of the atmosphere. Not only tuberculous matter, but almost any extraneous substance is capable, upon inoculation, of producing tubercle in subjects predisposed, so that it is quite unnecessary to torture animals in order to verify this position. The general spread of tubercular disease does not depend upon inoculation, but on the respiration of prebreathed air. If we only avoid prebreathed air, tubercle and tuberculous disease become impossible. At least a fourth of the human race are reputed to perish tubercle-stricken.—I am, sir, your obedient servant,

HENRY MACCORMAC, M.D.,
Consulting Physician to the Royal Hospital.

[We have inserted Dr. MacCormac's letter, though the *Times* would not, because in such matters the fullest discussion is desirable. Albeit there is one point to which I must take exception. Professor Tyndall never made the mistake attributed to him by Dr. MacCormac. I remember perfectly well his first popular statement of the action of cotton wool in his lecture on dust and disease, and I am sure not a person in the lecture-room supposed for a moment that he meant what Dr. MacCormac implies, viz., that the air was otherwise purified than by the elimination of organic matter. Dr. MacCormac, in his enthusiasm for the theory which he enunciated in his book on the "Breath Rebreathed," seems able to close his eyes to facts which most students of minute life consider demonstrative. When Dr. Koch not only saw with the microscope (what Dr. MacCormac has not seen) the minute *bacilli*, but has succeeded in developing and as it were rearing, generation after generation of *bacilli*, it is rather too much to ask science to reject all belief in these organisms. Nor can we see how the crucial experiments, described so lucidly by Professor Tyndall (KNOWLEDGE, p. 517), can possibly be controverted by any number of experiments showing (what no one doubts) the bad effects of breathing prebreathed air.—ED.]

[Several interesting letters on "Consumption" are unavoidably held over.]

Answers to Correspondents.

**** All communications for the Editor requiring early attention should reach the Office on or before the Saturday preceding the current issue of* Knowledge, *the increasing circulation of which compels us to go to press early in the week.*

Hints to Correspondents.—1. *No questions asking for scientific information can be answered through the post. 2. Letters sent to the Editor for correspondents cannot be forwarded; nor can the names or addresses of correspondents be given in answer to private inquiries. 3. Correspondents should write on one side only of the paper, and put drawings on a separate leaf. 4. Each letter should have a title, and in replying to a letter, reference should be made to its number, the page on which it appears, and its title.*

X. K. You will see that the opponents of Dr. Koch's conclusions use arguments which, if accepted, would render fresh experiments necessary. However, there can be no question you are right in asserting the general principle, that to repeat experiments by which a truth has already been established is simply cruelty. Only doubt can justify the renewal of experiments of the kind.—W. Stringfield. We have posted your letter to "George," and have sent your communication to the publishers. Thanks.—Boyd Moss. We thank you; but the discussion of the general question of vivisection is not suited to our columns. In company with Professor Tyndall's interesting communication about consumption, we printed his incidental allusion to the necessity of certain experiments; but that does not open up a discussion on a subject already overworn. I am sure neither Professor Tyndall nor any true student of science would advocate other than such carefully-arranged experiments as you describe.—Alex. Scott. The paragraph you referred to was borrowed through another source; we named the original source, the "Journal of the Society of Chemical Industry," but we do not know the publishers. The address, "Publishers of the Journal," &c., would, we imagine, be sufficient.—Constant Reader and Recommender. Thanks. The quotation was made by a correspondent, and seemed so suitable that we added it to our set. If incorrect we must remove it, for as corrected it does not seem so suitable: the motto, "Orthodoxy is God's Truth," is open to misconception.—P. W. Clayden. The eclipse to which you refer was that of Sept. 7, 1820, well seen, as a partial eclipse, throughout the British Isles.—W. Dale. Letter received, and shall appear.—C. J. Watson. The theory is quite untenable; everything known about the action of solar heat, as of solar light, indicates radiation, and everything known about the sun indicates that its globe is intensely heated.—Canadensis. It is a mere accident that some articles of mine are inserted as "By the Editor," others by "R. A. Proctor." The "Answers to Correspondents" are not all by the same hand. I write most of them, because no one else would be willing to wade through so much of the correspondence which reaches the office of Knowledge. "Conducted by R. A. Proctor" means "edited," or rather includes it and something more.—Desmond. Enlarging Knowledge, and making the price 6d., might have the effect of increasing the "circulation among the higher and superior educated branches of society," but it would put it beyond the reach of many to whom I wish to be of use. Those who will not take a paper *because* it only costs twopence would hardly help us much. No; our plan was to make Knowledge as low-priced as possible, and to give as much as we possibly could for the money. To that plan we must adhere. To create a circulation, and after bringing our paper into request, to increase the price of it, would not be fair.—W. Gadd. I fear it will be impossible to find space.—C. Carter Braine. Will ask engraver if he can manage it. "Brake" is correct, but some prefer "break."—Jos. Offord. Many thanks. Will use as soon as possible.—Another Hater of Superstition. No fear of a theological controversy; but why should you object more to the term Almighty than to the term Infinite? What *are* natural causes and events, but those operations which, being finite, we can understand, or hope to understand? Outside of them lies the infinite. To speak of infinite time may be idle, seeing that we cannot conceive either; but any one may do so without offence; he may equally speak without offence of Infinite Power. There is nothing necessarily theological in the conception.—Erin go Bragh. Fear Otto of Roses somewhat outside our line.—E. H. B. Stephenson. It has been by noting changes among the stars that the sun's motion in space has been recognised; but the changes are too slight for ordinary observation. Some of them have only been detected after centuries of observation. Tyndall's book on "Dust and Disease" (Longmans) would suit you, I think; but the subject is very wide. Thanks for lines, but fear some would object to them as having rather a goody-goody tone.—A. C. H. If that were so, how could a ship sail close to the wind? Estimating as you do the driving force of the wind, the action of a wind dead abeam would produce no way at all, except leeway, and a wind six points (67½ degrees) from a ship's course would drive her astern. You can only resolve the wind's action as I have done, viz., parallel to the sail and square to it, then this last part must be resolved in direction of ship's course and square to it. The wind thus dealt with must be the relative wind, viz., that which seems to blow when the ship is travelling on her course at whatever rate she may have at the moment. T. P. Garrett. Thanks, but it reaches two members of staff. Subscriber to Knowledge. We can give no information as to either advertisement. No telescopes procurable at the office of Knowledge, or sold directly or indirectly through the agency of publishers or proprietors of the paper. Regret that no correspondent has been able to reply to question about sulphur cast (query 266, Feb. 24). A Churchman. I should imagine the Rev. Dr. Liddon would be a better judge than you can be on that subject. But then, your letter being anonymous, you may answer that as I do not know who you are, I cannot tell whether you are a judge or not. After all, what can it matter to you, being so sure as you are of your opinion, whether others think differently or not? S. C. Gatbeer. The generalisation is bold; certainly so much has not yet been proved.—Cycloid. Certainly, the lowest point is for the moment at rest, the uppermost moving twice as fast as the circle's centre. You ask, why? The answer is, because at the lowest point the advancing motion of the circle as a whole is exactly counterbalanced by the (there) receding motion of the circumference due to rotation. At the uppermost point both these motions are in the same direction. If v be the rate at which the centre advances, and also that at which, were the centre at rest, a point on the circumference would be moving on account of rotation, then the velocity of the lowest point is $v-v$, or 0, while that of the uppermost point is $v+v$, or $2v$. Solarium. The correct angle for the gnomon of a sundial to be erected on Streatham Common would be 51½°. There may be a minute or two of difference between 51° 31′, the latitude of London, and that of Streatham, but it would be impossible to make a gnomon so precisely as to take this into account. The best way to place the style is by the sun, not by the pole star. At true solar noon (which you can get from "Whitaker's Almanack," if you have true clock time, by adding or subtracting the equation of time according to the date), the style must have its shadow in its own plane. Thus, on May 12, see "Whitaker's Almanack," p. 34, the sun is 3m. 52s. before the clock, so that the sun is due south at 3m. 52s. to twelve clock time. The style can then be set with its plane vertical, and due north and south, the slant edge, of course, pointing towards the pole of the heavens, or due north, 52½° above the horizon.—A Constant Reader. We must not trespass on the province of medical men. A surgeon who has seen the formation would know better than correspondents who had not; and we should be afraid of advice being given which might lead to mischief.

[About two pages of "Answers" have been unavoidably held over.]

Our Mathematical Column.

THE LAWS OF PROBABILITY.

By the Editor.

The law enunciated at the close of the last paper enables us to determine the probability that a certain series of results will follow, in a certain definite order, when any definite *trial*, as a tossing, drawing, or the like, is repeated such and such a number of times; but it does not tell us what the probability is that so many results out of the total number will be of one kind, without regard to order. For example, suppose there are three white balls and seven black balls in a bag, and that we draw a ball five times, always at random, and always returning the drawn ball. Then, the chance that the drawings give, first a white ball, then three black balls, then a white ball, is as follows:—

$$\frac{3}{10} \times \frac{7}{10} \times \frac{7}{10} \times \frac{7}{10} \times \frac{3}{10}; \text{ or } \frac{3087}{100000}$$

—the odds in fact are more than 30 to 1 against such a result. But the probability that two drawings out of the five will give a white ball and that three drawings will give a black ball is very different. The odds are against such a result; but they are not nearly so heavy as against the former. This is easily seen; because the particular succession above considered is only one out of several results which would give two white drawings and three black ones. If we consider in how many ways this proportion of white and black drawings may be brought about, we shall be led to recognise the true method of determining the probability of this result.

Call a white drawing w, and a black drawing b. Then the above particular result is represented by the arrangement, $w\,b\,b\,b\,w$. But

algebra tells us that out of two w's and three b's we can make $\frac{1.2.3.4.5}{1.2 \times 1.2.3}$ different permutations. Now, we have seen that the chance of any given one of these occurring is

$$\frac{3}{10} \times \frac{3}{10} \times \frac{7}{10} \times \frac{7}{10} \times \frac{7}{10}; \text{ or, } \frac{3^2.7^3}{10^5}:$$

hence, obviously, the chance that some one or other of these permutations occurring is obtained by multiplying their total number into the probability of the occurrence of one out of that number. This gives, as the required probability, $\frac{1.2.3.4.5}{1.2 \times 1.2.3} \times \frac{3^2.7^3}{10^5}$; which may either be reduced into the form $\frac{4.5}{1.2} \times \frac{3^2.7^3}{10^5}$ (for further reduction), or may be conveniently written, $\frac{\lfloor 5}{\lfloor 2 . \lfloor 3} \cdot \frac{3^2.7^3}{10^5}$; its value is $\frac{3087}{10000}$.

If we notice how this result has been obtained, we readily deduce the following important law:—If, at each of a set of $(n+m)$ trials, there are $(p+q)$ possible results, all equally likely, p being of one kind and q of another, then the probability that n results will be of the former kind, and m of the latter, is

$$\frac{\lfloor n+m}{\lfloor n \lfloor m} \cdot \frac{p^n q^m}{(p+q)^{n+m}}$$

I give a few illustrations of the application of this law, before proceeding to notice how the expressions representing these probabilities are related to certain well-known algebraical theorems.

Suppose we wish to determine the probability that in tossing a coin eight times there will be five heads and three tails. Here p is 1 and q is 1; n is 5 and m is 3. So that the required probability is—

$$\frac{\lfloor 8}{\lfloor 3 \lfloor 5} \cdot \frac{1}{2^8} \text{ that is } \frac{6.7.8}{1.2.3} \cdot \frac{1}{2^8}; \text{ or, } \frac{7}{32}.$$

So that the odds are 25 to 7 against five heads and three tails being tossed.

If we required the odds against five tossings being of one kind and three of the other, without caring whether heads or tails showed oftenest, we must obviously double the above probability, since there must be exactly equal chances for the result five heads and three tails, and for the result three heads and five tails. Thus, we get as the chance that five tossings will be of one kind and three of the other $\frac{7}{16}$, or the odds 9 to 7 against such a result.

Now let us inquire what the chance is that the eight tossings will give four heads and four tails. Our formula gives in this case—

$$\frac{\lfloor 8}{\lfloor 4 \lfloor 4} \cdot \frac{1}{2^8}; \text{ or, } \frac{5.6.7.8}{1.2.3.4} \cdot \frac{1}{2^8}; \text{ or, } \frac{35}{128}.$$

So that the odds are 93 to 35 against such a result. (The reader will readily see why there is no doubling in this case.)

Observe that $\frac{35}{128}$ is greater than $\frac{7}{32}$, but less than $\frac{7}{16}$; so that when a coin is tossed eight times, we are more likely to get four heads and four tails than *either* five heads and three tails, *or* three heads and five tails; but we are more likely to get one of these two last results than the first result.

What, however, is the chance that six heads and two tails will result?

Our formula gives

$$\frac{\lfloor 8}{\lfloor 2 \lfloor 6} \cdot \frac{1}{2^8}; \text{ or, } \frac{7.8}{1.2} \cdot \frac{1}{2^8}; \text{ that is, } \frac{7}{64},$$

the odds are therefore 57 to 7 against such a result.

The chance that six tossings will be of one kind and two of the other is $\frac{7}{32}$.

It is similarly shewn that the chance of seven heads and one tail being tossed is $\frac{1}{32}$; the chance that seven tossings are of one kind and one of another being $\frac{1}{16}$.

The chance that all the tossings give head is $\frac{1}{256}$; the chance that all are of one kind is $\frac{1}{128}$.

We notice, then, that the most probable number of heads is four; and in like manner the most probable number of tails is four; but the most probable assortment of heads and tails is such that there will be five of one kind and three of the other.

It would follow, therefore, that if two persons of equal fortune were to venture half their fortune on each of eight successive tossings, the most likely of all results is that one or other will be just ruined at the end of the series of tossings. But it is equally likely that one or other will be the loser; and it is rather more likely that they will come off quits than that one of them (specified beforehand) will be ruined. This supposes that all the eight tossings are completed before accounts are cleared; and therefore the policy of gambling is somewhat too favourably treated; for clearly two unfavourable tossings to begin with, or three unfavourable out of the four first tossings, although they might be cancelled by favourable throws if the tossing were continued, would yet complete the ruin of a player, if the money ventured had to be handed over to the winner after each several tossing.

Let us next take the following example:—

A die is thrown eight times; what is the chance that ace is thrown twice (exactly)? Here the p of our formula is 1, the q is 5 (since there are five throws other than ace); n is 2 and m is 6. Thus the required chance is by our formula—

$$\frac{\lfloor 8}{\lfloor 2 \lfloor 6} \cdot \frac{5^6}{6^8}; \text{ or } 28 \cdot \frac{5^6}{6^8}$$

the value of which can be easily obtained either by direct calculation or by means of logarithms.

If we examine our formula—

$$\frac{\lfloor n+m}{\lfloor n \lfloor m} \cdot \frac{p^n q^m}{(p+q)^{n+m}}$$

We find that it can be divided into two parts, each readily defined. First, there is the expression $(p+q)^{n+m}$, which obviously corresponds to the total number of possible results when there are $(p+q)$ possible events at each trial, and $(n+m)$ trials. The other portion

$$\frac{\lfloor n+m}{\lfloor n \lfloor m} p^n q^m$$

must, therefore, represent the total number of favourable results, that is, the total number of results fulfilling the required conditions. It is easily seen that this is so. For in fact, if we take any particular case in which n of the results are of the kind which can happen in p ways, and m are of the other kind which can happen in q ways, we see that this particular case can be varied in $p^n q^m$ ways. For instance, reverting to our illustrative case, the particular result $w\,b\,b\,b\,w$ may be varied by having any one of the three white balls to give the first w, by having any one of the seven black balls for the first b, any one of the same set for the next b, and so on; giving in all 3 times, 7 times, 7 times, 7 times, 3 possible variations in which the sequence is $w\,b\,b\,b\,w$—that is, $3^2\,7^3$ such variations. And the number of possible sequences of $n+m$ results, of which n are of one kind and m of another, is, by a well-known rule,

$$\frac{\lfloor n+m}{\lfloor n \lfloor m.}$$

Hence the total number of favourable results is obtained by multiplying these numbers together, or is,

$$\frac{\lfloor n+m}{\lfloor n \lfloor m} \cdot p^n q^n.$$

This expression is the term involving $p^n q^m$ in the expansion of $(p+q)$ to the power $(n+m)$.

So that our law may be thus expressed. If there are $n+m$ trials, at each of which some one of $p+q$ events, all equally likely, must occur, p of these events being of one kind and q of another; then the chance that n events will be the former kind and m of the latter, is represented by the fraction of which the numerator is the term involving $p^n q^m$ in the expansion of $(p+q)$ to the power $(n+m)$, the denominator being the complete expansion.

This is the chance that there will be *exactly* n results of the former kind. The chance that there will be *at least* n results of the former kind is obviously obtained by adding together for the numerator all the terms of the expansion from the first down to the term involving $p^n q^m$ (both inclusive), the denominator being, as in the former case, the complete expansion.

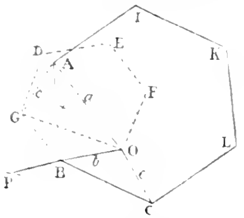
* The symbol $\lfloor$ implies that all the whole numbers, from one up to the number indicated within the symbol, are to be multiplied together.

MATHEMATICAL PROBLEM.

[41]—Show that a proper fraction, which in its lowest terms takes the form $\frac{a}{b \cdot 2^m \cdot 5^n}$ (a and b being prime to each other and to 10), when converted into a decimal, recurs from, but not before, the $(m+1)^{th}$ or $(n+1)^{th}$ digit after the decimal point, according as $m >$ or $< n$.—Gradatim.

["Gradatim" supplies a solution of the above problem, turning on the lemma that if a is prime to b, the fraction $\frac{a}{b}$ = a decimal recurring from first digit after point. I would submit that writing the fraction in the form $\frac{a}{b} \cdot \frac{5^{m-n}}{10^m}$ or $\frac{a}{b} \cdot \frac{2^{n-m}}{10^n}$ according as $m >$ or $< n$, the above result follows at once from this lemma.—E.]

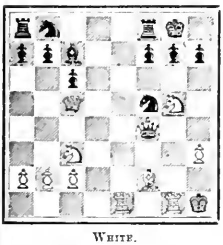
[42]—Given the length of three lines a, b, and c, drawn from any point within a regular polygon of n sides, to any three of its consecutive corners, A, B, and C. Required a geometrical determination of the polygon, granting that the polygon of n sides can be constructed when one of its sides is given.—Y.



Our Chess Column.

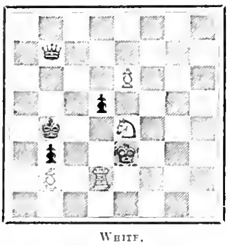
Endings from actual games contested by Leonard P. Rees.

Position No. 40.
Black.



White.
White to play and win.

Position No. 41.
Black.



White.
White to play and win.

HIGHGATE CHESS CLUB.

President — Professor Tomlinson.

Consultation Game, played March 6, 1882.

Kings' Gambit.

White.	Black.	White.	Black.
1. P to K4	P to K4	11. P to K6	P takes P (*e*)
2. P to KB4	P takes P	12. B takes P(ch)	B takes B
3. Kt to KB3	P to KKt4	13. Q takes B(ch)	K to Rsq
4. B to B4	B to Kt2	14. Kt to QR3	Q to Q4
5. Castles	P to Q3	15. Q to KKt4	QKt to K4
6. P to QB3 (*a*)	P to KR3	16. Kt takes Kt	Q takes Kt
7. P to Q4	Kt to K2 (*b*)	17. B to Q2	QR to Qsq
8. Q to QKt3 (*c*)	Castles	18. QR to Ksq (*f*)	Q to QB4(ch)
9. P to K5 (*d*)	P takes P	19. Resigns.	
10. P takes P	QKt to B3		

NOTES.

(*a*) P to Q4 deserves a slight preference; it enables White to choose different lines of play, in addition to his having the option of arriving at the position in the text by following with P to B3. The attack obtained in this variation of the King's Gambit is very indifferent.

(*b*) His best.

(*c*) Sounder than the usual move of P to Kt3, as Black, by replying to the latter move with P to Kt5, will obtain a good game.

(*d*) Here we think P to Kt3 might have been played with more safety than before; there is nothing to be gained by P to K5, which weakens White's centre.

(*e*) This is good enough, but Black might also have played P to B4 in order to be able to win; the P on K6 play would, however, have become more difficult.

(*f*) This was an oversight; he now loses the Bishop. He might have played B to Ksq with a view of playing B to B2 and getting his Rooks into play, and also bringing his Queen's Knight into active operations. Black had an extra Pawn, but his King's side was exposed, and White still had some chance of retrieving his fortunes.

The following bright game illustrates the attack obtained by 10. KKt to Kt5 in the Giuoco Piano:—

White. Leonard P. Rees.	Black.	White. L. P. R.	Black.
1. P to K4	P to K4	12. Q to R6	Q takes P
2. Kt to KB3	Kt to QB3	13. R to Qsq	Q to K4
3. B to B4	B to B4	14. P to B4 (*e*)	Q takes BP (*f*)
4. P to B3	Kt to B3	15. Q to Kt7	Q to K6(ch)
5. P to Q4	P takes P	16. K to Bsq	Q to B5(ch)
6. P takes P	B to Kt5(ch)	17. Kt to B3	Q to B4 (*g*)
7. B to Q2	B takes B	18. Q takes R(ch)	K to K2
8. QKt takes B	Kt takes KP	19. K to B2	P to KR4 (*h*)
9. Kt takes Kt (*a*)	P to Q4	20. Kt to B6 (*i*)	Q takes Kt
10. KKt to Kt5 (*b*)	P takes B (*c*)	21. KR to Ksq(ch)	B to K3
11. Q to R5	P to KKt3(*d*)	22. R to Q7(ch)	Resigns

NOTES.

(*a*) As we have shown in our analysis of the Giuoco piano, p. 442, White can continue with 9. P to Q5, obtaining thereby a very fair game.

(*b*) In the same analysis we characterised the move as an attacking style; it may become very dangerous, but, with correct defence, it will prove less effective.

(*c*) This is exactly what nine players out of ten will do, but we demur to this move. White will obtain a strong attack by Q to R5. Black can prevent this by just delaying the capture of the piece for one move, and playing instead 10. B to B4. It is obvious that the key move of White's attack, Q to R5 would be bad now, as Black would reply with B to Kt3. Q to K2 would be met by Black's castling. 10. B to B4 destroys White's attack entirely, and, in our opinion, even gives Black a superiority; for supposing now B takes P, Q takes B, White's Queen's Pawn must eventually fall.

(*d*) Q to K2 would be met by White Castling, with a good game.

(*e*) An attacking move; 14. Castles would also have been good play.

(*f*) Black's position is very precarious; 14. Q takes KtP would have prevented for a time the entry of the White Queen on Kt7. 14. Q to R5(ch) would have been met by 15. Kt to QB3, and, if Black then proceeded with 15. B to Q2, with the idea of Castling, White would play 16. Kt takes BP.

(*g*) White has played very well. Black is now compelled to give up his Rook, as otherwise he would lose his Queen, *i.e.*, 17. R to Bsq, 18. Kt to B6(ch), 18. K to K2, 19. Kt to Q5(ch).

(*h*) R to Ktsq was better, as he could then play B to K3.

(*i*) This wins the Queen; KR to Ksq, however, looked stronger.

ANSWERS TO CORRESPONDENTS.

*** *Please address Chess-Editor.*

Leonard P. Rees.—Best thanks for Endings, which were ingeniously played. Solutions of Nos. 36, 37, and 38 correct.

G. Licence.—Solutions correct. Problems received will be examined. Thanks for good wishes.

H. A. N.—Problem received with thanks.

Brenton.—Solution of 35, 36, and 38 correct.

A. McD.—Solution of 38 incorrect.

J. B. B.—Solutions of Nos. 35, 36, and 37 correct.

Correct Solution of No. 38 received from G. W. Edward Wilson, Fusee, Moleque.

Fusee.—You are quite correct in your explanation.

H. J. Barker.—W. W. Morgan, 23, Great Queen-street, sells pocket chess-boards. It is quite "straightforward" to draw by perpetual check; in some cases even highly creditable.

Correct solution of Problem No. 39 received from Moleque, Fusee, Alfred B. Palmer, Soc, J. Wrigley, J. P., H. A. L. S., H. A. N.

Solutions of Nos. 38 and 39 also received from J. Griffith, J. B., of Bosford.

J. C. Royle & J. Griffith.

Our Whist Column.

By "Five of Clubs."

A GAME FOR STUDY.

THE HANDS.

A.
Clubs—K, 10, 9, 8, 7, 6, 2.
Spades—K.
Hearts—K, 4.
Diamonds—K, Kn, 9.

Y.
Clubs—Q, 4, 3.
Spades—Kn, 8, 5.
Hearts—A, 5, 2.
Diamonds—A, Q, 10, 2.

B

Y — Z Dealer.

Trump Card, Club Five

A

B.
Clubs—A, Kn.
Spades—Q, 9.
Hearts—Kn, 10, 9, 8, 7.
Diamonds—7, 6, 4, 3.

Z.
Clubs—5.
Spades—A, 10, 7, 6, 4, 3, 2.
Hearts—Q, 6, 3.
Diamonds—8, 5.

Score.—*A B*, 3; *Y Z*, 4.

Players are invited by a correspondent to test these hands (which will be familiar to readers of Cavendish) by playing them in the usual way, and report through our columns—with the original lead in each case—the results of their game.

Solutions of Problem IV.—H. L. L., K. C., Corrigan, and others correct. We propose to publish the solution next week.

A Two-Suit Hand.

A correspondent, W. H. G., points out correctly that a two-suit hand, 7 of one suit and 6 of another, may be formed in twice as many ways as we have indicated in our discussion of such hands at page 561. Our reasoning assumes that each set of six from one suit may be combined with each set of seven from another, or *vice versâ*, whereas for *or* we should read *and*, thus of course doubling the actual number of such combinations. Thus, instead of the odds being 53,911 to 1 against such a combination outside of trumps, we should have obtained 26,955 to 1; and instead of 35,940 to 1 for such a combination in any suit, we should have obtained 17,970 to 1. W. H. G. suggests also, but with less confidence, that instead of considering that there are 51 cards out of which a combination of the former kind may be made, we should have taken the whole pack. Let us consider this point. Any player but the dealer is to have thirteen cards dealt to him, and the question is, what is the chance that these thirteen cards will consist of seven of one suit, six of another, neither suit being that to which a certain card in the dealer's hand, the last he deals himself, belongs? Now if we consider these conditions (more carefully than I did in replying at p. 561 to my correspondent's query), we shall see that it is not the particular card which the dealer turns up last which should be excluded, but one suit, which has been already done. In fact, the question really is this, What is the chance that a given set of thirteen cards taken at random will be six of one suit, seven of another suit, out of three suits? The chance of this is just half the chance that the thirteen cards will be six of one suit, seven of another suit, out of all four suits; hence the odds against are rather more than 35,940 to 1, instead of 26,955 to 1.—Ed.]

J. Montague.—It is the same problem. Mr. Clay's treatise appeared in 1861; but, in the latest edition, the wording is unaltered. Doubtless, had he been alive, he would not have allowed the words "a few months back" to stand. Five of Clubs.

"WHIST FOR BEGINNERS."*

It is somewhat singular that when a very small and elementary book is to be written, the writer seems to think the occasion one for being discursive. In this little book, with only twenty-seven very small pages, there are more wasted words and sentences than in Cavendish or Pole. The mistake is made, in fact, of supposing that the best way to explain matters for beginners is to wander round and round the point as long as possible. For instance, where, except in a ten-volume treatise on whist, ought the writer to expatiate like this over the suits:

"When the beginner has sorted his cards into the four suits, he will greatly assist his memory if he will try to consider his hand as containing only four units, each suit being treated as an unit of more or less strength. It is difficult to remember all the thirteen cards in detail; but if each suit is treated as an unit of strength, the memory has to deal primarily with only four things instead of thirteen. It is something like treating each suit as a separate regiment; if you call your trump suit artillery, and your long suit, *i.e.*, the suit in which you hold most cards, cavalry, the other two suits are your infantry. This metaphor may seem strained, but whist is a battle, and you must at once begin to attack or to defend yourself." More than a page taken up in saying what might be said in two lines.

If, with all this palaver, the rules for correct whist play in all ordinary cases were properly given, we might still be content. But they are not. Thus, the only rule given for a suit headed by Ace King is this—if you have Ace and King and three small cards in a suit, you should lead the King first; and similarly with King Queen; as though with Ace, King, and two small ones, or King, Queen, and two small ones, the lead should be different. Here again is a general rule which is enough to make Clay rise from the grave:—"As the game progresses, you may find yourself obliged to lead from a suit in which you hold only three cards. It is the safest plan to lead the highest of the three cards, as it may strengthen your partner:" the truth being that in a great number of cases your only chance is to retain the best in a three-card suit, as a defence against your adversaries, while in every case in which you have nothing to guide you, the odds are 2 to 1 against your partner being stronger than either adversary in the suit in which you are weak.

Again, instead of the general rule that in returning your partner's lead, you should return the best of two cards left, and lowest of three cards left, Mr. Buckland says, "When you take the trick in the suit led by your partner, you should return," &c., implying not only that when an adversary has taken the first trick, the rule does not hold, but also that you ought always to return the lead when you take the trick in your partner's suit.

The rule given for the discard is also only true when trumps are not declared against you; when they are, the discard should be from longest suit.

A little book like this might contain a great deal of useful and correct information. This little book does not.

* "Whist for Beginners." By C. T. Buckland, F.R.Z.S. (London: W. H. Allen & Co.)

NOTICES.

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CONTENTS OF No. 29.

SCIENCE AT THE ROYAL ACADEMY.

ANATOMY IN THE SCULPTURE GALLERIES.

A CORRECT knowledge of anatomy is so much more important in sculpture than in painting, though important in both, that in considering this part of our subject we shall do well to turn for a while into the sculpture galleries. As we pass quickly through the other rooms, we note the absurdly long legs in Mr. Pettie's picture of Eugene Aram ("He talked with him of Cain"), No. 18, Room I.; the aged hands of Mr. Wirgman's girl in No. 19 ("I cannot mind my wheel, mother"); the incorrectly drawn right foot in Mr. Cameron's otherwise pretty picture "Evening," No. 48; and the most monstrous foot in Mr. Marks' "A Fugitive Thought." Mr. Pengelley's left hand (Portrait by Cope, No. 79) is manifestly deformed, if the painter has correctly represented it, which we doubt. It would be absurd to criticise the anatomical development of Mr. Melville's "Sower" (No. 98, Room II.), for the simple reason that he has none, nor has he any respect for the law of gravity. In the same room we note, still *en passant*, the singularly unfinished condition of Mr. Starr's portrait of Mr. George Wilkinson, M.A. In Room III., "Bad News," No. 222, a picture of the melodramatic school, by Mr. Marcus Stone, suggests that the trooper who has brought the bad news, and who, if the perspective is correct, must be about ten inches shorter than his mistress, requires a course of athletic exercise to give him a respectable development of chest and shoulders. Ella's hips, in Mr. John Scott's pleasing picture "The Wild Swans" (No. 270), are in danger of dislocation; and in No. 384, Mr. Barwell's "Sweetness and Light," Gallery IV., there are two most hideous, though perhaps correctly-copied, feet, the only excuse for which is that they turn the attention for the moment from the absolutely impossible lilac tint of the riverside scene. Mr. Barrable's "Little Patience," No. 396, in the same room, must be a little opera dancer, if she can keep her feet patiently in the attitude represented. To the artistic eye, the attitudes of ballet dancers are not beautiful; but it is easier to draw feet in the pointed position such folk affect than in one of the many positions which the feet naturally assume. In Gallery V. we note, in passing, that the anatomical development of the late Edwin Christy (No. 133), as presented by Mr. Sidley, would hardly have sufficed for the effective wielding of the sword, some 5 ft. in length, which the artist has bestowed on the hussar. The left arm of the young girl in No. 500, "To be Left till Called For," by Mr. Jerry Barratt, is indescribably incorrect in an anatomical sense: that is to say, it is impossible to tell what the artist meant to give that arm in the way of muscles and tendons—certainly not such as are known to the anatomist—but the crabbed hand may have really belonged to the model. We note in the same gallery a serious fault in an otherwise charming little painting, Mr. Edwin Douglas's "Place of Safety." Such a horse, one with which a dog could safely play, and on whose back a cat could find "a place of safety," never had such an eye. Even without these proofs of a trustworthy nature, the wicked eye would be inconsistent with the rest of the head. Every one acquainted with the ways of horses knows what such an eye means, and how the ears follow suit. In the next gallery (No. VI.) we observe that the bigger dog in Mr. Strutt's "Extremes Meet," is meant to be one of the biggest of the big, but is made, by incorrect drawing, decidedly undersized for his breed.

However, we must hasten to the sculpture galleries if we are to have time to discuss the anatomical defects and excellencies of the works there exhibited.

The first feature which always strikes any one familiar with antique art, in studying the works of modern sculptors, is their unfinished condition. Compare the nude arm or leg of a Greek female statue of the highest type with the nude arm of the best modern statues, and the difference to which we refer will be seen at once. (We mention female statues because there is not in the female figure that obvious muscular development which even the inexperienced can recognise; though in male as in female statues the difference we are considering exists.) By the careful finish of their surface, the older statues indicated not merely the surface contour of the flesh, but the actual form and proportions of the muscles. In modern statues we usually have to be content with a general intimation of the existence of the chief muscles, the delicate gradations of surface which indicate the presence and form of minor muscles being generally neglected altogether. Sometimes even the chief muscles are incorrectly represented. Take for instance Pradier's "Toilet of Atalanta" (sometimes absurdly called the *Venus accroupie*, or Crouching Venus): in this statue, the head of which alone is worthy of Pradier's reputation, the arms have no muscles at all, the muscles of the thigh and calf are not merely incompletely, but incorrectly, rendered, and the flat and long left foot would have been regarded by Phidias or Praxiteles as altogether unfinished,—probably as not worth finishing. Houdon's "Diana" (bronze), compared with a good ancient statue, is like a sketch in crayons compared with a finished painting.

In the sculpture gallery of the Royal Academy we find this year many pleasing works, and many which are suggestive of exquisite beauty, but none (at least of the higher class of statuary) which can be regarded as finished; none, at any rate, bearing evidence of the loving care which the sculptors of Greece bestowed on their best statues. It may be that the fault lies in the want of such models as the Greek sculptors had. Women may be as beautiful in face now as the Greek women of old, but in form they naturally cannot be; for, apart from the use of stays, women have scarce any exercise by which beauty of form can be

developed, or retained where it already exists. As Pradier's "Atalanta," in her sloping shoulders and pinched waist, bears evidence of Parisian tastes (or of the only type of female beauty which can exist where Parisian fashions prevail), so Mr. Thornycroft's "Artemis," No. 1,644, Mr. Ball's "Lancashire Witch," No. 1,571, and other works in the sculpture gallery at the Royal Academy, bear evidence of the imperfect muscular development of the women of our time. The right arm of "Artemis," for instance, is incorrectly shaped, both upper arm and fore arm, though very likely he correctly represented the arms of his model. This, however, is a fine sculpture, though the idea is somewhat worn. In the "Lancashire Witch," a really charming work, the proportions are more correct, but the limbs are unfinished, or rather the limbs *copied* would have been regarded by an ancient sculptor as imperfect and flaccid. On the other hand, "My Dainty Ariel," by Mr. Armstead, R.A. (No. 1,680), is absolutely and outrageously incorrect in proportions, as well artistically as anatomically. The short fore-arms are out of all proportion to the monstrous hands; the attitude is hideous, bringing out, and, as it were, emphasising, the boniness of the knees; the face is frisky, perhaps—certainly not dainty. Of the wings, as we know nothing of the anatomical arrangements by which wings stuck upon human shoulders could be made to work, we say nothing; but the statue, as a whole, is quite unworthy of Mr. Armstead's repute.

Speaking of winged figures, we are led to notice the representations of "Jacob wrestling with the Angel." Giving priority to the fair sex, we take first No. 1,578, by Emmeline Halse. Jacob is supposed to be at that stage of the encounter where he remarks, "I will not let thee go, except thou bless me." He is certainly not wrestling with the angel. He is holding the angel up from the ground, and his opponent is manifestly conscious that he is in no danger of being thrown, for he is curling up his toes like a child at play. Properly to represent such an encounter, the artist should pass hours in watching *bonâ-fide* encounters between good wrestlers. We will undertake to say that she would never in a real encounter see the toes of a wrestler who has been caught up by a strong opponent, twiddling in the air as her angel's toes are. The actual position taken up by the toes when a wrestler is thus placed (we suppose the opponents, as usual in the North, to be in stockinged feet) is quite characteristic, all the toes closing in towards the ball of the foot; only when the body is actually swayed round for the throw is this position of the toes changed. The toes are not turned upwards when great exertion is made in which the lower limbs take part, any more than the fingers are turned backwards towards the wrist when the muscles of the arm are energetically exerted.

Mr. Robert T. Fallon's treatment of the same subject, No. 1,542, is still less consistent with anatomical facts. The angel (with a face of the type of a Yankee pedlar's, and with an expression disagreeably suggestive of Mr. Julian Hawthorne's impossible pedlar in "Fortune's Fool"), is carefully holding up Jacob with one hand while endeavouring to thrust him down with the other. This is not usual in such encounters. We would remind Mr. Fallon, also, that although certain muscles are called into active exercise in an energetic wrestle, they only become exceptionally developed in persons who give much of their time to such encounters. To give to sculptured figures the peculiar development of men whose chief business is wrestling is to imply that the persons represented were trained wrestlers. There is nothing in the Hebrew record to suggest that either Jacob or the angel belonged to this category. Certainly, Mr. Fallon goes far to correct any erroneous impression in this respect by indicating Jacob's manifest want of skill, and by assigning to the angel the unusual task of holding up his opponent and pushing him down at the same time. But it remains the case that some of the muscles, both of the patriarch's body and of the angel's, have a relative development, such as we only see in veteran wrestlers.

THE THREE COLD DAYS OF MAY.

By the Editor.

It is a singular, and as yet unexplained, circumstance, that usually in the second week of May two or three cold days occur. And although the fall of temperature is not quite so strongly marked as that which occurs between the 10th and 14th of April—the "borrowing days,"—yet the cold days of May are quite sufficiently marked to be unmistakable. The mean annual curve of temperature derived from half-a-century's observations at Greenwich shows a well-defined depression near the end of the first third of the month of May; and a peculiarity striking enough in its occasional manifestations to attract popular attention, and sustained enough to show through all the variations which have occurred in the weather during half a century, must be regarded as real, not accidental. Its nature is shown in the temperature curve in p. 277 of the second series of my "Light Science," where the peculiarity is far from being exaggerated. Indeed, the curve is softened off on account of the method adopted for tracing it. We see in the temperature for Greenwich a wave-like rise from January to July, the curve sinking then to January again; but the ascending curve is affected by three well-marked depressions, one in February, one in April, and one in May, while, strangely enough, three similar irregularities affect the descending curve in the parts for November and December. It is, indeed, now an established meteorological fact, not for Great Britain only, but for Europe, that during the first fortnight in May the average temperature is considerably below that which might be expected from the increasing elevation of the sun and duration of daylight. It is not altogether true, as I have seen stated in a Continental journal of science, that a week of cold occurs with extreme regularity in the first half of May. It requires but a brief search among meteorological records to find instances of warm first fortnights in May. If we take up any weather summary for a few successive years, we find abundant evidence of the irregularity with which "the cold week of May" makes its appearance. For example, in the summary of the weather given by Gilbert White in his "Natural History of Selborne," we find such records as follows: In 1771 frosty weather to the end of the third week in April, followed by spring weather and rain to the end of the first fortnight in May, and then dry warm weather to the end of June; in 1772 the first fortnight of May was dry, with cold piercing winds; in 1773, throughout May and June, "warm showers"; in 1774 no marked peculiarities; in 1775 warm weather throughout May; and in 1776 cold weather throughout the month; the last half of April, 1778, "snow and ice," followed by rainy weather to June 11; thence warm Mays till 1782, when the first week of the month was cold and dark; in 1783 there was thick ice on May 5; in 1784 cold dry weather during the first twelve days of May; in 1785 mild weather during the first seventeen days, and then cold weather to the end of the month; on May 1 and 2, 1786, "thick ice"; in 1787 fine bright weather to the 22nd, then warm, but on June 7 "ice as thick as a crown piece;" in 1778 a warm dry May; in 1789 a warm moist month; and lastly, whereas May in

1790 and 1791 was a warm month throughout, May in 1792 was cold and bleak. Certainly there is no evidence here of extreme regularity.

I have already pointed out (see last number) that the praises bestowed by the poets of the sixteenth and seventeenth centuries upon the month of May do not relate to the thirty-one days forming the May of our present year, but to those which now fall between May 11 and June 11. The May-day of those times fell, strangely enough, at the very coldest part of what may be called the average cold week of May, but the month of May, as a whole, was then much warmer on the average than our present May, and well deserved Dryden's warm description:—

> For thee, sweet month, the groves green liv'ries wear,
> If not the first the fairest of the year:
> For thee the Graces lead the dancing hours,
> And Nature's ready pencil paints the flow'rs.
> The sprightly May commands our youth to keep
> The vigils of her night, and breaks their sleep;
> Each gentle breast with kindly warmth she moves,
> Inspires new flames, revives extinguished loves.

It is only when the average temperatures of the first fourteen days of May are considered, that we find the now prevalent belief in a "cold week in May" fully justified. The curve of temperature for the year from the observations of the last half century, shows, as already mentioned, a decided depression at the part corresponding to the second week in May, though it is to be noticed that it slips down quite as decidedly at the part corresponding to the second week in April. It appears to me that when we combine the ascertained fact that there is on the average a fall of temperature at this part of May, with the equally certain fact that there is no regularity in the recurrence of the cold week, we must regard as extremely improbable the theory which attributes the peculiarity to a cosmical cause. This theory was thus placidly presented some time ago by M. de Fonvielle as a known truth:—

"The chilliness is due to the fact that the earth passes behind a ring of asteroids, which absorb a portion of the sun's warmth, due to us while it remains above the horizon. The temperature will not resume its ascensional movement until the annual rotation shall have carried our sphere from the shadow of the multitude of the small planets which is always projected on the same point of our orbit."

Next week, I shall point out a few objections to this theory.

Poisonous Crayons.—A little girl, æt. two-and-a-half years, recently died at Brockley from the effects produced in part by sucking poisonous crayons. At the inquest held on the body, the tradesman from whom the things had been purchased disclaimed all knowledge of their injurious properties, and said that he sold them in considerable quantities in penny boxes. Post-mortem examination, however, revealed that the brain and stomach alone of all the organs were in an unhealthy condition. The stomach was much inflamed, and perforations of its coats occurred in two places, while the left side of the brain was distended with fluid. There was evidence that the child had sustained a fall, and to this it was sought in part to attribute the death; but information concerning the accident was incomplete and unsatisfactory, although the jury, by their verdict, credited it in part with the fatal result. Analysis of the crayons conclusively proved that they all contained poisonous material, and there can be little doubt they were chiefly to blame for the death. One of the crayons, a pink one, contained more than fifty per cent. of its weight of white lead, and as the unfortunate little victim lingered for three weeks in much suffering, it ought to be possible to ascertain how far this substance influenced her condition. The newspaper reports give very insufficient details of the case, of which, however, Dr. Kavanagh, the medical attendant, may possibly provide more comprehensive notes. The case is an instructive one, as showing the need for sweeping measures of reform in connection with the indiscriminate sale of poisonous materials of all sorts by general shopkeepers; and in this way it may excite useful discussion.—*Medical Press.*

CRYSTALS.

By William Jago, F.C.S., Assoc. Inst. Chem.

No. II.

In the last paper on this subject directions were given for the preparation of crystals of bismuth and sulphur, in both cases by the solidification of the fused substance. Before leaving the crystallisation of metals, reference should be made to those experiments in which metals are displaced from a solution of their salts by some other element. The well known "lead tree" is a type of such changes; its formation depends on the fact that zinc is a more active element, chemically, than lead; hence, if zinc be introduced into a solution of a compound of lead with an acid, the lead is "displaced," and its former position usurped by the zinc. The lead is deposited in the metallic state, and under favourable conditions assumes a beautiful crystalline form. The experiment of making a lead tree may be performed with a *minimum* of apparatus and experience of chemical manipulation. An ordinary pickle bottle, or other vessel of clear glass and similar shape being obtained, fill it with a solution of lead acetate (sugar of lead). About an ounce of the acetate will be sufficient for a bottle of the size mentioned; if dissolved in spring water, a slight sediment will be formed—this, however, if allowed to subside, will not interfere with the experiment. Distilled or even rain-water is preferable for making chemical solutions. Next a fragment of *clean* zinc, about the size of a small walnut, must be procured, the more irregular the better; suspend this by a piece of string, the end of which passes through a hole bored in the cork of the bottle containing the acetate. Put the zinc in the bottle, cork it up, and so arrange the length of the string that the zinc is just beneath the neck; fasten it in this position, and set the whole arrangement where it will not be disturbed. In a short time crystals of lead will be seen to deposit themselves on the zinc, and soon it will be covered with a tree-like growth of crystals. If left perfectly still, it remains a long time before the mass drops off. With this and other similar experiments, one half the pleasure consists in watching and studying the crystal growth for one's self.* Silver, which is in many respects a metal closely allied to lead, may also be made the subject of interesting experiments on crystallisation; the so-called *Arbor Dianæ* is produced by placing a globule of mercury in a solution of nitrate of silver; a growth ensues of long thin crystals of an amalgam of silver; these, in addition to their beauty of shape, possess that magnificent lustre which causes mercury and silver to be almost unrivalled among the metals.

Those who possess a microscope will find a few prepared specimens of crystals a valuable addition to their stock of slides. Not only are they of great interest, but as an introduction to microscopic analysis and microscopic study of rocks, the systematic student will find them worthy of special study. It has been previously stated that the crystalline form of many substances is one of their most characteristic properties; and as in the detection of poison and other important cases there is often a trace merely of the substance to be obtained, a microscopic examination is of great importance; it has, too, this further merit, that the substance is afterwards available for other chemical tests.

The preparation of such slides is very simple. In the first place the glass slips must be perfectly clean and free from grease; it is well to wash them in a solution of soda,

* It should be stated that lead acetate is a poisonous salt. In all cases chemicals should be kept clearly labelled and locked up.

rinse with rain or distilled water, and then wipe dry with a clean linen cloth. The substances that may be selected for study are legion; those figured are very suitable for a first attempt; make solutions by putting a pinch of common salt, potassium nitrate (saltpetre), oxalic acid and potassium dichromate in separate clean test-tubes, and add to each a tea-spoonful of water, they will dissolve rapidly to clear solutions. Take a drop out of the common salt test-tube on the end of a glass rod, and place it on a clean slide, spread the drop out with the rod in as thin a layer as possible; warm the slide very gently over a lamp until the salt begins to crystallize round the edge of the drop, then place it under the microscope and watch the progress of crystallisation. Little cubes of salt will be seen to form, and ultimately the field will appear as shown in Fig. 1.

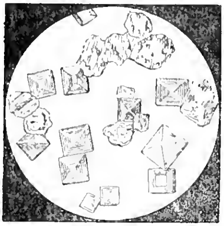


Fig. 1.—Common Salt.

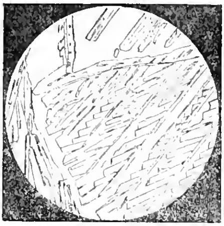


Fig. 2.—Potassium Nitrate.

Precisely the same experiments being made with the other solutions, the shapes of the respective crystals are shown in the accompanying figures. Potassium nitrate differs remarkably from the salt; instead of the little cubes, we have the crystals arranged in long parallel feathers. The oxalic acid, again, shows forms differing from the other two; from a centre the crystals radiate out in every direction. Of the four specimens, however, the potassium dichromate is the most beautiful; the crystals, instead of being colourless, are of a deep amber hue, while, in mode of arrangement, they resemble a fern group rather than mere inanimate matter. The figures must be looked on as

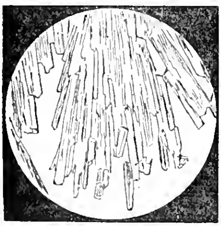


Fig. 3.—Oxalic Acid.

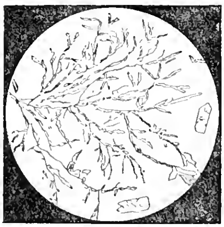


Fig. 4.—Potassium Dichromate.

giving some idea only of what is actually seen. The leading outlines have been drawn, but to copy the delicate tracery of the finer crystals is impossible. In the case of the potassium dichromate in particular, some parts of the field defy all attempts at even affording a conception of their exquisite beauty. But were even all this possible, there is yet the greater charm remaining to the actual worker, and that is to see the growth proceeding. The specimen being so placed that the edge of the crystals already formed is just within the field, the main lines shown are first rapidly filled in, and then the smaller branches dart out until, the water having evaporated, the whole of the salt has regained the solid state.

PHOTOGRAPHY FOR AMATEURS.

By A. Brothers, F.R.A.S.

PART VII.

THERE are some advantages in trimming the prints before toning. One is, that the black edge would take up gold wastefully, and as the paper contains silver, the waste edges should not be destroyed. The saving, where the quantity of paper used is large, amounts to an important item in the course of a year. A third advantage is, that the prints may be mounted at once. Prints on albumenised paper have a habit of curling, and it is, therefore, less troublesome to mount them while damp. The medium for mounting may be gelatine or starch. The starch may be of about the same consistence as is used for stiffening linen, and should always be used fresh. The only disadvantage in the use of starch is, that if it be required at any time to remove the print from the mount, there is some risk of spoiling the print; but it may be done by steeping in hot water, and gradually stripping or rubbing the paper or cardboard from the back of the print. Prints mounted with gelatine are readily detached from the mount when placed in hot water. The mountant should never be used if at all acid.

When dry, the prints should be pressed with a hot flat-iron, a piece of smooth paper being used between the print and iron. But a bett[illegible] face will be obtained by passing the prints through a suitable rolling-press. A solution of indiarubber is sometimes used for mounting prints, and it has the advantage of not cockling the paper, but the prints are liable to peel off after a time. There is always a difficulty in mounting prints on paper, or even cardboard, unless very thick, on account of the cockling caused by the contraction of the print in drying. If the prints are larger than 8 in. by 6 in., they have a much more finished appearance if mounted on plate-paper with an india tint; but this cannot be done by an amateur, and the prints should be sent to a professional mounter, the cost being very little more than if done at home on cardboard, and the superior finish is well worth the extra cost.

Up to this point I have endeavoured to describe a process by which photographs may be obtained, and if the directions are carefully followed, a very little experience will enable the student to become expert. The chief difference between the amateur and the professional photographer is that the latter has more practice, but there is no reason why the work of the amateur should not equal that of the professional. It may happen that our pupil has no friend to whom he can apply to help him over difficulties. By a recent change introduced into the management of the correspondence columns of KNOWLEDGE, we offer to take the place of the friend, and we hope during the present season to welcome many workers in one of the most fascinating of arts.

Before closing this part of our subject, a few general remarks may be useful.

In the early days of paper prints, the skies of landscapes were almost always left white, but the effect was most inartistic. It is now rare to see a landscape without appropriate clouds, often taken at the same time as the rest of the picture. This, however, is not always possible, and for one reason, amongst others, that the sky may be quite clear when our view was taken. It is quite easy to make

photographs of clouds, and no opportunity should be lost to obtain negatives of various size, or they may be taken on small plates, and afterwards enlarged. These negatives are to be used for *printing in* the skies. Always be careful to select a cloud negative with the light falling in the same direction as in the picture. Place the print on a flat board or sheet of glass, and then put over it the cloud negative, carefully arranging it so that the masses of cloud fall suitably. Cover the lower part of the print with a piece of cardboard, so that no light can injure it; but the top part of the cardboard must be bent upwards, permitting the light to graduate towards the horizon of the print. Now expose to diffuse daylight to print the clouds. This will very soon be done, as the printing must not be carried too far, or the results will be heavy and unpleasing.

Success will depend very much on care and cleanliness in all the various operations described. The dish used for nitrate of silver solution should not be used for any other purpose. All bottles should be carefully labelled, and always put in proper places. Glass measures should always be washed out after use. The bottles containing collodion, developing, fixing, and other solutions, should always be kept in the same places while in use, and preferably should be of different shapes, so as to avoid mistakes in the feeble light of the darkened room. The glass vessel, or bath, containing nitrate of silver solution, should have a wooden case made to slope at a suitable angle. This case is to protect the collodionised plate from light caused by the chance opening of a door, and if the top be protected by a suitable cover, the plate may be safely left with a flood of white light in the room while the operator proceeds with other work. The dark slide of the camera should be wiped dry after using each plate. Thick blotting-paper may be used for this purpose. Nitrate of silver stains on linen are troublesome to remove, therefore keep certain cloths for certain purposes. To remove accidental stains, a strong solution of cyanide of potassium should be used, and the parts should then be washed with soap and water. Stains on the hands may be removed by rubbing them with solution of iodine and then with cyanide of potassium. Dilute hydrochloric acid will also remove stains from the skin.

CURIOSITIES OF COLOUR.

By Henry J. Slack, F.G.S., F.R.M.S.

INTENDING to resume the "Studies of Minute Life" by a notice of the micro-ferments and M. Pasteur's discoveries, the writer may be permitted to indulge in a temporary change of subject, and mention some interesting experiments with colour, in the hope of thus supplying answers to some of the questions that have been put by readers of Knowledge.

A couple of prisms will enable two spectra of the sun to be thrown upon a white ceiling, or sheet of white paper fastened to the wall. By a little management, the two spectra may be wholly or partially superposed, and the very different behaviour of light rays from pigments thus displayed. The exact and coincident superposition of two similar spectra will reinforce the colours, and if one is thus made to slide over the other, the effects of combination will by no means agree with the mixtures of analogous pigments. The red and the green will give a yellow, and this result of combining red and green is also made evident by Lord Rayleigh's "New Colour Combination Plates," made by Browning. In this apparatus a blue disc is superposed over a yellow one, and then, as the inventor says, "the yellow disc absorbs the blue light. The blue disc absorbs the yellow and orange light. By superposing the discs, and viewing a white object, as a cloud, the resultant light will be yellow. But on analysing the same with a spectroscope, it is proved that yellow light is produced by a commingling of red and green, since these rays of the spectrum are alone transmitted."

The success of the experiment depends upon hitting the exact tints of the two colours. In the specimen before the writer, the yellow is rather orangey, and when examined with the spectroscope shows colours from deep red to a blue-green. The blue disc analysed in the same way shows a band of bright red, succeeded by a cloudy one, something like a dingy, brownish puce, and then green, blue, and violet; no yellow. The combination, which gives an orangey yellow, when analysed shows red and green, and a cloudy band in place of the orange and yellow. The blue and violet are suppressed.

Another way of making interesting experiments is by obtaining from an artists' colour-shop (such as Brodie & Middleton, in Long-acre) various sheets of coloured gelatine and a sheet of black cardboard. Cut some of the cardboard into 4-inch squares; make a round hole in the middle of each square, 1½ inches in diameter. Over these holes fasten coloured gelatine of all the chief obtainable tints. This is done most usually by laying the gelatine on the unblackened side of the cardboard, and gumming over it a square paper with a hole in it corresponding with that in the cardboard. This keeps the galatine in its place. When one of the squares so prepared is held near the eye, the black part excludes most of the diffused white light, and objects in a garden or in a room may be seen as affected by the tint of the gelatine, with a result that is often very different from what might be expected. For example, a sky-blue disc slightly blues white flowers, gives a dingy purplish hue to the rich pink of dielytra, and makes the yellow-green of aucuba leaves less noticeable. After this take a disc of purple, like the common small blue glass so often misapplied in cockney conservatories. Many green leaves now flash out with red tints. At this moment the writer sees this effect in young April rose leaves, the lightest aucuba leaves, the golden green of *Thuja aurea*, and many more. Polyanthus and winter aconite leaves assume a dull lurid tint, while the dielytra blossoms have a richer glow. A crimson disc quite changes the bright leaves of aucuba and *Thuja aurea*. This shrub looks as if covered with orange-yellow flowers, while the sky, now covered with a pale cloud, takes the full crimson hue.

All the objects at the time of writing are in a fairly strong white daylight, and the leaves, according to their character and the angle under which they are seen, reflect more or less white light, besides exerting their normal effects of absorbing portions of the spectrum.

For another set of experiments, cut some pieces of black cardboard six inches long and four wide; punch, or cut, then, half-inch in diameter round holes. Cover one set severally with a blue, a crimson red, and a yellow piece of gelatine. Hold it up against a white cloud; look at it with a double image prism. By keeping the prism near the eye, and holding the cardboard at a suitable distance, to be found by experiment, and then rotating the prism, it is easy to make any one of the coloured discs overlap any other one. Sky-blue and pale yellow thus treated give nearly white light. If the same tints of pigments were mingled, the result would be a pale green. The yellow superposed upon the crimson makes it orange. Superposing the pale blue upon the crimson gives a whitish, violetish grey, the tint varying with the quantity of white light reflected from the cloud.

Prepare another card by occupying two holes with discs of blue and yellow, each having one, and the third hole with both colours superposed. The result of this superposition is a green, as it would be with pigments, and it is interesting to compare this green with the approximate white obtained by superposing the tints by means of the double image prism. A similar card prepared with crimson and green separate, and also the two combined, are likewise instructive. The actual superposed combination of the gelatines effects an orangey modification of the crimson, but when the superposition is made by increase of the prism, yellow is the result. Other combinations by superposition and by prism should also be compared.

After the above experiments, gum three strips of white paper, about one inch long by half-an-inch wide, on a square of black cardboard, a little way apart. On one slip fasten (which can be done by slightly moistening) a square of blue gelatine and one of yellow beside it. Expose to full white light, and cause superposition by the prism. The tint obtained is a chocolate grey. Red and crimson thus tinted give orange; crimson and green, dark grey.

The effect of a double image prism, by dividing one coloured ray into two, is to lessen its force, and the effect of adding, by reflection or transmission, a strong white light to any colour is to thin it out, or completely subdue it. Stick a small square of crimson gelatine in the middle of a sky-blue disc fixed in a black cardboard square; hold it against a white cloud; duplicate the image of the crimson square with the prism; such image is bluish-grey. Make portions of the two images touch; the crimson, modified by the blue, at once appears.

In studying these various effects, the exact conditions of each experiment must be noticed. Thus, dealing with spectrum colours is employing rays of light of certain refrangibilities, according to the dispersion obtained. Pigments are either opaque or transparent. The action of an opaque pigment is to absorb certain rays composing white light, and to reflect the residue. If such a pigment is seen in strong white light, the quantity thereof which it reflects acts in proportionate diminution of its normal colour. In viewing gelatine pastes held up against a white cloud, the colour seen is the residue left after its absorption of the rays that cannot pass through it. If one film is imposed over another, and both are held up to the light, the first film only transmits to the second the rays that remain after its absorptive powers have been exercised. If a transparent tint is superposed upon white paper, that paper reflects through it the rays which the coloured film has not absorbed, and subjects them to a fresh action of the film as they pass for the second time through it.

To show what white, or approximately white light can do with a strong colour, throw a full red on a white screen with a magic-lantern. Then with another magic-lantern, or a common bull's-eye lantern, placed nearer, and thus casting an intenser light, overpower the coloured light. A hand with outspread fingers, held so as to obstruct the white light, casts a red shadow. The stoppage of the white light permits the redness to reappear. This might be used in melodrama with powerful stage effect. A black figure could be made to cast a blood-red shadow as he passed a white-looking wall. The experiment in a room with the outspread fingers is very striking. Many similar arrangements will occur to your readers, and among them not the least interesting is the production of subjective tints by throwing with a lantern on a screen the images obtained by Wheatstone's superposed discs of perforated zinc. It is easy to fit in the ordinary wooden slide-holder a square of the zinc, which may be fastened to the frame, and to fix to it a circle of similar zinc, so as to rotate freely on a pin. The circle should be wide enough to project a little above the frame, and be easily reached with a finger, to make it revolve. Coloured gelatine films placed close to the front lens of the lantern will give the effects required.

"THE GREEN-BEAM PAPER."

(A SEQUEL.)

By LIEUT.-COLONEL W. A. ROSS, LATE R.A.

A SUNNY day—"happy Saturday," April 8—which has intervened since the publication in KNOWLEDGE (page 496), of the article, aptly named by you as above, having enabled me to add a few more auxiliary experiments to the original one there detailed, made on the 7th of October last, I would ask of your courtesy a little space in an early number of our delightful "Mag." for a brief description of them before your readers forget my former paper.

1. A plate of blue-violet glass, 0·162 inch thick (the thickest I had), transmitting blue-violet light, but from which light, so transmitted, a sheet of white paper reflected reddish-violet light, was held in the path of a sunbeam admitted through the open window of a scarcely-darkened room, and condensed by a lens about 2 inches in diameter, in two positions—(*a*) so that the sunbeam was focussed *on* the glass; (*b*) so that the sunbeam was focussed on the paper about half a foot beneath it, *through* the glass. In the case of (*a*) the solar image on the paper was large of course, and oval, but of a pure blue colour; being apparently the result of the elimination of red rays by the thick glass, and of yellow rays by the blue part of the blue-violet glass, leaving the blue rays only for transmission. In the case of (*b*) the focussed solar image was small, nearly circular, and of a pure blue colour, contrasting strongly with the violet transmitted to the paper by the rest of the glass. As the mixture of red and blue lights, in certain proportion, undoubtedly forms violet light; and, as blue or bluish glass intercepts yellow rays, this blue colour was, on this hypothesis, to be expected, and it was supposed by me that the purity of the blue focus on the paper was due to the thick glass cutting off red rays, according to the experiment detailed in KNOWLEDGE of April 7, and to the blue in the glass cutting off yellow rays, leaving only pure blue rays to pass on to the paper.

The focus (*a*) appeared at first of a pale orange colour on the surface of the glass, but soon became greenish, perhaps from the simultaneous transmission of some reflected blue rays given out by the main body of the glass to the retina.

2. A similarly tinted violet glass, but only half the thickness of 1, was held, with reference to the lens and paper, in the positions 1 (*a*) and 1 (*b*); when—(*a*) there was a large violet image on the paper, but with a distinct reddish border.

(*b*) The focussed solar image was small, and of a faint bluish-violet colour, so extremely pale as to be nearly white, and tolerably brilliant as reflected directly from the paper; but this bluish-white brilliant image, viewed through the glass itself, appeared of a pale violet colour, with a bright pure red or "crimson" border. *No red* was observable through the glass of 1 (*b*).

3. After dark, a thick platinum wire, bent at a right angle, and heated to faint redness before a blowpipe, was viewed through glass 2, when the "blue" blowpipe pyrocone appeared a red violet, and the wire a brilliant red colour, similar to the border 2 (*b*), which became bluish white when the wire was made white hot.

Am I wrong, then, in supposing that the thin glass 2 did not effectually cut off the red rays, some of which, therefore, escaped through it from the lens to the nearly white focus on the paper, and were contained in the reflection of that which passed directly to the retina, although invisible there; but when the violet glass intervened, the red rays were seen to be partially separated from the blue rays in their vibrations through the glass, and placed as the outer border of the pale-violet focus?

4. A green-blue glass, the thickness of 2, was treated with sunbeam and lens similarly to 1 and 2, but the only remarkable phenomenon I could observe was that the rays reflected from the focus on the glass in position *a* were an almost brilliant reddish orange colour.

5. A slip of thin ordinary glass (a microscope slide) transmitted the focussed sunbeam as white light through its shorter axis, but when turned edgeways, the "green beam" immediately appeared, and it could even thus, by turning the glass slowly round, be ascertained to what depth the beam could travel as white light; so that it would appear as if the red rays were absorbed, and not "cut off"

by the vitreous medium; and glass, I believe, is known to absorb the ultra-red rays.

6. A thin dinner-table water carafe, full of pure water, transmits the focussed sunbeam as white light.

7. (*Easter Sunday*).—This fine sunny morning I really believe I have proved my point by eliminating, or nearly eliminating, the red rays from the solar spectrum thrown on a sheet of white paper, by carefully introducing "the green beam" into the prism: when, instead of the broad red band, an oval spectrum appeared, full of pale bluish light, with a broadish yellow, and an extremely narrow, faint, red border, the latter attributable, I think, to the imperfection of my apparatus, for my little room was full of white light and even sunbeams, which were also copiously reflected to the prism from the sheet of white paper on the floor.

After all this, some of your readers may ask "what is 'your point'?" I will, therefore, conclude by answering (1) that I believe glass of ordinary density, and of considerable thickness, absorbs the least refrangible (or red) rays of white light which has already been refracted through a lens: and (2) that Sir David Brewster was not far wrong when he asserted that white light is composed of three primary colours, red, blue, and yellow: from which all other colours can be produced.

(*May* 7, 1882).—Since the above was written, last Easter Sunday, having been to Germany, *viâ* Holland, to place my two little daughters at school in Coblentz, I took the opportunity of showing, or communicating the "Green-Beam Phenomenon" to Professors H. A. Lorentz, of Leyden University; M. Kekerlé, of Bonn University; and Bruno Kerl, of the Imperial Mining Academy, Berlin, who has published articles on my new system of Blowpipe Analysis in the *Berg und Hüttenmannische Zeitung*.

Dr. H. A. Lorentz, to whom I showed the phenomenon at Amsterdam on Saturday, April 29, was astonished that such a startling phenomenon as the "Green-Beam" had not been discovered before, but expressed still greater surprise when I told him I had been trying in vain to get an account of it published in the proceedings of our scientific societies for six months!

I have now made the following additional experiments.

8. I have passed the Green-Beam through a thin glass flask filled with sulphate of quinine solution, and *vice versâ* (to see if it was due to fluorescence).

9. I passed it into a cobalt-blue glass paper-weight at least two inches thick.

10. Into a thin glass flask containing a blue solution of ammonia-sulphate of copper, and *vice versâ*.

11. Reflected the focused beam from a small mirror upwards into a thick glass paper-weight.

12. Passed the Green-Beam through one solid glass paper-weight into a prism.

I must, however, reserve the results of these experiments for another paper.

After the above article was in type, Prof. G. G. Stokes was so kind as to examine the "Green Beam" at the Royal Society's rooms, on May 11 (although there were only glimpses of sunlight), and to refer me to Sir David Brewster's papers "On the Decomposition and Dispersion of Light," in the *Philosophical Magazine*, &c., which, he says, must be determined to contain the first discovery of this phenomenon, termed "Internal Dispersion," by Brewster. Here are Sir David's own words (*Phil. Mag.*, 1848, Vol. XXXII., page 403):—"In order to observe the phenomena of dispersion most distinctly, I transmit a condensed beam of the sun's light through the specimen when partially covered with black wax or velvet. . . . page 404. I have found several glasses which possess internal dispersion one in particular, of a yellow colour, which disperses a brilliant green light. . . . In these cases the glass has a decided colour of its own; but I have found many specimens, both of colourless plate and colourless flint glass, which disperse a beautiful green light."

Some of these words most decidedly describe in part the phenomenon I thought I had first discovered. But there is also a great difference. Sir David evidently only experimented on (comparatively) thin plates of glass (those were not the days of massive glass paper-weights); whereas the "Green Beam" can only be properly exhibited in blocks or cubes of solid glass some $2\frac{1}{2}$ inches thick. Secondly, it is evident from his drawings and descriptions that he merely passed the sunbeam through the thin glass, whereas I focussed it upon the surface of the thick. In the first case there is no green beam at all, but only a kind of diffused green light, much less brilliant than the beam.

That the phenomenon of the green beam is not the result of dispersion but of transmission, seems to me provable by the facility with which it burns black cloth when passed through a crystal paper-weight $2\frac{1}{2}$ inches thick. Who ever heard of burning cloth with the green colour dispersed from a solar beam by a prism? Finally, I think it is also provable that the green beam is due to the union of blue and yellow rays, by the fact that if the thick cube crystal paper-weight is covered by a plate of yellow or orange glass—which cuts off the blue rays—you have no longer a green beam by focusing on the surface, but only a faint yellow one, whereas the green beam is invariably transmitted through violet, pink, and other coloured glasses, which are not opaque to blue rays, with the exception of red glass,* through which no beam at all is transmitted into the paperweight; *i.e.*, white light seems to be re-formed.

MEASUREMENT OF THE FOCAL LENGTH OF DEEP CONVEX LENSES.

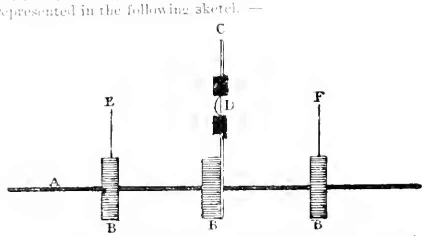
By T. W. Webb.

THE difficulty of ascertaining the focal length of small convex lenses within reasonable limits of error is so considerable, and the result is often so untrustworthy, that I am induced to ask the editor's sanction for the appearance of the following extract from one of my old note-books. Its value, I fear, is not in proportion to its length, on account of the needless minuteness of its detail; all that is worth preserving might have been compressed within much narrower limits; but, possibly, those who, like myself, are fond of manufacturing optical contrivances with ordinary materials may find in their own experience some apology for its tediousness.

T. W. Webb.

Account of a Method of finding the Focal Length of a very small Lens, employed by me, November 24, 1835.

As this process may be useful, on many occasions, both to myself and others, I intend to give an account of all the steps of it. The deepest eye-piece of my $5\frac{1}{2}$-feet achromatic is marked by Mr. Tulley 250. To increase the power I had frequently unscrewed the field-glass, and used that next the eye only; and my object was to find the power thus obtained by measuring the focal length of the lens. There were, however, considerable obstacles in the way. It is very difficult to measure the focus by receiving the image of the sun on paper, because the spherical aberration of a deep lens is so considerable that it is hard to say where the image is best defined; and this uncertainty is greater than might be imagined by a person who has never made the trial. I attempted to measure the image of the object-glass formed by this eye-lens with a micrometer, upon the principle of the dynamometer; but this image was formed so close to the eye-lens that the hairs of the micrometer could not be brought near it. It then occurred to me that if I could ascertain the distance between the conjugate foci when they were equidistant from the lens, one fourth of that distance (by Prop. XLV. of "Wood's Optics") would be the (principal) focal length. To attain this object by the simplest means, I took a knitting needle, and having bored holes through three thick discs of cork, made them to slide upon it. To the centre disc was glued a piece of card, into a hole in which I stuck the brass cell containing the lens. In each of the other discs I stuck a piece of a broken sewing-needle, so that its end might range at about the same height with the centre of the lens. This apparatus is represented in the following sketch.—



A is the knitting-needle, B B B the cork discs, C the card into which was stuck the brass cell carrying the lens D, E and F the pieces of sewing-needles. Then, by the above-mentioned proposition, if E and F could be so arranged with respect to D, that, while they were equidistant from it, the image of E should be formed at F, and of F at E, then the distance from E to F would be four times the focal length for parallel rays. I therefore took a deep lens in my hand, and while I viewed F in its focus, moved E to different distances, until its inverted image was seen in conjunction with F.

If then E and F appeared to be equidistant from D, they were in

* The red glass I have is red only on the upper and under surfaces; internally it is colourless. All my other coloured glasses are homogeneous.

the required position, or sufficiently near it; if not, both would require to be moved a little so as to satisfy the above condition. When that was obtained, all that remained was to take the distance between E and F by a pair of compasses, and determine its value from a scale. I have an excellent ivory scale, divided into fortieths of an inch; and to save trouble, I assumed this quantity as the unit of my measurements. Having made the simple and rude arrangement described above, I attempted the operation, E and F being stuck with their points into the cork discs. My first trial, however, proved that under a deep lens their blunt ends appeared too coarse to admit of accuracy. I therefore stuck them the other way into the corks, with the points upwards, which answered much better. My first trial, with the blunt ends, gave the distance from E to F 30 parts of the scale; the second, with points, 31; both by candlelight. The next morning five trials gave 31·166, 31·666, 31·0, 30·666, 31·0. (When the distance taken by the compasses did not exactly correspond to any division of the scale, I repeated it on a paper by the edge of the scale till a coincidence was obtained, upon the principle of the vernier.) The lens was plano-convex, and these trials had been made with the convex side towards the eye; it struck me that, on account of the aberration, it ought to be turned the other way, which was done, though, in fact, it was needless, as I was mistakenly reasoning from the case of parallel rays. I then commenced a fresh set of trials, which gave me the following results:—32·0, 31·333, 31·5, 31·5, 31·25, 31·8, 31·5, 31·3, 31·6, 31·6, 31·6. It should be mentioned that, after every trial, the discs carrying the needles were moved from their position, and the succeeding trial was commenced entirely afresh. It was soon apparent that the method deserved confidence, and the results agreed more nearly as my eye and hand became accustomed to the operation, the last three exactly coinciding; whence I preferred their value, as an approximation to the truth, to a mean of the whole, and deduced $\frac{31{\cdot}6}{4} = 7{\cdot}9$ (fortieths of an inch) as the focal length of the lens for parallel rays.

I will now mention the difficulties and the advantages of this method. The greatest difficulty is in measuring the distance between the points of E and F with compasses. This, perhaps, could only be done by a very near-sighted person like myself (or by the aid of suitable spectacles), and required practice and steadiness of hand. Another source of error lay in the possibility that the points E and F might not lie in the axis of the lens, but might be originally fixed too high or too low, or might slip on one side, as, indeed, frequently happened from the turning of the cork discs upon the knitting-needle as an axis. Another difficulty consisted in estimating the position where E and F should be equidistant from D. The situation of the latter in (the depth of) its cell could not be very plainly seen, and as its thickness was considerable, it was uncertain from what point in it they should be equidistant.

It must be observed, however, that every one of these sources of error is to be ascribed, not to the method, but solely to the defects of so rude and imperfect an apparatus, which might easily and completely be remedied. A microscopic eye and steady hand were the only means I employed to obviate them, and, as the results show, not without success. As I became more familiar with the operation, I guarded more carefully against the second source of error; and hence, probably, arose the increasing coincidence of the later determinations; the third difficulty, which I found least remediable, was not likely to produce any injurious effects. For calling x the principal focus between E and D, and y the principal focus between D and F, we have, by Cor. 3 to the before-cited Proposition,—$\mathrm{E}x : x\mathrm{D} :: \mathrm{D}y : y\mathrm{F}$. Supposing now that E has been placed at a distance of 14·8, instead of 15·8, from D, we have $y\mathrm{F} = \frac{x\mathrm{D} \cdot \mathrm{D}y}{\mathrm{E}x} = \frac{7{\cdot}9^2}{6{\cdot}9} = 9$ nearly, and EF will be $= (6{\cdot}9 + 7{\cdot}9 + 7{\cdot}9 + 9 =)\ 31{\cdot}7$, instead of 31·6, an inappreciable difference. Or if ED was only 14 (and a greater error is very unlikely, since that would make DF = 18·1), still EF would be only 32·1, which would make but about 0·012 of an inch difference in the focal length.

The advantages of the method are—1. The accuracy with which the place of the conjugate focus may be determined. If the image of E is brought so that its point may be directly over, and close to, that of F, an eye accustomed to telescopic observation will determine, without much trouble, the situation in which both are most distinct at the same time; and this might be reduced to greater certainty, if the lens with which they are examined is fixed in a slide instead of being held in the hand. 2. The elimination of the thickness of the lens whose focus is to be measured—a very troublesome quantity, which it is a great advantage to get rid of. 3. That the errors of observation are diminished to one-fourth in the final result; the measured distance being four times the focal length required.

BUTTERFLIES AND MOTHS.

THE science of Entomology, during the last few years, has made enormous progress, both in regard to the number of its devotees, and also in the discoveries of the best methods of obtaining and preserving the insects. We purpose, therefore, giving a few notes weekly relating principally to the order Lepidoptera, about the haunts, times for catching, food, &c., of our British butterflies and moths, which one may generally expect to find during the course of the year.

The present may be called the opening month of the season, and the entomologist will find plenty of work to do, both day and night, in the lanes and woods of the country. A bright May-day will most certainly bring numbers of butterflies to the net of the energetic collector who keeps his eyes open, and who can manage a sharp walk over fields and lanes without feeling the fatigue.

The first thing for the would-be entomologist to do is to obtain a net, a few setting-boards, some entomological pins, and two or three dozen chip-boxes, and then he can consider himself well set up to begin with. All these articles he can make himself, with the exception of the pins and chip-boxes, which can be obtained at a small cost of the dealers in natural history implements and specimens. The setting-boards mentioned are pieces of soft pine about fourteen inches long, and of various widths, with a cork face, and a groove cut down the centre.* Entomological pins are a very fine and sharp-pointed kind, made specially for this purpose; they can be obtained in assorted sizes at 1s. per ounce, one ounce lasting most people an entire season.

We will now suppose the insects to have been caught, and the momentous question of how to kill them crops up. For butterflies, the readiest method is to use a killing bottle composed of cyanide of potassium, covered over with a layer of plaster-of-paris in a wide-necked bottle; the insect is killed very quickly, but unfortunately the wings get set equally rapidly. Another way is to squeeze the thorax of the victim whilst in the net. This kills immediately, but has the disadvantage of slightly damaging the insect. For moths, the best plan is to stupefy with chloroform, and then prick them just below the *thorax*, or the under side, with a sharp pen dipped in a saturated solution of oxalic acid.

Setting is the next process. It is accomplished in the following manner: One of the entomological pins is thrust through the moth near the head and the wings stretched out,* and then a narrow strip of cardboard pinned over to keep them in position. This operation requires great care and neatness, and must be done very shortly after the death of the insect, or the wings get *set*, and cannot be moved without considerable trouble and delay, and very often damage to the moths during the process of relaxation. This latter will be described in a future paper.

The insects must be left on the setting-boards for a period varying from four or five days upwards, according to the size of the moth or butterfly and the length of time the boards can be spared. Personally, whatever the size of the moth, we let it remain until we require the board again, which is generally at least a month or six weeks after, as we keep a large number f boards in use.

The different stages of the work required to be done in killing and setting the insects having been described, we will pass on to the manner of keeping the specimens in good order and condition.

Our first advice to the tyro is, not to get a cabinet unless he can afford to buy a thoroughly good one, with air-tight and dust-tight drawers.

By far the best plan is to use store boxes of moderate size, either plain or covered with green baize, as they are perfectly air-tight, and do not allow the light to penetrate, which is a very great consideration, owing to the colours of some moths being very unstable, greens and other light colours often becoming white in a very short time, owing to exposure to the action of light. However, if getting a cabinet is decided on, we repeat, *let it be a good one, with well-fitting drawers.*

The arrangement of specimens depends to a great extent on the taste and fancy of the collector; but the most scientific and satisfactory method is to follow the order of *genera, families*, and *sub-families*, which the best naturalists have adopted. Next week we will give this classification in full, as space forbids in this paper. A piece of camphor about the size of a walnut must be put in each box or drawer to keep off the mites, whose ravages have sometimes, in one single winter, destroyed or damaged the whole work of the preceding summer, when this precaution has been neglected.

Everything now is done connected with the indoor work of the entomologist, and next week we purpose describing the different processes used for catching both diurnal and nocturnal Lepidoptera, together with their habits and other useful information.

* Diagrams illustrating the above will be given in our next number.

THE ELECTRIC TELEGRAPH.

By W. Lynd.

CIRCUITS AND GALVANOMETERS.

In a previous article I explained that when the wires attached to the metallic plates of a simple voltaic cell are joined together, chemical action begins, and a current of electricity is said to flow from the zinc through the liquid to the copper, through the wire, and back to the zinc plate. Without a complete circuit, telegraphy is impossible. For instance, suppose that a wire is connected to the terminals of a battery, and carried on poles to its destination—say from London to Birmingham—and the extremity of the wire attached to the terminals of an instrument at the latter place, no current would be received at Birmingham. If we lay on a water-pipe from a reservoir to a private house, water will flow through the pipe, and it can be drawn off by means of a tap, when required. Compare the voltaic battery with the reservoir, the conducting wire with the pipe, and the difference will be clearly understood. From the reservoir, one pipe is sufficient to enable the water to flow, but the voltaic battery must have a wire or conductor in connection with its positive pole carried to its destination through the instrument and back again to the negative pole; the current must make a complete circuit, no matter how great the distance may be between the two poles of the battery. In actual practice, the return wire is dispensed with, and the earth performs the functions of a second wire, and completes the circuit in a very remarkable manner. To make what is called the earth circuit perfectly clear, take a voltaic cell, and instead of bringing the wires in contact, connect them with two plates of metal buried in the earth, or, for convenience sake, join the wires to the gas or water-pipes; the current will flow as before, and in exactly the same direction. The earth actually plays the part of a metallic wire, and the electric current flows from plate to plate, no matter how great the distance may be. The essential parts of a telegraph circuit are the battery, or generator of the current, the conducting-wire, the earth, and the instrument for recording signals. The circuit is open when there is a break in the wire, and closed when it is continuous and the current is flowing.

The time a current takes to flow from pole to pole in a circuit is imperceptible to the senses. A telegram can be sent from London to Glasgow or Paris, as quickly as from St. Martin's-le-Grand to Shepherd's Bush. During the recent repairs of the telegraph cable near Bombay, the steamers *Chiltern* and *Great Northern* were about half-a-mile apart, the former having hold of a shore-end cable, and so was in telegraphic communication with Bombay, the latter having hold of a sea-end, and so was in telegraphic communication with Aden. The *Chiltern* desired the *Great Northern* to splice on to the cable-end held by the latter, and pay out three-quarters of a mile of cable, and this was communicated by wire from the test room of the *Chiltern*, passing through all the coils of cable in her hold, and on to Bombay, whence it was sent on to Aden, and back from Aden to the *Great Northern!* Thus, as a *speedy* means of sending a message *half-a-mile*, it was forwarded by a route between three and four *thousand* miles long! The following morning, when the two vessels were within a quarter-of-a-mile of each other, communications passed between them constantly in the same way. Of all the miracles of modern science, truly this annihilation of distance is the most wonderful! The student will now understand how the electric current is generated, and conducted to its destination. He will, however, naturally desire to know how the electricity which flows through the circuit is made to record signals—in short, how intelligible communication is established between two distant places. All telegraph signals depend on the power of the telegraphist, who, to make them transmit, controls and modifies the current at will. Let us imagine a battery of several cells in the office of the editor of this journal in Great Queen-street, and suppose that we connect a wire to the positive pole, and carry it on posts to the Observatory at Kew, where the wire must be attached to a plate buried in the earth or connected with the gas-pipe. Join up the negative pole of the battery with a buried plate or gas-pipe, and the circuit will be complete. Cut the wire at any point between London and Kew, and the circuit will be broken—no electricity will flow through it. The current can be instantly re-established by bringing the cut ends together. Now, if we had some kind of apparatus that would make the currents visible, we could easily arrange a code of signals to represent the letters of the alphabet. There are now many ways of recording signals. Some of the instruments used are very complicated, and depend to a great extent upon ingenious mechanical contrivances. The simplest form of instrument is called a galvanometer, a modification of which is known as the receiving portion of the needle telegraph. To understand the principle of the galvanometer, the relations between electricity and magnetism must be studied. A very simple experiment will suffice to prove that electricity is influenced by magnetism, and *vice versâ*. A copper wire suspended horizonally over a movable magnetic needle will, when connected with the poles of a voltaic battery, cause the needle to place itself at right angles to the wire. If the current passes above the needle, and goes from south to north, the north pole of the magnet deflects towards the west. If the current passes below the needle, also from south to north, the north pole will deflect towards the east. If the current passes above the needle, but from north to south, the north pole is deflected towards the east. If the current goes from north to south below the needle, the deflection is towards the west. To enable the student to remember these movements, Ampère suggested the following rule:—If we imagine a man swimming in the wire, with the current, with his face turned towards the needle, the north pole of the needle will be deflected towards his left hand. According to Preece, "the motion of the needle is produced by the mutual action of currents and magnets. Electricity and magnetism are so intimately related to each other, that by many they are thought to be only different phases of the same agency. Thus the motion of a magnet always produces electricity; the transference of electricity always produces magnetism. The neighbourhood of a current is, in virtue of this fact, a *magnetic field*, a term introduced by Faraday to denote the entire space through which a magnet diffuses its influence—and a magnet or piece of soft iron placed there is influenced by the magnetism of that field." To increase the intensity of the current, the wire is wound several times round the magnetic needle, each coil being still parallel. Each successive coil produces a separate effect upon the needle, so that if there are a great number of coils of wire passed carefully before and behind the magnet, the deflecting force will be multiplied in proportion to the number of coils, and the most feeble current can be made to affect the needle. The fine wire used for this purpose is covered with cotton, silk, or some substance which is a non-conductor of electricity. If the coils of wire were not insulated, they would allow the current to pass from coil to coil, instead of following the continuous thread of wire. The wire is coiled round a frame, in the centre of which is suspended the magnetic needle; upon the same axis is fixed a steel indicator, which points to a scale, and this measures the strength of the current sent through the wire. The galvanometer is included in the circuit, and when contact is made, the indicator, which hangs in a vertical position when at rest, deflects in proportion to the intensity of the current. The single needle instrument, which is only a modification of the galvanometer, will be explained in a subsequent article.

Reply to Query.—[349]—Quotation, p. 502:—

> "Little things
> On little wings
> Bear little souls to heaven,"

Are lines taken from a poem written by Father Faber.

Tubercle and its Cure.—The consequences that will be witnessed as flowing from the studies conducted by Koch in connection with tuberculosis are impossible to foreshadow in any completeness, but we can, even at this early stage, perceive something of the widespread benefit that may ensue from them and their publication. Not least important of these must be considered the bearing which antiseptic treatment has been shown to have on the productive cause of the disease; the inference that is irresistible from a careful survey of the facts demonstrated by Koch's researches, indeed, is to the effect that we may very possibly find a future remedy for incipient, and even for pronounced, phthisis in the submission of the infected individual to active antiseptic measures. In this connection, an instructive and important communication has been made to the *Times* by Mr. R. R. Maddison, who states that, having proceeded to Madeira in the hope of simply prolonging life somewhat, but with absolutely no prospect of recovery from the consumptive condition, he resorted to the use of carbolic acid as a protection against mosquitoes. The vapour of the acid was necessarily inhaled by him, and to its beneficial effect on his lungs he ascribes an improvement in his state. He continued the use of the acid, and returned to England recovered, with the further consequence that he has remained well ever since. If, as seems quite justifiable, we may attribute this cure to the destructive effect of the germicide acid on the bacilli of tubercle, there opens up a very grateful prospect of possible relief in innumerable cases of lung disease; and at any rate it offers strong inducements to resort to treatment at once easy and possessing such promise of successful results.—*Medical Press.*

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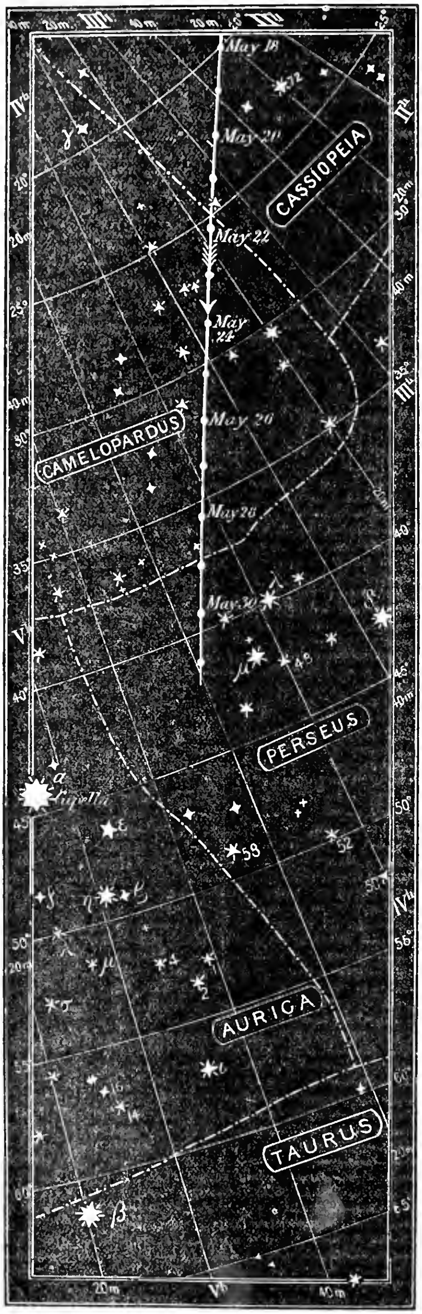

THE COMET.

THIS week we give the comet's path (as computed by Dr. E. Lamp, of Kiel), from May 18 to the end of the month. As some correspondents who have equatorially-mounted telescopes are not altogether content with the mere mapping of the path, though the comet can now be readily seen in a good field glass (we have seen it for the last fortnight in one of Browning's Panergetic Glasses) directed to its mapped place for the night, we append Dr. Lamp's positions in right ascension and North Polar Distance for the last half of May :—

Date.	R A. h.	m.	s.	N P D. °	′
May 13	0	41	31	15	56
„ 14	1	5	14	16	29
„ 15	1	27	30	17	11
„ 16	1	48	5	18	3
„ 17	2	6	52	19	2
„ 18	2	23	53	20	9
„ 19	2	39	14	21	22
„ 20	2	53	0	22	41
„ 21	3	5	21	24	5
„ 22	3	16	26	25	34
„ 23	3	26	24	27	6
„ 24	3	35	20	28	41
„ 25	3	43	27	30	20
„ 26	3	50	46	32	2
„ 27	3	57	24	33	47
„ 28	4	3	27	35	35
„ 29	4	8	57	37	25
„ 30	4	14	1	39	19
„ 31	4	18	39	41	15

The perihelion passage will take place on or about June 10, midnight, at which time the comet's distance from the sun will be only 61 thousandths of the earth's mean distance.

CHALCEDONY ENCLOSING LIQUIDS.—I have read with much interest the paper by the Rev. Mr. Wiggins on chalcedony enclosing liquids, in your issue of March 24. A specimen, such as he describes in the collection of Mr. Patrick Dudgeon, is referred to in Traill's "Treatise on Quartz and Opal," 1870, where a considerable quantity of water enclosed in the chalcedonic druse was lost by gradual exudation through the crystalline pores during the course of years, but which was partly restored. At Professor Heddle's suggestion, the nodule was immersed for a considerable time in water, under the exhausted receiver of a powerful air-pump; the air was thus exhausted from the interior of the nodule, and favoured the gradual admission of water upon the restoration of the atmospheric pressure.—MINERALOGIST.

THE FEVER TREE.—The *Eucalyptus globulus*, which is being introduced in California, has many qualities which recommend it to Eastern sylviculturists. It comes originally from Australia, where the tests of various soils and varying seasons have amply demonstrated the good qualities of the tree, and its rapid growth even under adverse surroundings. Trials of late years in Southern Europe have further verified these claims, and there is no reason why the tree will not flourish in every section of the United States. It is hardier than the chestnut, and, like the latter, it will grow in the rockiest soils. It is more independent of rain food than any tree known in this country, wet and dry seasons alike failing to affect its growth. Its wood is hard, somewhat of the nature of yellow pine, but firmer and stronger, and fit for use in ship timbers, while in Australia cabinet-makers, wheelwrights, and carpenters use it throughout their trades. The bark yields a febrifuge second only in efficiency to quinine, but superior in all medical qualities to cinchona. This quality alone must make the tree invaluable, and its culture here, to an appreciable extent, would settle for ever the vexed question of quinine duties. The rapidity of its growth is its most wonderful feature. It grows four times as fast as the American pine, and for all ordinary purposes is fit to cut in five or six years. For the Eastern and Middle States, where the lack of forest protection is not infrequently felt, no tree has been offered the culturist that can present so many primary points of vantage as the Australasian immigrant, and its general introduction and culture should only be a matter of but little time. It may be added, the tree yields fragrance, but produces no fruit or nut. Its beauty of form and luxuriant evergreen foliage are additional qualities that must recommend it in country or city.—*Frank Leslie's Illustrated.*

WEATHER REPORT, FOR WEEK ENDING SATURDAY, MAY 13.

STATION.	ABERDEEN.						
Day of Week.	Sun.	Mon.	Tues.	Wed.	Thurs.	Fri.	Sat.
Thermometer in Shade. Max. ... Min. ...	48 41	48 38	49 36	51 37	61 43	56 40	57 41
Day's Range..	7	10	13	17	18	16	16
Barometer. Inches. 10ths...	30 ·03	30 ·20	30 ·21	30 ·21	30 ·12	30 ·03	30 ·32
Wind. Direction	E	ENE	S	W	SW	NW	NW
Force (0 to 12) ...	3	2	6	2	2	8	5
Weather. Beaufort Scale* ...	o	b c	o	b v	c	b	o
Rainfall. 100ths of inch..	0	3	0	10	0	39	0

STATION.	LIVERPOOL.						
Day of Week.	Sun.	Mon.	Tues.	Wed.	Thurs.	Fri.	Sat.
Thermometer in Shade. Max. ... Min. ...	55 44	59 43	55 41	53 45	60 51	66 46	55 44
Day's Range..	11	16	14	8	9	20	11
Barometer. Inches. 10ths...	29 ·97	30 ·08	30 ·33	30 ·37	30 ·25	30 ·27	30 ·37
Wind. Direction	SSE	ENE	SE	WNW	SW	WNW	NW
Force (0 to 12) ...	1	1	2	2	13	3	1
Weather. Beaufort Scale* ...	b c	m	o	b	b	b	b
Rainfall. 100ths of inch..	0	0	0	17	0	4	0

STATION.	VALENCIA.						
Day of Week.	Sun.	Mon.	Tues.	Wed.	Thurs.	Fri.	Sat.
Thermometer in Shade. Max. ... Min. ...	55 43	57 44	57 51	55 52	58 52	57 45	62 42
Day's Range..	12	13	6	3	6	22	20
Barometer. Inches. 10ths...	30 ·02	30 ·22	30 ·27	30 ·39	30 ·26	30 ·31	30 ·23
Wind. Direction	WNW	NNW	SSW	WSW	SW	ENE	ESE
Force (0 to 12) ...	3	1	6	4	3	3	3
Weather. Beaufort Scale* ...	b c	b c	o u	o m	o m	b	b c
Rainfall. 100ths of inch..	4	0	0	8	1	1	0

STATION.	LONDON.						
Day of Week.	Sun.	Mon.	Tues.	Wed.	Thurs.	Fri.	Sat.
Thermometer in Shade. Max. ... Min. ...	64 43	65 42	60 40	59 46	68 47	70 50	65 38
Day's Range..	21	23	20	13	21	20	27
Barometer. Inches. 10ths...	29 ·97	30 ·01	30 ·13	30 ·36	30 ·31	30 ·21	30 ·35
Wind. Direction	SSW	NW	NNW	W	SW	NNW	—
Force (0 to 12) ...	1	2	1	2	1	2	0
Weather. Beaufort Scale* ...	b c	b m	m	o	—	c	f
Rainfall. 100ths of inch..	0	0	0	0	0	0	0

STATION.	LYONS.						
Day of Week.	Sun.	Mon.	Tues.	Wed.	Thurs.	Fri.	Sat.
Thermometer in Shade. Max. ... Min. ...	71 51	69 54	70 45	54 41	61 ?	72 52	81 46
Day's Range..	20	15	25	13	?	20	35
Barometer. Inches. 10ths...	29 ·95	29 ·96	30 ·32	29 ·45	30 ·37	30 ·16	30 ·17
Wind. Direction	NNE	NW	NNW	W	N	S	S
Force (0 to 12) ...	1	3	5	7	3	1	1
Weather. Beaufort Scale* ...	b	o	b	b	b	b	b
Rainfall. 100ths of inch..	0	32	12	0	0	0	0

* WEATHER.—*Beaufort Scale* is, b. blue sky; c. detached clouds; d. drizzling rain; f. fog; g. dark, gloomy; h. hail; l. lightning; m. misty (hazy); o. overcast; p. passing showers; q. squally; r. rain; s. snow; t. thunder; u. ugly, threatening; v. visibility, unusual transparency; w. dew.

Reviews.

"STUDIES IN MICROSCOPICAL SCIENCE."*

THESE studies ought to succeed, for they supply a real want—one which, in the case of microscopic study, can fortunately be supplied. The astronomer cannot, unhappily, send round specimen planets for study by those who possess telescopes of sufficient power. Nor can the chemist or geologist conveniently send out geological or chemical specimens for examination. But it is possible to send microscopic subjects, and this is what Mr. Cole proposes to do viz., to issue weekly for the use of students, teachers, the medical profession, and others, microscopical preparations, together with finely-executed lithographs of the specimens thus sent out for observation. The first specimen is one of yellow fibro-cartilage from the pinna of a cow's ear, stained doubly in logwood and resin; it is shown in the lithograph magnified 33 times linear. The methods of staining are charmingly described, and the explanation of the structure, shape of cells, matrix, and other features of the specimen may be relied on as thoroughly accurate. In Number Two, the subject for illustration is a transverse section (through the internode of the stem of a first year's twig) of the copper beech, stained in carmine and iodine green, and magnified 25 diameters in the illustration. The bibliography of the subject is in each case very full. The work is thoroughly sound and good, and will delight practical microscopists.

LAND AND WATER.

ALL the fossil-bearing rocks on the globe have been formed from the sediment brought down by rivers to the sea, and this sediment has been worn off from the hills and valleys and plains of ancient continents. In recent years it has been attempted to calculate the amounts of sediment worn off by various great rivers from the surface of the regions drained by them; and the results are very interesting and instructive. The Mississippi, for example draining a country with scanty rainfall, and having its sources in the Alleghanies and the Rocky Mountains, where there are no glaciers, performs its work of denudation slowly. The Mississippi wears off from the whole immense area drained by it about one foot in 6,000 years; while the Po, on the other hand, having its sources in the glaciers of the Alps, works with great rapidity, and lowers the area drained by it at the rate of one foot in 729 years. The mean rate of denudation over the globe seems to be not far from one foot in 3,000 years. Now at this rate, and from the action of rivers alone, it would take only two million years to wear the whole existing continent of Europe, with all its huge mountain masses, down to the sea-level, while North America, in similar wise, would be washed away in less than three millions.

But while the raindrops, rushing in rivers to the sea, are thus with tireless industry working to obliterate existing continents, their efforts are counteracted, here and there, and with more or less success, by slow upward thrusts or pulsations from the earth's interior, which gradually raise the floors of continents. The general result of the struggle has been that, ever since the earliest geological periods, the surfaces of the great continents now existing have been subject to irregular oscillations; now partially or almost entirely disappearing beneath the sea, now recovering ground as archipelagos, or rising high and dry to great elevations, as in the case of Africa. The oscillations have not ordinarily exceeded from 6,000 to 10,000 feet in vertical extent. There is no reason for supposing that the general relative positions of the great continents and great oceans have altered at all since the beginning of the Laurentian period. Since life began on the earth there is no reason for supposing that the bottoms of the stupendous abysses which hold the waters of the Atlantic, the Pacific, and the Indian oceans have ever been raised up so as to become dry land. Once geologists thought otherwise, and land was turned into sea and sea into land by facile theorisers, as often as it was supposed to be necessary to account for the distribution of certain lizards or squirrels, or for changes in climate, such as have left marks behind in many parts of the earth. The greatest physical geologists now living, however—such as Mr. Croll and the brothers Geikie—are convinced that there has been no considerable change in the positions of the great oceans from the very beginning; and this view is ably sustained by Mr. Wallace who is probably the highest living authority on the distribution of plants and animals—in his profound and fascinating treatise on "Island Life," lately published.—Mr. Fiske, in the *Atlantic Monthly*.

* "Studies in Microscopical Science." By Arthur C. Cole, F.R.M.S. (Baillière, Tindall, & Co.)

Letters to the Editor.

[*The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with their writers. All communications should be as short as possible, consistently with full and clear statements of the writer's meaning.*]

All Editorial communications should be addressed to the Editor of KNOWLEDGE; *all Business communications to the Publishers, at the Office, 74, Great Queen-street, W.C.*

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*** *All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.*

All Letters or Queries to the Editor which require attention in the current issue of KNOWLEDGE, *should reach the Publishing Office not later than the Saturday preceding the day of publication.*

(I.) Letters to have a chance of appearing must be concise; they must be drawn up in the form adopted for letters here, so that they may go untouched to the printers; private communications, therefore, as well as queries, or replies to queries (intended to appear as such) should be written on separate leaves.

(II.) Letters which (either because too long, or unsuitable, or dealing with matters which others have discussed, or for any other reason) cannot find place here, will either be briefly referred to in answers to correspondents, or acknowledged in a column reserved for the purpose.

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

"God's Orthodoxy is Truth."—*Charles Kingsley.*

Our Correspondence Columns.

THE "CONEY" OF SCRIPTURE AND ITS ZOOLOGICAL POSITION.

[398]—"S. LUCAS," in a letter addressed to the Editor, enters into a description of the resemblance which is alleged to exist between the rhinoceroses and the little hyrax, or "coney," of Scripture. Mr. Lucas also gives a comparison of the habits of the former animals as compared with the latter, and then asks how or why Cuvier included both in the same family. He adds, what of course no zoologist denies, that the teeth alone are by no means certain guides to the affinities of animals. Mr. Lucas, by consulting any recent manual of zoology, will readily satisfy himself that the hyrax is no longer placed near the rhinoceros-group; Professor Owen being the only authority, perhaps, who still places the hyrax-family definitely amongst the "odd-toed" hoofed quadrupeds like the rhinoceroses. No anatomist leans upon teeth alone as a guide to classification. Furthermore, hyrax exhibits even in its teeth important differences from rhinoceroses; and I may add that it was not so much the similarity in *number* of molars and premolars (as Mr. Lucas seems to think) that suggested rhinoceros affinities, but the shape and structure of these teeth. The hyrax has four incisor teeth, eight premolars, and six molars in each jaw, canines or eye-teeth being wanting. The upper incisors grow from permanent pulps, as in *Rodents.* The placentation of the hyrax is utterly unlike that of rhinoceroses, being deciduate and zonary, as in *Carnivora.* Again, the nails of hyrax are not hoof-like, but almost flat; and the upper lip is cleft, as in rodents. Hyrax is also remarkable for possessing more vertebræ in the back and loins than any other land mammal—the number being from twenty-nine to thirty-one. The modern view of the zoological position and affinities of the coneys is that which regards them as intermediate between the hoofed quadrupeds and the rodents. This is Brandt's view, and he adds that it is more ungulate (not necessarily rhinocerine) than rodent in its nature. Huxley says that the coneys lie between the hoofed quadrupeds and the rodents and insectivora. In any view, Mr. Lucas may rest assured that the position of hyrax is discussed and settled to-day from a consideration of its anatomy as a whole, and not from the shape or structure of its teeth alone.

ANDREW WILSON.

CONSUMPTION.

[399]—Without infringing on the space of KNOWLEDGE, allow me to say in its columns, *àpropos* of Professor Tyndall on "Consumption," that Koch does not show that *bacilli* are the *primes,* or first causes, or occasions of natural consumption; or that hereditary phthisis arises from them. He does not show that *bacilli* = scrofula; nor that in the course of these generations—their "culture"—they are more than mere carriers of an evil matter, influence, or incitation; but not the causes of it. Ordinary generation does not necessarily disown spontaneous generation, or *genesis,* as its beginning. *Bacilli* may be not the *principium* of the disease, but one of its consequences—perhaps a remote consequence,—capable, however, in rabbits, &c., of generating the disease after it has been their genesis. I think it is Wilson Fox who said that scratching rabbits with a sharp stick—*baculus*—will sometimes produce consumption in them, so liable are they to the peculiar rot of scrofula.

Trusting to your openness in KNOWLEDGE for the insertion of this note, which opposes such a sea of opinion, I am, yours obediently,

GARTH WILKINSON, M.R.C.S.E.

[400] If Koch's views, endorsed by Professor Tyndall, and published in the *Times,* should prove to be correct, a great social revolution will be brought about by them; for hitherto, with rare exceptions, pulmonary consumption, the chief of the tubercular diseases, has been looked upon as non-contagious, and its unfortunate victims have freely mingled with their family connections and with the people at large. But let these views be adopted by the medical profession, and generally received, and it will be imperatively necessary to keep them away, in great measure, from their fellow creatures, and to subject them to the same regimen as we use in the rest of the contagious diseases; and it is no small augmentation of their calamity that this regimen will require to be continued, not, as in other contagious diseases, for a few weeks, or months at most, but, in some instances, for many years.

Observing that you have transferred Professor Tyndall's letter, which appeared in the *Times,* to the pages of your magazine, I ask you to allow me to state a fact in connection with this subject which Professor Tyndall appears to have overlooked; and a fact which ought to make us cautious in accepting Koch's views on pulmonary consumption without requiring for them further consideration and research.

The fact I refer to is this,—that pus, brain, cheese, and other substances, when inoculated, will set up morbid processes in various organs, which cannot be distinguished from those which are set up by the inoculation of tubercle itself. If, therefore, pus, cheese, brain, &c., and tubercle itself produce the like results on inoculation, how can that which tubercle produces be specific? And, further, if *bacilli,* such as have been observed by Koch in tubercle, be also found in the rest of these products of inoculation, then, in all probability, they are effects, and not causes.

And finally, may not all the results obtained by Koch be due to a septic matter introduced along with his *bacilli?* If this be so, then the results obtained are *pyemic,* plus *bacilli,* now discovered, it appears, for the first time, and they are curious and interesting, but I fear they are nothing more; and, at least, they are far from being proved to be the *vera causa* of pulmonary consumption.

WILLIAM DALE, M.D., Lond.

PROBABILITIES.

[401]—In your last article on "Probabilities," you discuss the fallacy contained in the assumption that A must win in the long run his original stake, provided that each stake is double the previous one, and I think the fallacy of such an assumption is sufficiently clear; but in the scheme I here submit, the fallacy, although just as real, is not just as clear to me, and I should be glad if you would make it so.

Make three columns, one for winning, two for losing, thus:—

W.	L.	L.
	1	
	3	
	2	
	1	
	2	2
	3	3
5	4	4
6	4	4
6		
3		

Write down at the top of the second column the number you wish to win in as many parts as you please, thus, you desire to win £7. Write 1, 3, 2, 1.

Commence by staking a small figure, say £1, and, say you win, continue to stake small until you lose. Make no entry of your first winnings, but enter your loss in both losing columns. Say you have lost a stake of £2. Your next stake is the sum of the top and bottom figures, £3. You lose; enter the loss as before. Your next stake is £4; enter as before. Your next stake is £5; you win. Enter this in the winning column, and cross out the top and bottom figures of the centre column. Your next stake is the sum of the top and bottom figures not crossed out in the centre column, or 3 + 3 = 6. You win this, and enter the 6 in the winning column. Proceed as before, your

next stake being £4, you lose, and enter in both losing columns. You next stake (4+2) 6, and win. Proceed as before, and stake 3; if you win, your scheme will have completed, as it were, one revolution, as *all the figures* in the centre column are crossed out; and your winnings are £20, against £13 lost, leaving you £7 to windward, as sailors say. If you lose, you must go on staking as before, until the figures in the centre column are crossed out, when you will be the winner of £7, *in every case!**

It is obvious that instead of 7, *any number* may be used, and may be divided into the greatest possible number of whole parts.

N.

* This is exclusive of what you gained before you began to lose.

INTELLIGENCE IN ANIMALS—JUMBO.

[402]—As another proof that we are not the monopolists of intelligence in the animal world, it is reported from America that just before Jumbo reached the truck in which he was to be confined, some planks, two or three feet high, had to be crossed. Before proceeding, however, he thumped them hard with his trunk, and then pressed them with his fore feet. Why should this reasoning in a brute be called instinct, when with us it would be called common-sense?

HEWITT.

THE LUMINOUS MIXTURE OF BLUE AND YELLOW.

[403]—Professor Rood's observation is not silly, as Colonel Ross states, but incorrect—a fundamental error made by Brewster.

Colour is a sensation bearing no resemblance to its physical cause. To mix colours is merely to excite simultaneously the same parts of the retina with the stimuli, which are known by experience to excite severally the sensations of colour it is desired to combine. Lambert's method obviously accomplishes this object. Other methods are: by rotation on the colour top; by simultaneous illumination of a white object, as with the double magic-lantern. Moreover, elaborate apparatuses have been devised for the luminous mixture of the pure rays of the spectrum. All these methods lead to the same results, which are accepted by all who have studied the subject, and are really as well established as any scientific facts.

To explain Colonel Ross' results would require a special investigation of the circumstances of each case. I would remind him, however, that, in order that blue and yellow may combine to form white, the blue must be exact in hue; the slightest leaning towards green will impart a green tinge to the result, which, in the flame method, may seem very conspicuous.

In regard to the last experiment—

(1.) The explanation cannot be true. The idea that the mere concentration of the beam can affect its refrangibility is quite contrary to what we know of physical optics.

(2.) If it were true, it would not prove the point. It would prove only that the complementary colour of a red formed from the extreme red rays is green, as indeed it is. If, however, Colonel Ross insists on the *grass* green, I think he is at issue with accepted facts on another point.

JOHN TENNANT.

DOES THE MIXTURE OF BLUE AND YELLOW MAKE GREEN LIGHT? FLUORESCENCE.

[404]—Letter 388, in KNOWLEDGE, for April 28, proves what difficulty is caused (in treating of colours) from the want of definite names and an invariable standard of reference. Lieut.-Col. Ross shows that by common usage, the term "blue" is applied to the chloride of copper flame; and the same might be said of the Bunsen flame. In the same way Newton gave that name to the colour which follows green in the spectrum, and called the deeper blue, which comes next, "indigo," and the still deeper and darker blue, which closes the series, "violet." Yet Newton's indigo, which is exactly the hue of artificial ultramarine (or French blue), is really a purer blue, differing from his other blue in containing less mixture of green; and Newton's violet, which is commonly supposed to have a tinge of red, and which some even term "purple," has been proved by most accurate observations (when viewed by itself) to be absolutely free from red, and from any appreciable quantity of green. When the flames above referred to are viewed through the prism, it is seen that they give out a large quantity of green light as well as of pure blue. When the blue part is neutralised by the addition of a yellow light, as in the experiments referred to in Lieut.-Col. Ross's first letter (p. 496), the greenness still remains, and appears in the resulting colour diluted with white.

The following most beautiful and instructive experiment, however, settles at once and for ever what is the colour which, when added to yellow, makes white; and it teaches far more than any experiments with coloured flames or pigments can teach. Lay two rectangular pieces of white paper so as to touch cornerwise over a dark cavity, the edges of the touching corners being in the same two cross lines, and in the same plane. Then view through a prism, held parallel to one of the cross lines, the spectra of the two white spaces. On one side we have the series of colours—red, orange, yellow—formed by combinations of the prismatic rays beginning at the red end of the spectrum; on the other side, the series formed by the rays wanting in the first—sea-green, sea-green-blue, and blue (the best and purest blue that ever the eye can behold). And in these two series the opposite colours must be perfect complementaries—the sea-green to the red, the sea-green-blue to the orange, the blue to the yellow, as may be seen by causing one piece of paper to slip forward so as to pass the corner of the other piece, when the overlapping colours all turn to white.

It is easy, therefore, to see that when we inquire whether mingled blue and yellow lights make white or green, we must distinguish between the different colours which are commonly called blue. As to the binary compound of green and blue, the complementary of red (which is exactly the hue of fresh verdigris powder), it is unfortunate that we have no good distinctive name for it. I have not met with a better term than "sea-green"; which term is certainly not derived from the appearance of yellow sand seen through green sea-water, but is the colour of a peculiar reflection of the sky from the sea—a tint hardly ever seen in nature elsewhere, except in a few minerals, and sometimes in the sky itself.

I beg pardon for so hastily suggesting a mistake in the experiment described by Lieut.-Col. Ross in his Fact 4. The green light he saw in the prism, when the focus of his lens was thrown upon it, was no mistake, but is a beautiful, and to me quite new example of the fluorescence so ably expounded by Professor Stokes in the *Phil. Trans.* for 1852. Glass is known to partially obstruct the invisible ultra-violet rays; but I do not recollect seeing it noticed before that this is attended with the production of a green luminosity in the glass. If the converging rays are made to fall on the flat end of a prism, so that the focus is formed in its middle, the luminosity may be seen extending from end to end of the prism, accompanied with two parallel reflections, more easy to understand than the reflections which complicate the experiment described on p. 496.

A very beautiful and bright sea-green-blue fluorescence is produced when a sunbeam is made to converge to a focus in a weak infusion of the bark of the ash or the horse-chestnut, or a weak solution of quinine; and in these cases the peculiar luminosity is strong enough to be seen in the fluid when the sun shines upon it, without the aid of a lens to intensify the incident light.

W. BENSON.

THE POTATO.

[405]—In No. 24, p. 520, "F.C.S.," replies to my letter in No. 20. He takes me for an English farmer. I have the pleasure of informing him I come from the North, and know both the practice and theory of my profession. I agree with "F.C.S." that we get many hints from our Continental neighbours, but at the same time deplore the fact that most members of "F.C.S.'s" body think they know farming, whereas very few of them can talk of farming matters without immediately saying something which every practical man knows to be nonsense, and when the practical man sees him talk nonsense on what he knows about, he very naturally concludes he also talks nonsense when he goes so deep into the subject that he cannot follow him.

With five experiments during 1879, 1880, 1881, cut and uncut flowers were as near as possible identical, viz., 8 tons 15 cwt. per imp. acre, on an average.

What I meant about the frosted potatoes was this: take say two samples of frozen potatoes from the same heap, cook one in the ordinary way, by boiling; roast the other in hot wood ashes; those from the latter will be good food, the boiled ones too sweet to be palatable, so that I say it is the frost coming out which makes the change, not going in.

I have tried many times most manures, pure, single, and compound on many crops, both on sand and clay. I see "F.C.S.'s" experience is second-hand, although not much the worse of that.

Among most agricultural experimenters, it is generally acknowledged that the presence of a large quantity of decomposing organic matter increases not only the crop, but also the percentage of disease. Now, "F.C.S.'s" experiment being on sand, there was likely to be little organic matter present, or if so, it was not a suitable station, and had it been on moss, the percentage of disease would also have been small; therefore I cannot see but that there is a contradiction in saying that where we have organic matter, and, of course, expect a large crop, that there we have most disease, and again in the unmanured plot with no organic matter and a small crop, that it is also the worst with disease. At least such I take to be his meaning.

All other things being equal, it may be taken as settled that the percentage of disease in the unmanured plots will be the smallest, or nearly so.

"F.C.S." gives no proof of his assertion that "good peat is equal to stable manure." I again say it is nonsense, and wonder anyone signing himself "F.C.S." should have uttered it.

This discussion was commenced in KNOWLEDGE, and unless the Editor objects, another paper is not required to finish it, but if "F.C.S." has been a regular reader of the *Field* in past years, he must have seen many of my contributions on the same subject, but with a different signature.

Would "E. W. P." advocate an excess of mineral manure so as to increase the starch in potatoes, and does he know if such an excess would also increase the starch in the cereals? FARMER.

AN ARTIFICIAL MOON.

[406]—The moon question having been somewhat prominent in your columns lately, a trial of the following experiment may prove interesting to some of your readers. I venture no opinion as to the connection in cause of the strongly-marked resemblance between the real moon and the artificial.

Take a soup plate, and slightly grease the surface with lard or oil; distribute irregularly in varying thicknesses about a tablespoonful of so-called granulated citrate of magnesia. Take a basin, pour in enough water to fill the soup plate; shake into the water about two-thirds the quantity of fine freshly-burnt plaster of Paris, which will sink at once; pour off nearly all the superfluous water; stir two or three times with a stick or spoon, so as to mix irregularly the paste; then pour it on the powder in the soup plate. The water in the plaster will cause an immediate disengagement of carbonic acid gas, which will rise in bubbles of various sizes through it in irregular patches; the plaster almost immediately setting, the shape of the outline of the bubbles and the walls of them become fixed, and, as a result, a most startling resemblance to the cratered surface of the moon is produced.

If a photograph of this be taken with a strong light, the resemblance becomes so perfect as to deceive almost all who are not professional astronomers. I believe that a little sugar, or syrup, or gum in the water would produce larger craters, but I have not tried this. A. STEWART HARRISON.

[As we have for several years used illustrations of the moon's surface formed by Mr. Harrison in the way described above, we can vouch for the accuracy of his statements.—ED.]

"THE STARS AND THE EARTH."

[407]—In your notice of the new edition of "The Stars and the Earth," you remark that the author is unknown. Many years since, two papers "On Good and Evil" appeared in *Macmillan's Magazine*. The editor stated that those papers were by the author of "The Stars and the Earth," Dr. Felix Eberty, of the University of Breslau. In the Catalogue of the British Museum, both the English and German edition of the book are under Dr. Eberty's name, and as the two papers in *Macmillan* display the same ability as is manifested in the book, the common authorship may be regarded as certain. J. W. F.

SIDEREAL TIME.

[408]—The following neat method of ascertaining (without an ephemeris) the approximate sidereal time at noon for any day of the year, may be useful to such of the readers of KNOWLEDGE as observe in the day time and have no rule of thumb way of finding sidereal time. It was communicated to me by Alfred Fryer, Esq., of Elm Hirst, Wilmslow, with whom, I believe, it was quite original.

From the month and day increased by twelve months when necessary substract three months twenty-two days, call each month of difference two hours, and each day four minutes. Thus for the day of issue of KNOWLEDGE, May 5—

5 months 5 days—3 months 22 days = 2h. 52m. sid. time.
Nautical Almanac shows .... 2h. 53·1m.

OCEAN.

THE First Volume of KNOWLEDGE will be published early in June next, bound in red cloth, gilt lettered. Price 10s. 6d. Vol. I. will comprise the numbers from the commencement (Nov. 4, 1881) to No. 30 (May 26, 1882). As there is only a limited number of copies, the Publishers advise that orders should be sent in without delay, to prevent disappointment.

Binding Cases for Volume I. will also be supplied, price 1s. 6d. each. Complete copies bound (including case) for 2s. 6d. each.

Answers to Correspondents.

**** All communications for the Editor requiring early attention should reach the Office on or before the Saturday preceding the current issue of* KNOWLEDGE, *the increasing circulation of which compels us to go to press early in the week.*

HINTS TO CORRESPONDENTS.—1. *No questions asking for scientific information can be answered through the post.* 2. *Letters sent to the Editor for correspondents cannot be forwarded; nor can the names or addresses of correspondents be given in answer to private inquiries.* 3. *Correspondents should write on one side only of the paper, and put drawings on a separate leaf.* 4. *Each letter should have a title, and in replying to a letter, reference should be made to its number, the page on which it appears, and its title.*

PHINEAS FOGG. Rat her too many queries, now that the query column has fallen. There will be 30 numbers in Vol. I., and as many in Vol. II., after which the volumes will each contain 26 numbers—two volumes to each year. This has been arranged in response to the request of several correspondents, that the first numbers of volumes should fall, hereafter, in first weeks of January and July. "Is Mr. Grant Allen a follower of Darwin?" Why, certainly,—being a man of science. The east wind is supposed to affect the health and spirits, because too dry; I cannot say I am satisfied with the reason, but the effect of the east wind is, unfortunately, not doubtful.—C. H. WINGFIELD. That my little joke about Mr. Proctor should have been misunderstood was fortunately of no moment; but the lesson is worth noting, as a misunderstood joke may be mischievous. I will venture to say not one American out of ten thousand would have misunderstood me.—H. C. Astronomers know nothing of the inclination of the asteroids' poles. Noise heard when shell is put close to the ear is generally supposed to be due to the warmth of the face, and consequent air currents. The explanation is doubtful. Other questions answered in elementary treatises.—H. BRAIN. There is every reason for believing that no former nations possessed anything resembling our modern knowledge of mathematics. The history of the progress of mathematics would fill many volumes of KNOWLEDGE.—J. F. G. Persons who have been born blind and afterwards received sight have been found to require several weeks' experience, touching and feeling objects near them, and so forth, before they acquire the power of distinguishing the relative sizes and distances of objects.—ZION. Do not know of any such collection.—W. H. M. There is not any difference between tossing a million coins at once, and tossing the same coin a million times running, so far as the antecedent probability of the relative number of heads and tails is concerned; but, of course, in tossing the same coin a million times running, the observed result of the earlier tossings modifies the expectation as to the final result. A portion of that which had been doubtful has become certain. You say that at the start, in tossing a coin 100 times, it is likely there will be 50 heads and 50 tails. On the contrary, this exact equality is unlikely. Again, you say if the first toss is head, it is 50 to 49 that the next toss will be tail. On the contrary, the next toss is as likely to be head as to be tail. The mathematical chance for the result of many trials is considered in this week's KNOWLEDGE.—V. WHEELER. Yes; but with similar requests for weekly instalments of ten or twelve different subjects, what are we to do?—STELLA. No one else appears to have seen the two large red spots which remained visible to you from 10.5 to 10.15 p.m. near Alpha and Beta, Ursæ Majoris, on the 24th inst. Are you sure there was no optical illusion? Were your eyes tired? or had you been looking at two dark green masses?—J. A. O. In treating of special subjects, writers can name the books which they recommend. Letters asking for the names of books on particular subjects should be sent to writers who treat of those subjects. Science knows nothing about the colour of Adam.—H. J. IVERSEN. (1) When a carriage is turning round a corner, the inside wheel tends to rise off the ground, because the body of the carriage has a centrifugal tendency outwards from the centre round which for a moment the carriage is turning. (2) In railroads, the outer rail at curves is raised so that this centrifugal tendency may be overcome by the tendency of the carriage to lean over towards the side on which the lines are lowest. (3) A turning carriage raises the inside wheel off the ground when the velocity is such that the moment of centrifugal tendency exceeds the moment of the carriage's weight around the lowest point of outer wheel. Thus, suppose M the mass of the carriage, h the height of the centre of gravity above the ground, v the velocity with which it is travelling (in feet per second), d the distance between the wheels, R the radius of the circle in which the centre of gravity of the carriage is moving. Then the moment of the weight round the outer rail or lowest point of outer wheel, is $\frac{Mgd}{2}$, the centrifugal tendency is $\frac{Mv^2}{R}$, and the moment of this tendency round the lowest

point of outer wheel is $\frac{Mv^2}{R}$. Thus the limiting velocity is deduced from the equation—

$$\frac{Mgd}{2} = \frac{Mv^2}{R}$$

$$\text{or } v^2 = \frac{gdR}{2h}$$

This is, of course, only a rough treatment of the problem, obtained by regarding the carriage as if its whole mass were at the centre of gravity. It shows that for a given velocity the stability increases with the distance between the wheels and the largeness of the turning circle, and diminishes with the height of the centre of gravity above the ground. (In the above, $g = 32{\cdot}2$.)—E. D. G. Thanks; your method, and any common-sense method, is better than the ordinary rules for dividing decimals.—K. H. Nisbett. (1.) The moon does occult stars continually; but the Nautical Almanac only gives the occultations of certain catalogued stars. (2.) The stars in the field of view of a telescope are not so crowded that the new moon would seem like a dark circle on the starlit background, even if the new moon were black, which, however, is not the case.—E. D. Archibald. Many thanks for your letter and paper. There can be no doubt on the subject. I should be glad to hear from you at your convenience.—H. Clift. Many thanks; but we have not space.—S. Hopkinson. I hope your bet was only imaginary, though, it made, it was certainly won. The question is, If the odds are the same on two independent events, and the betting is even that both happen, what are the odds on each separately? Let the chance of each be x, then the chance that both will happen is x^2; but this, on the supposition that it is even betting, is one-half, or $x^2 = \frac{1}{2}$, and $x = \frac{1}{\sqrt{2}} = {\cdot}707$ approximately. The chance of each event being 707-1,000ths, the odds on each event separately, are 707 to 293, or roughly 7 to 3.—W. Watson. You object to severity, and probably to unfairness; and to illustrate our fairness and gentleness you characterise my answers to correspondents as a weekly dose of sulphuric acid, and contrast them unfavourably with those given in a contemporary paper. Now, I have taken the answers given under this heading in the last five numbers of Knowledge, and I find that roughly only about one answer in twenty is severe at all, ten or twelve neutral, and the rest useful replies to sensible queries. I have not here counted forty or fifty replies which, in my anxiety to meet the requirements of readers, have been obtained from experts, and paid for at the same rate as original articles. About one answer in forty since Knowledge first appeared has been *really* severe; that is, not meant for banter, such as any sensible person would either take good-humouredly, or answer in kind. I venture to say that every really severe reply has been more than merited. I have no fault to find with the answers in the journal you name; but I happen to know that the paradoxical and the idiotic, the sour-tempered and the malicious among the correspondents of that journal were long since weeded out by treatment far severer than any applied in Knowledge, greatly to the gain of that journal and its readers. When the same work has been done (more easily, I trust) here, our answers to correspondents will no longer need even that occasional drop of diluted sulphuric acid which your imagination has modified into pure and pervading vitriol.—School-Boy. Thanks (you write a very bold hand for a school-boy). You are quite right; in our haste—for be it known that answers have to be very hurriedly written or not at all—in the "fourthly" example at foot of second column of p. 576, we *did*, as you say, shift the decimal point different ways, instead of the same way. The quotient, as you say, has only two integral digits. Good boy!—Gradatim. You are right. Professor Huxley's remark that mercury expands more than the glass holding it, and that *therefore* the barometer is higher on a hot day, is unsound. The size of the tube in no way affects the height of the mercurial column. The diminution of the specific gravity of the mercury is, of course, the true cause of the greater height, *cæteris paribus*, on a hot day. His remark at p. 355, that the earth moving more rapidly as it approaches the sun, neutralises any augmentation of heat which may be due to increased nearness to the sun, is even more obviously incorrect.—Cogito. Yes; the word centrifugal (p. 565, col. 2, line 12 from the bottom) should obviously have been centripetal; as the "copy" was printed matter, the mistake should not have occurred.—H. E. Kilby. Hipparchus first suggested that method of determining the sun's distance. Theoretically sound, it fails utterly in practice, as explained in the first chapter of my treatise on the sun.—M. E. Benham. We must content ourselves with notices of men and women who have in some way advanced scientific researches.—C. Moon. Kindly send the trigonometrical method; if you will also send the "fly," I will try to get the information you require.—W. S. S. S. Place of Uranus can be determined from the zodiacal map in Part IV.—W. S. D. (Chronological.) Your Association cannot teach astronomers anything about eclipses, past or future; it is evident, on the contrary, that you have much to learn. Your views about eclipses, transits, &c., are such as beginners very naturally form.—Intro-Geographicus. No, they do not correspond; if the celestial sphere were a plane, they might. The outlines of constellations have no fixed positions like those of continents and seas; but are carried along so as to include the stars shown in their proper constellations.—M. J. B. "Norma" must be the constellation you refer to; it was invented by Lacaille.—C. J. Caswell. Thanks for kindly words. We note your question about Venus in daytime.—J. B. Question recently answered (in Answers to Correspondents).—E. W. White. The subject is one of great interest; the influence certain; but that there is any magnetism in the matter more than doubtful.—J. McGregor Allan. Thanks, but it has been already done in our columns.—E. Semment. We know of no treatise on the Flora of Singapore.—J. O. Do not know who is the publisher of J. R. Young's "Solutions of Cubic and Biquadratic Equations."—J. Hamer. The polar axis of a heavenly body is that axis on which it rotates, and the equator is that great circle of the body which is at right angles to the axis of rotation. Was not my note of admiration, then, justified by your remark that the moon, instead of rotating on its polar axis, rotates on an equatorial one? It is a contradiction in terms. If you had said, as I think you mean, that the moon rotates on an axis directed earthward, I should have understood you. You might then simply have said the moon's polar axis is directed towards the earth. The idea is erroneous, but not self-contradictory, like the other. The moon's axis is very nearly at right angles to the plane of her orbit round the earth, and she rotates in it once in each revolution round the earth.—Bruce Cook (1). The angle ARB exceeds APB by the sum of the angles PAR, PBR (this follows obviously by joining PR, producing and applying Euclid I., 16). Similarly the angle ARB falls short of the angle AQB by the sum of the angles QAR, QBR. Hence, since angle PAR = QAR, and PBQ = QBR, we get, by adding, twice the angle ARB = angle APB + angle AQB or is constant.—Q. E. D. (2). For action of syphon we must refer you to elementary treatises. (3). If n is even, its square root will contain $\frac{n}{2}$ digits. If n is odd, its square root will contain $\frac{n+1}{2}$ digits.

Both cases are included in the formula.

$$\frac{1}{2}\left\{n + \frac{1}{2} - (-1)^n\right\}$$

$$\text{or } \frac{1}{4}\left\{2n + 1 - (-1)^n\right\}$$

BIOLOGICAL.

Yang fottin (Aspull, near Wigan. Why don't correspondents at least write their pseudonyms plainly?). 1. There are no perceptible morphological differences between man's red blood-globules and those of the higher apes: indeed, no physiologist can venture to swear in a court of law which is the blood of a pig and which that of a man—so close are the resemblances. Differences there may and must be, but they are those dependent on minute structure, and not on any broad lines of bodily conformation. To all intents and purposes the blood globules of man and higher apes are similar in size and structure. With the exception of the Camelidæ, all quadrupeds have round, uncleaved blood globules. 2. So far as we know, the higher apes coincide nearly with man in their gestation. 3. Regarding the occurrence of puberty in the higher apes, we still require exact information. 4. The tail in the highest apes is relatively as rudimentary as in man. On *à priori* grounds, we should presume that in the embryo of a tailless ape the tail might persist longer than in the human embryo, but no exact observations have been yet made on the gestation of apes. 5. Man's teeth have no break or interval (a peculiarity shared in by the little lemur, *Tarsius*), whereas in the gorilla, for example, there is a break. The *number* is the same, and the arrangement similar in man and the gorilla; and even in a baboon, the same number and arrangement exist; but in both gorilla and baboon there are to be seen differences in the pattern and shape from those of human teeth. The dental formula of all the old world apes is, in fact, the same as that of man: 4 incisors, 2 canines, 2 premolars, and 3 molars in each jaw.

Obituary.

WE regret to have to announce the death, at the early age of thirty-two, of a promising young science teacher and lecturer, Mr. Thomas Dunman, Lecturer on Physiology at the Birkbeck Institution, and Physical Science Lecturer at the Working Men's College. His early education was limited, but his reading was wide and his memory remarkably retentive. It was always in spite of his surroundings that he went on adding to his stock of knowledge. About seven years ago he took charge of the Physiology class at the Working Men's College in Great Ormond-street, where his class was one of the largest and most popular in the College. The practical results shown by the examinations at South Kensington attested the thoroughness of his teaching. Like success attended him in other courses of lectures in other branches of science. At the Birkbeck Institution, where he succeeded Dr. Aveling as Physiology Lecturer, his work was much appreciated.

In 1879 he published a very useful glossary of "Biological, Anatomical, and Physiological Terms," and four of his lectures had appeared, "The Mechanism of Sensation," "The Starlit Sky," "Prehistoric Man," and "Volcanoes and Coral Reefs." He contributed to Cassell's "Science for All," to Ward & Lock's "Universal Instructor," "Amateur Work," and several other publications.

He married early, and the effort to support his family by science teaching and lecturing may fairly be said to have cost him his life. During the past two years there were warnings that his energies were being too strongly taxed, but they were unheeded, and at the beginning of the present year brain troubles became markedly apparent, and he was obliged to give up work; but it was too late. He gradually grew worse, and died on the 9th inst., leaving a widow and two children, for whom he had been unable to make any provision.

Our Mathematical Column.

THE LAWS OF PROBABILITY.

By THE EDITOR.

AS an illustration of the rules established in our last, take the following:—

There are in a bag three white balls and seven black balls; a ball is drawn at random and replaced, and this process is repeated five times; what is the probability that at least two white balls will be drawn?

Applying the rule, we must suppose $3+7$ expanded by the binomial theorem to the power 5, the complete expansion being thus written:—

$$3^5+5.3^4.7+10.3^3.7^2+10.3^2.7^3+5.3.7^4+7^5$$

then the fraction obtained by writing the first four terms of this expansion over the whole expansion represents the chance than at least 2 white balls will be drawn. The whole expansion is, of course, equal to 10^5 or 100,000; and the sum of the first four terms is easily found to be 47,178, so that the required probability is $\frac{47178}{100000}$, or nearly one-half.

It need hardly be remarked, however, that the practical application of this rule is not always quite so easy as in the above instance. Tables have been constructed for the determination of approximate values when $n+m$ is large and direct calculation out of the question.

Of course, the chance that at least two black balls will be drawn is given by taking the *last* four terms of the expansion for numerator. In this case the calculation is even easier than in the former, though it would be less easy if the student proceeded directly to calculate the value of the four terms, and then to add them together. There is no occasion for this, however, for he knows that the total expansion of 10 to the power 5 is 100,000, and he has only to deduct from this the sum of the first two terms—that is, 3,078, leaving 96,922. The required probability is therefore $\frac{96922}{100000}$, or more than twice as great as that of drawing at least two white balls.

It is easily seen that by precisely such reasoning as we have used to establish the law discussed above, we can obtain the following law:—

If at each trial there are $p+q+r$ possible results, all equally likely to occur, of which p are of one kind, q of a second, and r of a third, then the chance that in $(n+m+l)$ trials n are of the first kind, m of the second, and l of the third, is represented by the fraction—

$$\frac{|\underline{n+m+l}}{|\underline{n}\;|\underline{m}\;|\underline{l}}\cdot\frac{p^n\,q^m\,r^l}{(p+q+r)^{n+m+l}}$$

Here, too, as in the former case, the expression for the probability is divisible into two parts: a denominator, the expansion of $(p+q+r)$ to the power $(n+m+l)$; and a numerator, the term of this expansion involving $p^n q^m r^l$. And if we require the probability that at least n of the results will be of the first kind, and at least m of the second, we must for a numerator add together all those terms in the expansion of $(p+q+r)$ to the power $(n+m+l)$ which involve p^n, p^{n+1}, p^{n+2}, &c., and *also* q^m, q^{m+1}, q^{m+2}, &c., that is, all terms in which the power of p is not lower than n, and the power of q not lower than m; so if results of the first and third, or of the second and third kind are in question. Of course, if we only require to know what is the probability that n, at least, of the results will be of the first kind, the problem belongs to the former case.

The extension of these considerations to cases where there are four possible classes of result, or five, or more, will be a simple matter to the algebraist. The following example will be more interesting to the general reader than a mere statement of the law; but it will be well to notice that the formula for all such cases bears precisely the same relation to that last given that this formula bears to the former.*

The letters forming the word "Mississippi" are marked on eleven tablets, all similarly shaped, and placed in a bag. A letter is drawn from the bag at random and replaced; and this is repeated twenty-three times; what is the probability that these twenty-three drawings will give 3 *m's*, 8 *i's*, 7 *s's*, and 5 *p's*.

The bag contains 1 m, 4 i's, 4 s's, and 2 p's, or eleven letters all, and the required probability is—

$$\frac{|\underline{23}}{|\underline{3}\;|\underline{8}\;|\underline{7}\;|\underline{5}}\cdot\frac{1^3.4^8.4^7.2^5}{11^{23}}$$

which may be written—

$$\frac{|\underline{23}}{|\underline{3}\;|\underline{8}\;|\underline{7}\;|\underline{5}}\cdot\frac{2^{35}}{11^{23}}$$

and is readily calculable by logarithms.

The value of the probability in this and all similar cases is not changed when the number of possible results of each kind are multiplied in the same proportion. Thus, if the bag contained 20 m's, 80 i's, 80 s's, and 40 p's, we should obtain the same value for the probability above required, as in the case actually described.

When the number of possible results of each kind is very great indeed compared with the number of trials, we get appreciably the same probability whether after each trial matters are restored to their condition before the trial or not. Thus if a bag contain a million red balls, a million white, a million black, and a million green balls, we should get the same probability for the result of twelve drawings (say) whether after each drawing the drawn ball were replaced or not. The difference, at least, is not appreciable. Hence we get the same probability as respects a single trial in which twelve balls are drawn at once, as for twelve several drawings (followed by replacement).

Next week we propose to give the solutions of several problems which have been standing over for some time. Our papers on "Probabilities" will probably be concluded in the two first numbers of Vol. II.

Our Chess Column.

THE INTERNATIONAL VIENNA TOURNAMENT.

[*By Telegram.*] Café Reichsrathpark, Vienna, Tuesday night.

The following is the score of the English players:—

Mason	$5\frac{1}{2}$	Blackburn	4
Mackenzie	5	Steinitz	$2\frac{1}{2}$
Zukertort	$4\frac{1}{2}$	Bird	$\frac{1}{2}$

We have grave apprehensions that Steinitz's health must have broken down. On Friday, when playing with Captain Mackenzie, he overlooked that he could win a piece on his twenty-second move—the consequence being that the game was drawn. On Saturday he fared still worse; he lost to Zukertort. His score then stood at $2\frac{1}{2}$. On Monday he had to play Kruby, and on Tuesday Ware. As

* At p. 319, No. 16, will be found a convenient formula for the expansion of a multinomial to a positive integral power.

is highly improbable that he should have lost to both of these players, we conclude that he did not play at all, or our telegram became distorted in transmission. The best score has undoubtedly been made by Captain Mackenzie, who, although ½ game less than Mason, nevertheless has relatively done best, he having beaten Winawer in the first round, then drawn with Zukertort and Steinitz, and beaten Paulsen. Mason has not encountered any of the best men except Paulsen. It is a curious coincidence in the above score that the youngest man heads the list, while the oldest player stands lowest.

Play began on Wednesday, the 10th inst. Specially noteworthy in the first week is, firstly, the encounter between the two great rival masters, Steinitz and Zukertort. The latter had the satisfaction of conquering his mighty opponent. Secondly, the meeting of Steinitz and Blackburne—the former won. Thirdly, the encounter of Captain Mackenzie, the representative of America, with Steinitz and Zukertort, with both of whom he drew.

It is a matter of astonishment and regret that we do not have daily information of the Tournament in the press. There can be no doubt that thousands of chess players in this country, who follow with eager interest this gigantic tournament, and in particular the fortunes of the English contingent, would be glad to have some information through the daily press. No less than five English players have gone to Vienna. Surely they deserve at least as much notice as a couple of tramps would get who run, hop, or jump around Lillie Bridge grounds.

We have much pleasure in giving a smart game played by Captain Mackenzie against Herr Winawer, of Berlin;—

Game 613.

(Played in the first round of the Vienna International Tourney, May 10, 1882.)

Ruy Lopez.

White. (Captain Mackenzie.)	Black. (Herr Winawer.)
1. P to K4	1. P to K4
2. Kt to KB3	2. Kt to QB3
3. B to Kt5	3. Kt to B3 (*a*)
4. P to Q4	4. P takes P
5. Castles	5. B to K2 (*b*)
6. P to K5	6. Kt to K5
7. R to Ksq	7. Kt to B4 (*c*)
8. Kt takes P	8. Kt takes Kt
9. Q takes Kt	9. Castles
10. Kt to B3	10. Kt to K3 (*d*)
11. Q to K4	11. P to QB3 (*e*)
12. B to Q3	12. P to KKt3
13. B to R6	13. R to Ksq
14. QR to Qsq (*f*)	14. P to KB4
15. Q to B3	15. P to Q4 (*g*)
16. P takes P *en pass.*	16. B takes P
17. B to B4 (*h*)	17. B takes P(ch)
18. K to Bsq	18. Q to R4
19. B to Kt3 (*i*)	19. Q takes B
20. P to Kt3 (*j*)	20. Q to Bsq (*k*)
21. K to Kt2	21. B takes P
22. Q takes B	22. K to Rsq
23. R to KRsq	23. R to K2
24. R to Q6	24. P to B5
25. Q to Q3	25. P to B6(ch)
26. K to Bsq	26. Q to B4
27. R to Q8(ch)	27. K to Kt2
28. Q to Q6 (*l*)	28. Q to Kt4
29. R to Ktsq	29. Q to QB4 (*m*)
30. R to Kt8(ch) (*n*)	30. K takes R
31. Q takes Q	Resigns

NOTES.

(*a*) We do not pretend to decide analytically the merits of this defence, we merely say we prefer P to R3. As a remarkable fact, we have seen a great many games prematurely break down—this defence having been adopted; noteworthy amongst them being one at Berlin, where Winawer defeated Dr. Schmid in twelve moves.

(*b*) This is better than Kt takes P.

(*c*) Had Black played the defence of 3. P to R3, the White Bishop in the usual course would have retreated to R4, and, therefore, after Black's seventh move Kt to B4, that Bishop would be attacked and compelled to move, thus giving Black time. Upon this fact we base our opinion, that 3. P to R3 is a good defence.

(*d*) The Knight is also disadvantageously placed on K3. We think Black might have played P to Q3 instead of Kt to K3; it would have better developed his game.

(*e*) The very thing White wanted; he now brings his Bishop into activity at the cost of Black's time.

(*f*) Now White's superiority is established, R to Qsq is very good; it further weakens Black's Queen's file in combination with the Pawn on K5. Black's defence, therefore, turned out badly.

(*g*) A desperate effort to force his cramped position, but risky, in view of the position of White's Rooks.

(*h*) Overlooking the palpable rejoinder of Black; a waiting move, such as P to KR3, would have done good service to White.

(*i*) In case White should have made an effort to retrieve his lost fortunes by B takes Kt, B takes B, and then retire his Bishop, Black would have a winning check with his B on B5; but through the move in the text White also loses two Pawns, which defence turned out more fortunate for White than could be expected.

(*j*) White relied upon this move to regain the piece.

(*k*) Surely Black had a straight road to victory by Q to R6(ch), and on Queen interposing exchanging. K to K2 would have been too dangerous for White to venture on, after exchanging Queens, and Bishop takes Pawn, Black would be two Pawns ahead.

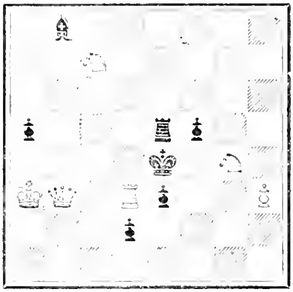
(*l*) White is playing well, and makes the utmost of his attack; while Black is evidently playing carelessly.

(*m*) This loses the Queen; he might have played Q to R5. White could not then have played B takes Kt, on account of Black's reply of B takes B, threatening B to B5(ch).

(*n*) Highly ingenious. Black has no choice. If K to B3, Kt to K4 wins, or if K to R4, R to Rsq(ch), followed by Kt to K4(ch).

Problem No. 42, by J. A. Miles.

BLACK.



WHITE.

White to play and mate in three moves.

Mr. J. A. Miles intends publishing a collection of his "Problems, Poems, &c."—the problem above being one of the number. A selection of such excellent compositions is sure to be favourably received by the Chess public.

SOLUTION.

Problem No. 38, by J. A. Miles, p. 562.

1. P to Kt4	1. K to Q4
2. Kt to Kt2	2. K takes P
3. Kt to B4 mate	
	or if 2. K to K5
3. B to B3 mate	
	or if 2. K to B5
3. Kt to K3 mate	

ANSWERS TO CORRESPONDENTS.

*** *Please address Chess-Editor.*

Muzio 3.—Your best move is 8. B to R3 / Kt takes P, if then 8. Kt takes P / ——, 9. Q to Kt3 / ——, or if 8. —— / P to Q3, 9. Kt to K5 / Kt to K5, 10. P takes P / Kt takes P, 11. R to Ksq(ch) / Kt to K2, 12. Kt to Kt5 / Castles (best), 13. Kt takes BP / ——, &c., (2.) 12. B to Q5 is probably best; thanks for problem; solutions of No. 38 and 39 correct.

Correct solution of Problem No. 39 received from Senex Solitarius, G. W., W. C. Thomas, and Gos.

Edward P. Westlake.—1. Q to B5(ch), K takes Q; 2. QKt takes QP, and 3. P to Kt4 (mate); if 1. K to K2, 2. Q takes P(ch) and mate next move.

Alfred R. Palmer. Games received; will find you an opponent as soon as possible. Solutions of Nos. 40 and 41 correct.

Bright (Genoa). Solution of Problem No. 38 correct.

Leonard P. Rees.—Thanks for analysis, which will receive our best attention; our services are always at your disposal.

H. A. N. Problem received with thanks. Solutions of Nos. 40 and 41 correct.

Henry Planck.—Solutions of Nos. 38 and 39 correct.

G. W. Solutions 40 and 41 correct.

Brenton. Solution of No. 39 correct.

Our Whist Column.

By "Five of Clubs."

Play Third Hand (Plain Suits).

Many players seem to think the only rule necessary for third hand is to play the highest card, unless the suit is headed in the hand by a sequence, when, of course, they do not carry the rule to so absurd a length as to play the highest of the sequence. If to this rule they make one exception, in finessing the Queen with Ace, Queen, they suppose they know all that need be known about third hand play.

In reality, however, play third hand requires considerable judgment, and a thorough knowledge of the leads and of play second hand. In two-thirds, perhaps, of the cases that arise it may suffice to know that third hand should play his highest, unless, of course, he cannot play higher than his partner, or only a card which is the next in sequence above his partner's, when he plays his lowest. If his suit is headed by a sequence, he plays the lowest of the sequence (with the same exception that if his partner's card is higher, or belongs to the same sequence, he plays his lowest, unless he has such strength in the suit that he may with advantage take his partner's trick). But in other cases, the player third hand has to consider the lead, the play second hand, and the score.

Suppose, for instance, your partner has led Queen, and that the lead is original, or at any rate that there is no reason to suppose it forced. Thus, the lead is presumably from Queen, Knave, ten, with probably one small card at least. Then, if you have the Ace and one or more others, third in hand, how should you play if second hand does not cover? You know in this case that second has not the King, and the first idea would be that, since fourth player must hold the King, you should play the Ace. But in general this would be wrong. The state of the score might render it advisable to take the trick lest second round should be ruffed. But usually it is best to let the trick go the fourth player. By putting on Ace, you sacrifice Ace and Queen for one trick, and leave the best card in the adversaries' hands. Apart from ruffing—which, be it remembered, always means a trump drawn from the adversary—the King will make; that is, the adversaries will have one trick in the suit in any case, and it is far better for you that that one trick should be in the first than in the second round. Consider the effect (1) of putting on the Ace and (2) of passing the Queen, apart from ruffing. In case (1) Ace makes first round, King makes second round, and another suit is immediately led—as likely as not the suit is not led again; in case 2, King makes first round, Ace takes the second trick, the suit is probably led a third time by holder of Ace, and in that case two more tricks are made in it, or trumps are forced from the enemy.

Again, suppose ten is led and passed by second player, you know (see our account of the leads in Parts I. and II., or our synopsis of them in No. 14, p. 310), that the lead is from King, Queen, Knave, ten, or from King, Knave, ten, with or without small ones. If, third in hand, you hold the Ace, when ten is led, you put it on, leaving your partner to finesse (if he holds King, Knave) on the return of the suit. If you hold Ace, Knave, you know that the ten is led as a strengthening card; you pass it, and even if the finesse fails, as is probable (for if King, Queen were both with second player the Queen would be put on unless he were long in the suit), you remain with the tenace. If when ten is led you have nothing above it but the Queen, you pass it; for whether it has been led (as is most probable), from King, Knave, ten, &c., or is a strengthening card, the play of the Queen would be bad; in the former case, obviously; in the latter because by playing the Queen you give up at once the command of the suit.

These illustrations suffice to show that the general rule, Third in hand play your highest, is as insufficient as we have already seen that the general rule is for second play, Second in hand play your lowest. We shall, therefore, proceed to consider the play third in hand, first on general principles, and then in detail, as we have already considered the play of the first and second hands.

G. D. Brown remarks on the increased interest of Whist when honours are not counted, and gives the following short way of describing the double method of scoring.

"In addition to the usual score of the games another is kept of the balance of tricks throughout the rubber, which balance is paid for at the end, each trick counting one point in addition to (or sometimes in subtraction from) the points of the rubber."

In play the effect "is that instead of a hand being thrown up when a game is seen to be lost, the hand is played out in order that all the tricks possible may be made."

To the true lover of the game, who desires to see the element of chance as far as possible eliminated, counting honours, especially full honours in short whist, is always objectionable. But for one true lover of whist there are ten who love the excitement better which the element of chance introduces. For my own part, if it were not for the extra time it would involve, I should like to have all the honours of all the suits set together in a group of sixteen, and this set shuffled; the remaining cards being also shuffled; then the two sets being put together, and the cut made without further shuffling, every player would have four honours from among the four suits, and nine plain cards, in every hand. There would then be very seldom those cases of overwhelmingly good or bad cards between two partners which render good play either unnecessary or useless. This, of course, is heresy in the eyes of those who care more for the stakes than for the game, and more for the excitement of chance than for the *gaudia certaminis*.

Five of Clubs.

PROBLEM IV. (p. 506.)

The Great Vienna Coup.

The key to this problem, interesting as having occurred in actual play—though we venture to demur to the statement that the holder of the winning hands said he should make every trick *as soon as he had seen the hands*—consists in forcing the opposite hands to discard from one or other of the suits which seem to be perfectly guarded. A takes out three rounds in trumps, then leads his small trump. If now second player discards either a Spade or a Diamond, there is no difficulty, as he thereby unguards the suit from which he discards. If second player discards a Heart at the fourth round, he equally unguards that suit; but, owing to the position of the other two suits, it would not do for A now to lead a Heart. He must first lead the Ace of Spades, then a Heart, discarding Queen of Spades at the first opportunity. The rest is obvious. All the solutions we have received have been correct—indeed, one of the features of this double-dummy puzzle is that it is not easy to suppose one has solved it when one really has not.

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OUR NEW VOLUME.

TO OUR READERS.

OUR second volume begins with No. 31, and will end with No. 60, closing therefore with the year 1882. Hereafter there will be two volumes in each year. A copious index for the present volume will be published in a few weeks.

We propose in the new volume to continue the same general plan as in Volume I. The columns for queries and replies have been closed, for the simple reason that we could not meet the wishes of the majority of our readers, in other respects, without finding room somewhere; and these columns could be best spared. A large proportion of our letters have, for a similar reason, to be omitted. The reason is as inexorable as the law that two bodies cannot occupy the same space at the same time. Some of our readers have not been willing to take this inexorable reason into account. Others compare their letters with some which have appeared, to the disadvantage of the latter, forgetting that a letter which appears may represent, not one correspondent, but a great number. If we receive ten letters on a subject, we insert the best of the ten, even though it were in some respects inferior to a single communication on another subject.

Many ask us to be more popular, and we shall bear their wishes in view; but there are limits to what we can do in that direction. Others have asked that papers should be shorter, and appear more continuously. This suggestion seems good, and we shall follow it as far as contributors will allow us. The request is really for conciseness.

In the next volume will appear the first of a most interesting series of papers by Miss Amelia B. Edwards, placing beyond dispute or cavil the identity of the Pharaoh of the Oppression. Professor Wilson's "Found Links," a series of papers having special interest just now, will be concluded early in the volume. "Nights with a Three-Inch Telescope," by F.R.A.S., Chemical Papers by Mr. Jago, Photography by Mr. Brothers, Electrical, Geological, Botanical, and Entomological papers will appear systematically. The papers on Probabilities will be brought to a close with a brief discussion of the important subject of indirect probabilities—that is, of the chances, *not* that such and such things will happen, but that, such and such things having happened, the cause was this or that. Afterwards, we shall have a series of short papers on the solution of Geometrical Problems. Whist will continue under the management of "Five of Clubs," who hopes in the next volume to complete his simple explanations of the principal rules for play. We hope (but are not justified in promising) that the skilful players of Whist among our readers (like Mr. Lewis, "Mogul," and others) will, from time to time, contribute problems, interesting positions, &c., for discussion by our Whist readers. The Chess column remains in the able hands of that skilful and courteous player, Mephisto. The chief editor has not forgotten his promise that beside problems there should be illustrative simply annotated games, and discussions of the best lines of opening. In Volume I. the Two Knights' Defence and the Giuoco Piano were fully analysed, and Mephisto hopes in Volume II. to give the analysis of other openings. It may, perhaps, interest our readers to know that three games illustrating the openings already discussed have been in progress for some time past between the Chess Editor and the Chief Editor, and will presently appear, with notes by both players (written independently). We may point with some satisfaction to the early and full news we have given of the Chess Tournament at Vienna.

Early in the new volume we hope to commence papers on "Home Cures for Poisons" (beginning with prevention as the best cure, and pointing to medical aid, as soon as possible, as essential, whatever home cures may be available), on "How to get Strong" (without training for athletic exercises), on Health Resorts, Our Food and Drinks, Cycling (Bi- and Tri-), Swimming, and other subjects wherein a little knowledge is good, and more knowledge better.

We have been strongly urged by many not to continue our effort to keep KNOWLEDGE at its present low price, but to enlarge both the price and the paper. We believe we should be justified, in a mercantile sense, in increasing our price (without enlargement of the paper) in the same proportion in which the only contemporary paper with a similar (but less popular) scope, enlarged its price soon after it was started. But as long as steady growth promises that before long, even at our present very low price, expenses will be met, we shall hearken to no such suggestions. We have wished, and we still wish, to do "our little possible" in the cause of cheap scientific literature. As long as we have our readers' support, we shall keep to this line. We remind them that, if each reader (or if, now, but one in three) were to get but one new subscriber, we could at once, and definitely, reject the suggested departure from our original plan.

SCIENCE AT THE ROYAL ACADEMY.

LET us return to the paintings, leaving the sculptures for another visit. We have also the Grosvenor Gallery to visit, and we are warned that space is limited.

Mr. Arvid M. Lindström's "Winter Landscape from Kollandsö, Sweden" (No. 45, Room I.), shows close study of nature,—not wintry details only are represented, but the general effect is caught; we seem to breathe the cold crisp air; the snow is not white wool merely, as in so many winter scenes, but cold to the touch. There is similarly close observations of nature in Mr. Rickatson's "Autumn Evening" (No. 16). Mr. John Smart's "In the Track of the Storm" (No. 69) is in many respects very fine, but in such a picture as this, which cannot possibly be painted from nature, we seldom fail

to recognise certain incongruities. Only when an artist devotes himself year after year to the study of effects observed during and after storms, training his eye (as the eye can be trained) to take in simultaneous details, and not venturing to combine effects which have not been seen in a single view, can such incongruities be avoided. Now, there are dry storms and wet storms in storm-infested regions, and though the dry storm-cloud might be mistaken by an inexperienced eye for a much-disturbed rain cloud, there are in reality characteristic differences. The storm cloud in Mr. Smart's painting is a wet one; the storm-track is dry, although the wet storm-cloud has but just passed over it. Once more; it is manifest that the painter did not make the storm-track a study immediately or soon after the storm, but some considerable time afterwards. For the most striking feature of a storm-track, immediately after the storm is over, is the staring effect of the rent wood where boughs have been torn off. Whether the wood be white, or yellow, or red, the torn and ragged wood looks intensely bright by contrast with the foliage, or with the dark and time-worn bark. In Mr. Smart's picture this effect is not seen; on the contrary, the broken surface of the wood is shown with such tints as only come after a few weeks', or at least several days', exposure to wind and weather. Apart from this incongruity, and regarding the picture as representing a rainless storm passing over the track of a former storm, the painting is a very fine one, the only fault being that Mr. Smart has either not caught the peculiarities of the dry storm-cloud, or that, intending to represent the storm-cloud which comes accompanied by heavy rain showers, he has not duly represented the effect of such showers in the immediate track of the storm.

In Mr. Graham's "Inflowing Tide" (No. 77, Gallery I.), the sky seems to us unnatural, but the wild effect is well given.

As it is the business of science to try to discover the real nature of things mysterious and perplexing, we have endeavoured to find the true answer to Mr. Henry Vincent's "What is it?" (No. 87). At present, the answer which comes to us is "An exceedingly bad picture"; we should have thought it a school-girl's early attempt, touched here and there by a much-wearied master. The real mystery is "how got the picture in?" Near by is a "Monk of the Order of St. Francis," by Tremayne Lark (No. 88). For some reason the monk is anxious to suggest the idea that he is reading hard; but he is a very bad actor.

We have already referred to one of Mr. Brett's contributions this year—the larger. Of that picture we may simply add, that after the best part of a life passed near the sea, and with special opportunities for studying Cornish sea-scapes, we have never seen a rich blue sea in "the grey of the morning;" and we should be surprised to hear that any one else had: it seems a meteorological impossibility. Of his smaller and, in our opinion, much better painting, "A Falling Barometer" (No. 128), we may note that there is probable incongruity between the heavy, broken sky in one part of the picture and the wine-tinted sea in another. A similar objection applies to Miss Jane Inglis's pleasing picture, "On the North-west Coast of Cornwall" (No. 421, Gallery IV.). But occasionally such combinations are seen. Whether it is wise, or artistically sound, to combine them in a painting, may be questioned. But unfortunately the kind of sky which Mr. Brett paints best, and the kind of sea with which he seems most familiar, are not ordinarily seen together. We note in the smaller, as in the larger painting, that the sea is not level. Under broken lights, we are aware, the surface of the sea often presents a singularly illusive effect of unevenness; but Mr. Brett, in "A Falling Barometer," has done something more than to suggest this peculiar illusion.

In No. 182, "Bargaining for an Old Master," Mr. Henry Woods shows marvellous skill in representing pots and pans. (*Punch* has admirably, and as it were lovingly caught the Punch-like aspect of the principle figure). The Dutch school often introduce pots and pans with great effect; but we know of no painting in existence where there are so many pots and pans, drawn and painted with such exceeding care as in Mr. Wood's picture. They are so well depicted, that the painting ought to find a place in M. Mirobolant's *cuisine*—no one with higher artistic tastes could stand all these culinary utensils, we should imagine; but M. Mirobolant might like them, or even, like the celebrated M. Cavalcadour, "require eight more stew-pans, a couple of braising-pans, eight sauté-pans, six bain-marie-pans, a freezing-pot with accessories, and a few more articles of which I will inscribe the names."

The "Raven Crag," by Joseph Knight (No. 192, Gallery II.), is excellent, thoroughly true to nature from sky to foreground.

In Gallery III. we notice as a singularly fine painting, in which a transient effect is very perfectly represented, Mr. Maurice Page's "Startled" (No. 210). The painting of the startled wild ducks reminds us of Melchior Hondekoeter, but the landscape is better than any of the Dutchman's.

We have already touched upon the incorrect perspective in Mr. Marcus Stone's "Bad News." There is a more serious fault in the expressions of the three faces, which suggest anything but what, we conceive, Mr. Stone wished to represent. For instance, we suppose he did not intend the trooper to look as though he thought, "*This*, I suppose, is the kind of face I should wear till she looks round." But that is exactly how he does look. All three faces wear manifestly assumed expressions.

One of the most charming pictures in the Exhibition is No. 378, Mr. Noble's "Toilers of the Road." The contented look of the animals as they enjoy the cool water in the trough, is admirably true to nature. The donkey is "too sweet for anything," as school-girls say. Some have complained that this picture is hung so high. It is seen best at a distance; but, considering the subject, should have been put lower down.

Mr. John Piggott's "Dread Winter" (No. 387, Gallery IV.) is excellent; but the wintry effect does not go beyond bitterness. The name, "Dread Winter," is suggestive of something more appalling than anything shown in Mr. Piggott's capital picture.

No. 413, Gallery IV., "Inverlochty Castle and Ben Nevis," by Mr. Keeley Halswelle, is admirable, but the clearness with which the Castle in the midground is shown is inconsistent with the aerial perspective. No. 414, "Trimming the Net," by Mr. David Farquharson, is a charming picture, the sky especially true to nature.

Mr. Otto Scholderer, in his "Fine Yarmouth," No. 415, has successfully mastered the difficulty of representing the tint and appearance of dead fish,—so successfully as to suggest serious regret that the difficulty is not greater. If it were but impossible to paint such disgusting subjects! In Mr. J. T. Linnell's "Wild Flowers, No. 419, on the other hand, a very pretty natural effect has been exaggerated, and extended over too wide an area.

Mr. Hermann Smith is writing for *Musical Opinion and Music Trade Review* a series of papers entitled "In the Organ and in the Orchestra," in the course of which many new ideas bearing on the production and appreciation of musical sounds are put forth, and several old beliefs are viewed from new standpoints.

THE TOTAL ECLIPSE.

THE accounts hitherto received give very imperfect ideas as to what has been done, and the telegrams seem to have been despatched with careful instructions to leave entirely unnoticed whatever has been done by some of the most important members of the observing force. We hope next week to have letters from the scientific seat of war. In the meantime, we note that MM. Thollon and Trépied have determined the exact place of the bright line in the spectrum of the corona identified with 1474 of Kirchhoff's scale. Tacchini and Thollon could see no dark lines in the spectrum of the corona, using different dispersions, so that one or other might be expected to have seen such lines if on this occasion (as in the last Indian eclipse) they had been visible. Lines of hydrogen, and many bright lines seen in the spectrum of the corona, were studied by Thollon. The absorption lines observed in Group B (atmospheric) were observed by Trépied and Thollon to be stronger near the moon's edge, which has been described in the telegrams as "indicating a lunar atmosphere." It seems to us rather to indicate the weakening of the sky spectrum there. Considering how many spectra of stars have been observed by Huggins close to the moon's edge without change in the strength of the absorption lines, it would require something more than the comparatively rough observations possible during eclipse to indicate a lunar atmosphere, whether such atmosphere exists or not. Good photographs were taken (we trust Mr. Ranyard met with good success in this direction), and in these *a fine comet close by the sun* is well shown. Let us hope it is not the "*Spectator's* comet." More of this anon.

THE AMATEUR ELECTRICIAN.

ELECTRIC GENERATORS (*Continued*).

IT is probable that some of our readers, in making a machine, will experience some little difficulty in getting a current from it, unless the very greatest care is taken to ensure the perfect insulation of the wire. It would make the machine much more reliable, if before winding the wire, a layer of paper thinly-coated with shellac varnish is fitted and fixed on to the coil of the armature. The wire,

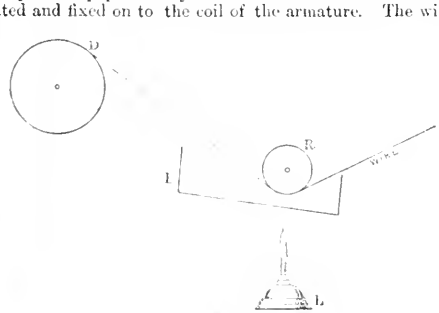


Fig. 4.

too, might with advantage be passed slowly through a little melted paraffin wax, and so be covered with an insulator of the highest quality. This may be very easily accomplished. Fig. 4 will illustrate the arrangement. (D) is the bobbin of wire (as it is received from the manufacturers), and capable of revolving on a horizontal pin formed by driving a pointed piece of stout wire into the side of a common box, or any suitable article. (B) is a small tin box, such as a one pound biscuit-box, containing the paraffin wax (say half-a pound), and a small reel (R) to revolve on an axle formed by fastening a piece of stout wire into the sides of the box. A spirit lamp or Bunsen gas burner (L) is placed under the tin box (B), to keep the wax in a thoroughly liquid state, but not sufficiently hot to produce any considerable amount of evaporation. The armature coil, or whatever else we may wish to cover with wire, may be easily attached to a spindle, so that, by turning it round, we can at the same time pull the wire from the bobbin (B) under the reel (R), and by using the left hand (or the right, if more convenient) as a guide, the layers of wire may be made very regular and compact.*

Those who have the necessary convenience are strongly recommended to measure the resistance of the coil of wire. Our armature should contain about 7 lb. of No. 24 wire offering a resistance of 6·9 or 7 ohms. The chief danger is that of the wire coming into contact with the iron. This is a fault which may be easily detected by using a battery cell with a galvanometer, such as the one described in our first article, in circuit. Take the two wires (one from the galvanometer, and the other from one pole of the battery); connect one to one end of the armature-coil, and hold the other on to the iron itself. If a deflection is obtained, it is clear that a current is passing—that is to say, the wire is bared somewhere, and is touching or making contact with the iron.

Before considering the armature as finished, we must decide on and arrange our "commutator," or current reverser. On to the plain gun-metal cap drive a boxwood or ebony ring† or cylinder half an inch long, tight enough to prevent it slipping, and on to this fix by small screws a brass cylinder cut longitudinally into four sections, so that we shall have four pieces of metal represented in section by A B C D (fig. 5); then join one end of the coil of wire to A and the other to B.

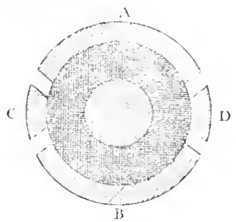


Fig. 5.

Now for the "inductor." In very small machines the magneto principle is preferable to what we have yet to describe as the dynamical. In a purely magneto machine, the currents of electricity obtained from the revolving armature are generated by the inducing force of permanent steel magnets. These should be very strong, each capable of supporting, under the most favourable circumstances, at least 7·5 times their own weight. The Jamin magnet‡ (a Parisian manufacture) is capable of sustaining fifteen times its

* Another method of paraffining is to heat some of the wax in a porcelain evaporating dish, and pass the wire through a loop made in a piece of glass rod, dipping below the surface of the wax.

† These substances are recommended where there is any difficulty in procuring ebonite.

‡ This is simply a compound magnet composed of a number of strips of very thin steel placed one over the other, the surfaces being coated with varnish.

weight, but it is feared that they are too costly for the average amateur. There are, however, several good makers in England, such as Theiler, Blakey, and others, whose magnets may be relied upon to support nine or ten times their weight. Here it would not be out of place to mention that the magnetisation of a piece of steel or iron depends, not so much on the strength of the inducing magnet, as on its own "saturation point" or its magnetic capacity. The purer the iron the higher is its saturation point, but this is obtained at the expense of its power of retention, that is to say, pure iron cannot become a permanent magnet, although it may be temporarily raised to a higher state of magnetisation than a piece of steel or iron containing a very small proportion of certain foreign substances (carbon, &c.).

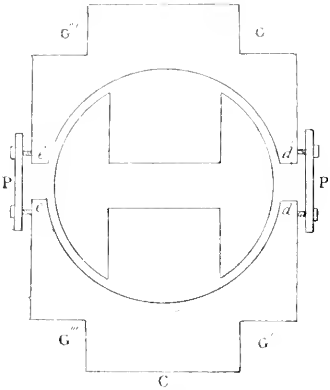


Fig. 6.

Fig. 6 is a vertical section of the "pole-pieces" of our magnets. B and C are two pieces of cast iron (softness not essential, because of their being closely attached to, and always actuated by, permanent magnets), each piece being 4 inches long. They should be cast with a semicircular cavity, the two parts temporarily clamped and carefully bored out to a diameter of about $1\frac{5}{8}$ or $1\frac{3}{4}$ inches, just large enough, in fact, to allow the armature to revolve freely without touching. The edges ($e\,e'$, $d\,d$), should be filed down about an eighth of an inch, brass plates (p and p'), having been previously fitted and tapped, ready for screwing on to the pole-pieces, to render the combination compact. G′, G″, G‴, G⁗ are grooves, into which the magnets are to be fitted, and should be just deep enough to make the top and bottom of the pole-pieces flush with the magnets.

Having secured the pole-pieces, the magnets should be fixed. They should be of the U form, but not too broad, a number of narrow ones put side by side giving much better results. Referring to fig. 6, put all the north poles in G′ and G⁗, and all the south poles in G″ and G‴. To fix them on, place small brass washers top and bottom across the opening between each pair of magnets, and pinch them together by small brass bolts and nuts. We should recommend our readers to make their own magnets if they possibly can, providing also that they can procure sufficiently good steel. The steel should be in strips about a quarter of an inch thick, half to three-quarters of an inch wide, and 9 inches long, bent so as to measure $1\frac{3}{4}$ to 2 inches between the legs of the U. The best way to magnetise a strip, after it has been brought to the desired shape, is to put coils of wire over the legs (in such a way as to form a continuous coil), and pass a current of electricity through the wire for a minute or so, breaking the circuit two or three times during the operation. The magnets should be fitted as close together as the bolts above referred to will permit, but not touching, and should as nearly as possible fill up the length of the pole-pieces. If any difficulty is experienced in this matter, Messrs. Blakey, Emmott, & Co. (Halifax), will supply magnets ready to attach to the pole-pieces at 1s. 6d. each. Ten of these would be required, that is, five on each side. There is a little more to be said, which we must, however, defer till next week.

THE COLD WEEK IN MAY.

THERE is something startling in the placid way in which M. de Fonvielle advanced, as we have seen, the asteroidal theory of the cold week in May is presented. It was never held, even by Ertel, who first advanced it, to be anything beyond a probable surmise, nor has it at any time been adopted by astronomers of standing. But, in reality, the supposed fact on which Ertel originally based the theory, the only circumstance which gave to the theory an appearance of plausibility, has been shown to be no fact at all. It was formerly supposed that the stream of small bodies, to which we owe the meteor shower commonly seen on November 13-14, is nearly circular in shape. Now, one of the points where this stream crosses the level of the earth's track corresponds with the place occupied by the earth on November 13. Half a year elapses between midnight, Nov. 13-14, and May 15, noon; but, owing to the earth's more rapid motion in winter than in summer, she crosses just opposite the place she had held on Nov. 13-14 at midnight, on May 12 about noon. Ertel reasoned that, assuming the November meteors to form a zone of small bodies around the sun, the zone being less in extent than the earth's orbit, this zone must of necessity cross the level of the earth's orbit at a point directly opposite the place of our November encounter with them, and lying inside the earth's track, or between us and the sun. The zone of meteors would therefore intercept a portion of the sun's heat on or about May 11, 12, and 13. The August meteors would, in a similar way, account for the cold spell of February. And though the April "borrowing days" could not be explained by a meteor system giving October displays (for there is no such system), yet the theory was not therefore invalidated. For, of course, a meteor system may lie between our earth and the sun at one part of the earth's annual course without necessarily crossing that course itself at its opposite point. Thus it seemed as though there were very strong positive evidence in favour of Ertel's explanation of these cold snaps, and no negative evidence of weight against it.

This reasoning is often quoted at the present day, when all its weight has departed from it. We now know very certainly that neither the November nor the August meteor systems pass between the earth's track and the sun on any part of their circuit. The November meteors cross the earth's track itself at the point she reaches on or about November 13-14, and of necessity they cross the level of her track again at a point exactly opposite, or lying in the same direction from the sun as the earth does on or about May 12. But, instead of this second place of crossing lying between the earth and the sun, it lies far away in the remote regions of the solar system, near the track of the planet Uranus, or about twice as far from the sun as Saturn, the remotest of all the planets known to ancient astronomers. In fact, it is believed that we owe to the

planet Uranus the introduction of the November meteor zone into our solar system, either through a close approach of that giant planet to the meteor family at that point of its track where it still approaches the path of Uranus, or else from the actual expulsion of meteoric fragments from the interior of Uranus millions of ages ago, when the planet was passing through its sun-like stage. Be this as it may, it is certain that the November meteor system—though at the very time when these words were written it lay in the same direction from the sun as our earth does—yet lies some nineteen times as far away, or, roughly, about 1,650 millions of miles further from the sun than we are at this present time. To charge the November meteor system, therefore, with robbing our earth of a portion of its supplies of solar heat is to imitate the wolf in the fable, who accused the lamb of troubling the stream, though the stream flowed from the wolf towards the lamb. The sun's rays in May pass our earth on their way to the November meteor system, not that system on their way to the earth. It is, therefore, now a matter of scientific certainty that the cold snap in May is not caused by the November meteor system. As for the August meteors, their track has been found to be even far wider, and in February, when Ertel supposed they were intercepting our supplies of solar heat, they lie forty times farther from the sun than our earth.

It does not of necessity follow that Ertel's theory is erroneous; but now that the evidence he seemed to find in its favour from the November and August meteors has been shown to be fallacious, the defects which from the first characterised the theory will be more readily admitted. These are sufficiently manifest. If meteors diminished our supply of heat, it could only be by coming actually between the earth and the sun in such a way that they could be actually visible (if sufficiently magnified with a telescope) upon the sun's face. If they formed a fine cloud of cosmical dust, they would reduce his apparent brightness in a measurable degree if they affected in such degree the supply of heat we received from him; and this has never been noticed. If they were individually large enough to reduce the supply of heat, they ought to be individually visible as black objects crossing his face even to the naked eye, but certainly with powerful telescopes. No such bodies have ever been seen.

However, there is a terrestrial test for the theory by which its validity could be readily determined. If meteoric bodies come between the earth and the sun at any time in such numbers as to make us feel cold in their shadow, they cool the whole earth, not England, or Europe, or the northern hemisphere. If, then, on a careful comparison of the mean daily temperature at observatories all over the world, it is found that the cold snaps of February, April, and May are everywhere to be recognised, then it must be admitted, at least, that the cause of the peculiarity is to be sought outside the earth herself. If, on the contrary—as, for my own part, I suspect would prove to be the case—no trace of these cold snaps could be recognised—say, in Australia and South Africa, though cold snaps at other dates might be noticed—then the peculiarity must be regarded as local. It may extend over the British Isles, or even the whole of Europe, or it may even, though this seems unlikely, prevail over much larger portions of the northern hemisphere; but if not recognised (and that, too, at precisely the same epochs) in both hemispheres, its cause cannot be regarded as extra-terrestrial. It should not be difficult, if the cause is on our earth, to trace it to its source. I should not be surprised if that source were found to lie among the ice-fields of the North Atlantic.

PHOTOGRAPHY FOR AMATEURS.

By A. Brothers, F.R.A.S.

PART VIII.

Landscape photography, portraiture, copying, and various other branches of photography may be followed by the amateur after he has mastered the working details given in previous papers. But there is one branch to which many readers of Knowledge will naturally turn their attention as soon as they can manipulate a collodion plate. Astronomical photography is one of the most fascinating sections of the art-science, and it is one in which it is not difficult to achieve success, always supposing that a good telescope is available. The method of procedure is the same whether the telescope be of the refracting or reflecting form. With a reflecting telescope, there is no difficulty with the actinic focus; but with a refractor, allowance has to be made in a way presently to be referred to. We will describe the apparatus necessary to attach the plate-holder to a refractor. The eye-piece must be removed, and in its place a piece of tubing of the same diameter must be provided. To this tube a piece of flat brass plate must be attached, and the dark slide of the camera may be arranged so that it will slide on to the brass plate and remain secure in any position of the telescope. No camera is required—merely the means of holding the prepared plate. On a plate $4\frac{1}{4}$ by $3\frac{1}{4}$, two pictures of the moon may be taken, merely by shifting the slide about one diameter of the moon, after making the first exposure. With a slightly-varied arrangement of the brass plate and dark slide, four exposures can be made on the same plate. With telescopes up to 6 ft. focal length, the size of plate named will be sufficient; but with larger instruments, a plate 5 by 4, or $6\frac{1}{2}$ by $4\frac{3}{4}$, would be preferable.

The preparation of the plate is just the same as for a landscape or copy. In order to find the focus, a piece of ground glass may be placed in the dark slide, with the ground surface towards the object, and the image of the moon may be brought into sharp visual focus. The focussing tube of the telescope having been drawn out so as to allow some "play" for finding the actinic focus, a slight scratch should be made on the tube to show the point of visual focus, the sensitive plate may be put into the slide and the shutter withdrawn. The cap of the telescope should be removed, and a cover of some opaque material substituted, and made to fit loosely, so that, in making the exposure, the telescope should not be disturbed. The driving clock having been adjusted to the moon's motion, which can only be done approximately, the object glass covered, and the prepared plate in its place, the temporary cap may be carefully removed, and held a short distance off, so as to allow any vibration of the telescope to subside, and the exposure may then be made. The time of exposure must be varied according to the phase of the moon, the focal length of the telescope, and the state of the atmosphere. At the time of full moon, and if the sky be very clear, the time of exposure may be one second or less—experiment alone will determine this point. The first image taken in the visual focus is sure to be indistinct; and now, to find the actinic focus, turn the focussing tube *outwards*—say one-eighth of an inch—and then take another picture, taking care that the slide has been moved forward; at the same time, a mark must be made on the brass tube. Proceed thus, carefully noting the improved sharpness of the image. It is obvious that, if the second picture is less distinct than the first, the plate must be placed nearer to the object-glass. The focus is longer or shorter, according as the object-glass is over or under corrected.

An expenditure of much patience may be necessary before satisfactory negatives of the moon will be obtained, but as soon as the actinic focus has been found approximately, the work on each evening will be more easy; but one or two trials must be made to test the focus, which will vary with the temperature, owing to the alteration of the length of the telescope tube.

In order to show the effect of atmospheric disturbance, a photograph of any bright star may be made—Sirius, for instance. Put the plate in position, and then, the telescope being at rest, allow the image of the star to pass across the sensitive film; on development, it will be found that, instead of a straight line, it will be of zig-zag form. This atmospheric disturbance, of course, affects the picture of the moon, and it is not easy to distinguish between this defect and the incorrect focus. It may be necessary to take dozens of negatives in order to secure one good one. For this reason, there is a great saving of time if the plate be arranged so that four pictures can be taken successively, the times of exposure being varied.

In a future paper, the gelatine dry plate process will be described. This process is very much more rapid than wet collodion, and it may be thought that, for that reason, it should be preferred for astronomical photography; but up to the present time the best photographs of the moon have been taken by the wet process, and so far as my own experience extends, the dry gelatine plate is not so suitable for enlarging—much of the finer detail of the original negative is lost in enlarging by either process, and there can be no doubt that the long-neglected Daguerreotype plate would give better results than any of the more modern processes.

It will be necessary to examine each negative with a lens, in order to judge whether sufficient sharpness of detail has been obtained, selecting the craters near the terminator of the moon as tests.

One of the most curious and interesting results of the application of photography to astronomical observation is in the combination of two pictures of the full moon to be viewed in the stereoscope. By selecting negatives that have been taken in suitable states of the libration of the moon, and by mounting transparencies (in preference to paper), enlarged to the same size, we obtain a picture showing the rotundity of the moon in a very remarkable way.

The diameter of the images of the planets in telescopes of moderate size is so small, that no results of any value can be obtained.

The great value of photography, as applied astronomically, has been in determining, first, that the red flames seen during total eclipses of the sun really belonged to the sun; and, more recently, the much-disputed question as to the solar corona was finally disposed of by comparing photographs taken at stations widely separated.

Reviews.

PROF. HUXLEY ON SCIENCE AND CULTURE.*

Like others of the most valuable works by Prof. Huxley, the book before us is a contribution rather to literature than to science, though the author derives some of his most effective arguments and illustrations from science. The Essays, Lectures, and Addresses which form the volume are gathered from various magazines in which they have appeared at intervals during seven or eight past years. Some critics (mostly those who, being themselves essayists, can find none to publish volumes of their collected papers) take strong exception to the course thus pursued by Prof. Huxley. Such a critic, in reviewing a volume of essays by the present writer, said that to collect and republish essays which have already appeared was the worst possible offence; and another compared the author of such a volume to a highwayman holding a pistol in the form of an octavo volume at the head of an unoffending public. The answer is obvious. If the public does not like such works, the public can and does leave them alone; but if the public finds an author's republished essays worth re-reading, the author is something more than justified in republishing them. It appears —who, indeed, could doubt it?—that Professor Huxley's readers are of this opinion. "I can give no better reason for republishing [these papers] in their present form," he says, "than the fact that three earlier collections of a similar form have been received with favour." And a very sound and sufficient reason it is.

The first paper—an address on science and culture—is interesting for the strong, yet moderate, assertion of Prof. Huxley's claim for pure science as a necessary part of culture. He rejects, on the one hand, the arguments of self-styled practical men (expressing in passing a belief, in which we wish we could share, that "the pure species has been extirpated"), and, on the other, those of certain classical scholars, who consider themselves, as it were, Levites in charge of the ark of culture. He shows very clearly the distinction between the mere Latinism of the Middle Ages and the true classical culture of the Renascence. He pokes a very clever joke at the advocates of merely scholastic training, where he says that "if we were disposed to be cruel, we might urge that they have brought reproach upon themselves, not because they are too full of the spirit of the ancients, but because they lack it." "Modern astronomy," he says justly, "is the natural continuation and development of the work of Hipparchus and of Ptolemy; modern physics of that of Democritus and of Archimedes; it was long before modern biological science outgrew the knowledge bequeathed to us by Aristotle, by Theophrastus, and by Galen." "We cannot know all the best thoughts and sayings of the Greeks unless we know what they said about natural phenomena. We falsely pretend to be the inheritors of their culture, unless we are penetrated, as the best minds among them were, with an unhesitating faith that the free employment of reason, in accordance with scientific method, is the sole method of reaching truth." This lesson from the ancients is, indeed, the key note of the first six of the Addresses, Lectures, and Essays gathered together in the present volume. Scattered through these essays there are numbers of pithy sayings, well worth quoting and remembering. Here are a few of them:—"An exclusively scientific training will bring about a mental twist, as surely as an exclusively literary training." "Knowledge is only the servitor of wisdom" (may this, our Knowledge, be so regarded!) "Do what you can to do what you ought, and leave hoping and fearing alone." "The assertion which outstrips evidence is not only a blunder but a crime." "The Nemesis of all reformers is finality." "When you cannot prove that people are wrong, but only that they are absurd, the best course is to let them alone." (Note that, Mr. Editor of Knowledge!)

The other essays are more specially scientific. In them Professor Huxley deals with the Border Territory between the Animal and Vegetable Kingdoms. Certain Errors respecting the Structure of the Heart,

* "Science and Culture, and other Essays." By T. H. Huxley, LL.D., F.R.S. (Messrs. Macmillan & Co., London.)

attributed to Aristotle, the Hypothesis that animals are automata (a most interesting essay), Sensation, Evolution in Biology, "The Origin of Species" (an essay of somewhat melancholy interest just now), and the connection of the biological sciences with medicine. In all these essays Professor Huxley shows the love of truth, the plainness of speech, the strong common sense, which characterise all his writings, while his profound knowledge of those matters whereon he speaks as one having authority, and not as a (mere) scribe, gives to these writings a scientific value altogether apart from their personal and literary qualities. Every one who wishes to be on a level with the scientific thought of the day must read this work.

SOLAR ENERGY.

By Dr. Siemens.

WHEN communicating to the Royal Society, on March 2, my speculation on the conservation of solar energy, I was aware that I might, perhaps, give displeasure to those who strongly adhere to what may be called text-book information on the subject; I cannot, therefore, feel surprised that Mr. Proctor refuses to accept either my explanation or the mathematical proof by which I endeavoured to establish the fundamental condition of my theory that of solar fan-like action in a space filled indefinitely with attenuated matter. I am bound to admit that in replying (necessarily somewhat hurriedly) to Mr. Archibald's letter in *Nature*, I used the word "moment" where "force" might have been more correctly employed, but with this exception I fully maintain my mathematical statement of the problem.

It is by no means necessary, as maintained by Mr. Proctor in the *Cornhill Magazine* article, that centrifugal force, acting upon the circulating matter, should balance the force of solar gravitation upon the same; it is in effect less than the thousandth part; and yet continuous equatorial outflow must take place. Astronomers have hitherto regarded the atmosphere surrounding a heavenly body as taking part wholly in its rotatory motion, in which case the only effect produced by rotation will be ellipticity, or a permanent rise of the atmospheric column in the equatorial regions unaccompanied by continuous motion. The fundamental difference in my assumption is the surrounding matter indefinitely extended into space, which cannot be supposed to take part in the rotatory movement of the gaseous matter in immediate contact with the rotating ellipsoid. In this case there is no elliptic atmosphere to be maintained in balance by its greater depth in the equatorial regions, and the tendency to rise to a greater height in that direction in order to attain a statical balance, can only result in equatorial motion, or in the circulating current, which I make the basis of my hypothesis.

It would be idle to attempt further argument on this subject; but solar eclipse observations must before long decide the question either in favour of Mr. Proctor or myself. C. W. SIEMENS.

[It is singular to find a man of Dr. Siemens' calibre, when possessed by a paradox (which has happened to Galileo and Kepler and Newton) adopting the tone of our Hampdens, Croslands, and their like, who have always "been *told* that there would be strong opposition to their views." I must confess, too, I am rather amused to find that I, of all men, should be regarded as "strongly adhering to text book science," who have been held utterly wanting in respect for mere authority. On this very subject about which Dr. Siemens has advanced these new views, I have done at least as much as any living writer to dispossess long established ideas,—*only*, having based my views (1) on sound and sufficient knowledge of what had been already done, and (2) on mathematical reasoning, I have seen them, after rather obstinate contest, take their place among accepted truths. Dr. Siemens will find that I was the first to assert, as demonstrably established, the configuration of the solar corona which his theory requires, and which actually exists (though it does not prove his theory), at a time when such men as Sir John Herschel (grand old man) and Sir George Airy held the terrestrial theory of the corona to be admissible.* Apart from what I have endeavoured to establish, myself, with more or less success, outside of text-book astronomy, I have always shown readiness to accept the extra-text-book science of others—where it really is science. If, then, I do not accept Dr. Siemens' explanation, it is because I hold it to be entirely unscientific—though Dr. Siemens is a man of science, and one of well-deserved reputation in his own departments. If I reject what Dr. Siemens calls the mathematical proof of the fundamental conditions of his theory, it is because there is nothing mathematical about it. Apart from the mistake (in one sense quite insignificant, in the other exceedingly significant) in the use of a familiar expression, the so-called proof proves nothing: *it is the mere beginning of a statement of certain impossible conditions.*

The question whether or not the atmosphere of a heavenly body extends indefinitely into space, its outer parts not sharing the rotation of the planet, has no real bearing on Dr. Siemens' views. So far from hitherto regarding the atmosphere as wholly sharing in the rotatory motion, I consider it far more likely that the rotating atmosphere merges into the general atmosphere of space. But the laws of motion remain. A gaseous mass, wherever it may be, is urged sunwards by the sun, and can only fail to tend sunwards when his attraction is counterbalanced by the attraction of some other heavenly body. The tendency, when not thus counterbalanced, may not lead to motion sunwards, because it may be balanced by centrifugal tendency or by elastic forces; but these elastic forces are generated by the solar attraction, and cannot possibly generate constant efflux from him. There is no difference between a cubic foot of gaseous matter opposite the sun's equator but outside his rotating atmosphere, and an equatorial cubic foot of such matter within his atmosphere, so far as the nature of the forces at work is concerned, *except* that the former, having less rotational motion, has relatively less centrifugal tendency. The best Dr. Siemens could do for his theory would be to have all the rotational movement of the sun at work to produce recession—and that would not suffice; yet he makes it a strong point of his reasoning that the gaseous matter which he wants to be continually receding, cannot be supposed to take part in the rotatory motion of the gaseous matter in immediate contact with the rotating ellipsoid.

It would indeed be "idle to attempt further argument on this subject," if the first principles of hydrodynamics are set on one side in the attempt to establish Dr. Siemens' paradox—for paradox it unquestionably is. But eclipse observations will not help to make the matter clearer. The appearance presented by the corona would be practically the same, whether the movements imagined by Dr. Siemens took place, or those movements of cosmical dust, under the action of gravity, which astronomers recognise.

I note that Dr. Siemens does not consider *at all* the equally decisive disproof of his theory (as a sufficient explanation of solar and stellar work) afforded by the shining of the fixed stars. We know that if there is no utilisation of star rays in space, Arcturus and Aldebaran are suns pouring out second by second much more light and presumably much more heat than our sun; Sirius pours out at least 200 times as much; and on the average, every star we see, and every star brought into view by the most powerful telescope, is a rival of ours. If their rays *are* utilised, these stars are, on the average, very much larger than our sun, and only so much is wasted as we can measure—that is, *still*, many millions of millions

* Singularly enough, I had barely laid down my pen after writing my reply to Dr. Siemens, when I read in *Nature* a passage forming a strange commentary on the above remarks. When in 1869 I showed, by mathematical reasoning (which Sir John Herschel and Sir G. Airy both, in letters to me, admitted to be sound and sufficient—as they were, of course, to any mathematician), Mr. Lockyer, of whom it was jestingly said a few months later, that "he called himself owner of half the corona," remarked in *Nature* that it was absurd of me to express an opinion where "even the workers" (meaning himself, I think), could not decide. Though he did decide; for having started a theory of the sun with which the great extension of the corona was inconsistent, he insisted very positively that the corona is, in the main, a terrestrial phenomenon. I was content to wait; but pointed out observations by which the real nature of the corona might be shown (to those who could not follow the mathematical proof), and others by which Mr. Lockyer's more general theory of the solar atmosphere could be disproved. These observations, as it chanced, were made during the next eclipse (the latter by Professor Young). Little by little "the workers," meaning Mr. Lockyer, had to yield more and more of the corona to the sun, until now we find in *Nature*, Professor Newcomb's picture of a corona extending many million miles from the sun, described by Mr. Lockyer as certainly representing a solar appendage. But the amusing thing is, that though Mr. Lockyer's theories are dead, "still in their ashes burn their wonted fires." He thus describes in the *Daily News* the state of scientific opinion which differs so diametrically from what was once his own:—"Certain and sure evidence was obtained that the outer atmosphere extends much further from the sun than had been previously supposed *by those most competent to form a just opinion.*" I would submit that to have formed and maintained for years a wrong opinion is not proof positive of superior ability to form a right one

of times as much energy as our sun pours on all the planets. The difficulty, or rather the mystery, underlying the constant emission of so much wasted light and heat is not removed, unless we are to assume that the distance between the stars and our sun is not sufficient for the utilisation of more than a part, possibly a very small part, of a sun's energy. But even if this were admitted as reasonable, instead of being rejected as obviously the reverse, who that recognises the vastness of the interstellar spaces or knows how many millions of years would be required to draw matter from mid-stellar space to the neighbourhood of even the nearest sun, can for a moment suppose that the matter thus prepared can be used up as Dr. Siemens' theory requires? What matters it, so far as this mystery of mysteries, the mystery of infinite energy seemingly wasted, is concerned, whether solar and stellar rays of light and heat are directly lost (*to our universe*) as they are radiated through interstellar space, or whether they are constantly employed in modifying matter which can never by any chance come (in time to be of use) to *any* star in space?

Considering that the forces conceived by Dr. Siemens have, in the first place, no existence, so that the mechanism of his "perpetual round" could never even be started; that, secondly, the work supposed by him to be done in interstellar and interplanetary space is certainly not done (or the stars would not shine as they do); and that, thirdly, if the work were done there it would be altogether worthless, I venture to say that Dr. Siemens' theory is absolutely inadmissible. If it be asked why, that being so, the mathematicians of the Royal Society invited Dr. Siemens to read his paper, I can only suggest that this was but a just and proper tribute to the excellent scientific work for which he is, and will always remain, deservedly eminent. Richard A. Proctor.]

BUTTERFLIES AND MOTHS.

As the month goes on, both moths and butterflies are becoming more and more plentiful, both in regard to the number of individuals and the species to be obtained.

During the week just passed we have had over twenty moths emerge from the pupa, the larva or caterpillars of which we bred last year.

This breeding is the best means of getting a good collection, as the insects come fresh and undamaged from the chrysalis, whilst those caught by any other method whatever are sure to sustain some breakage or other damage, and we advise collectors to take all caterpillars they can find, and breed them up. Some difficulty may be experienced at first as to food, but by taking notice from what trees the caterpillars are taken, and feeding with the same, the difficulty is overcome.

In redemption of one of our last week's promises, we now give the most commonly-employed methods for catching butterflies and moths. For butterflies the only way is to have a net, various forms of which are in use, the clap-net, and oval or round sweeping-net being most common. The clap-net is a rectangular piece of gauze with a rod at each side, which latter are used as handles. To capture an insect with this the net is thrown over it, and the side-rods or handles rapidly brought together. This action encompasses the victim in the folds of the net, where he is killed and transferred to the pocket-box.

The sweeping-net is a deep bag of the same material as the last, the mouth being kept open by a piece of cane or thick wire bent to the form of an oval or circle. The net should be about 3 ft. in depth by 1 ft. across the top, and tapering to a point at the bottom, this size being the most convenient, anything much larger proving too clumsy in use. Personally we prefer this form to the clap-net, though first-class work can be done with either with a little practice.

Whilst hunting butterflies, one rule must be observed, *never get between the sun and the insect*, as your shadow will nearly always prove fatal to the chance of capture; also, try to get to windward, if possible.

As moths fly principally in the night-time, very different methods to the above must be adopted. Sugaring is that most generally in use. It consists in smearing the trunks of trees in woods, forests, &c., with a composition formed of beer and sugar boiled together, with sufficient new rum added when cold, to give it a strong smell, and to increase its intoxicating properties. The moths come to it in great numbers, and becoming quite stupefied with the rum, can be easily taken in the chip-boxes mentioned in last article. When sugaring, a good lantern, preferably a bull's-eye, is indispensable to show up the moths in the dark. The hour to begin sugaring depends on the state of the weather, &c., but, generally speaking, as soon as it becomes tolerably dark it is time to start. A still, warm night will be sure to supply plenty of game, but if there is any East in the wind, or a bright shining moon, scarcely a moth will be seen.

Numerous other ways have been tried, some dependent on the various tastes of the insects, others on the well-known proclivity moths have for coming to a light. An ingenious contrivance called the "American Moth Trap," made by Mr. Cooke, of Museum-street, W., operates on the latter principle, and often makes very good bags. Details of this will be given in a future number.

Those moths which fly in the day-time can be caught with the net in the same manner as butterflies, and many nightfliers can be captured in a similar way in broad daylight by beating the hedges with a strong stick, the moths being frightened out of their hiding-places under the leaves by the unusual commotion.

The classification list which I mentioned in last week's paper is too long for these pages at present, but at some future period, when the season's work is nearly finished, we will give it in full, with Mr. Editor's permission. At present we should advise our readers to separate their insects simply into butterflies and moths until the busy time is over, when they can be arranged and named at leisure. The principal and most prominent difference between the two consists in the *antennæ*, or horns, of butterflies being furnished with a knob or club-shaped thickening at the extremity, while in moths this is wanting. There are several other structural differences, but the above is the most easily recognised.

We now give the two diagrams mentioned in last week's paper. Fig. 1 is a setting-board, the shaded portion being cork, with a groove cut down the centre, and the unshaded a wooden backing,

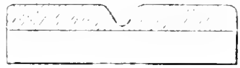


Fig. 1.

preferably pine, to increase the stiffness and strength. The whole should be covered with soft, white paper, as this adds greatly to the appearance, by giving a neat and finished look to the otherwise unsightly board.

Fig. 2 shows an insect set out on one of the above boards. The *modus operandi* is as follows:—First stick a pin through the thorax of the insect, and fix into the centre of the setting-board, with the body in the groove. Then stretch out the wings in the manner shown with a very fine needle, and keep in position with

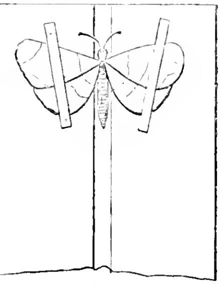


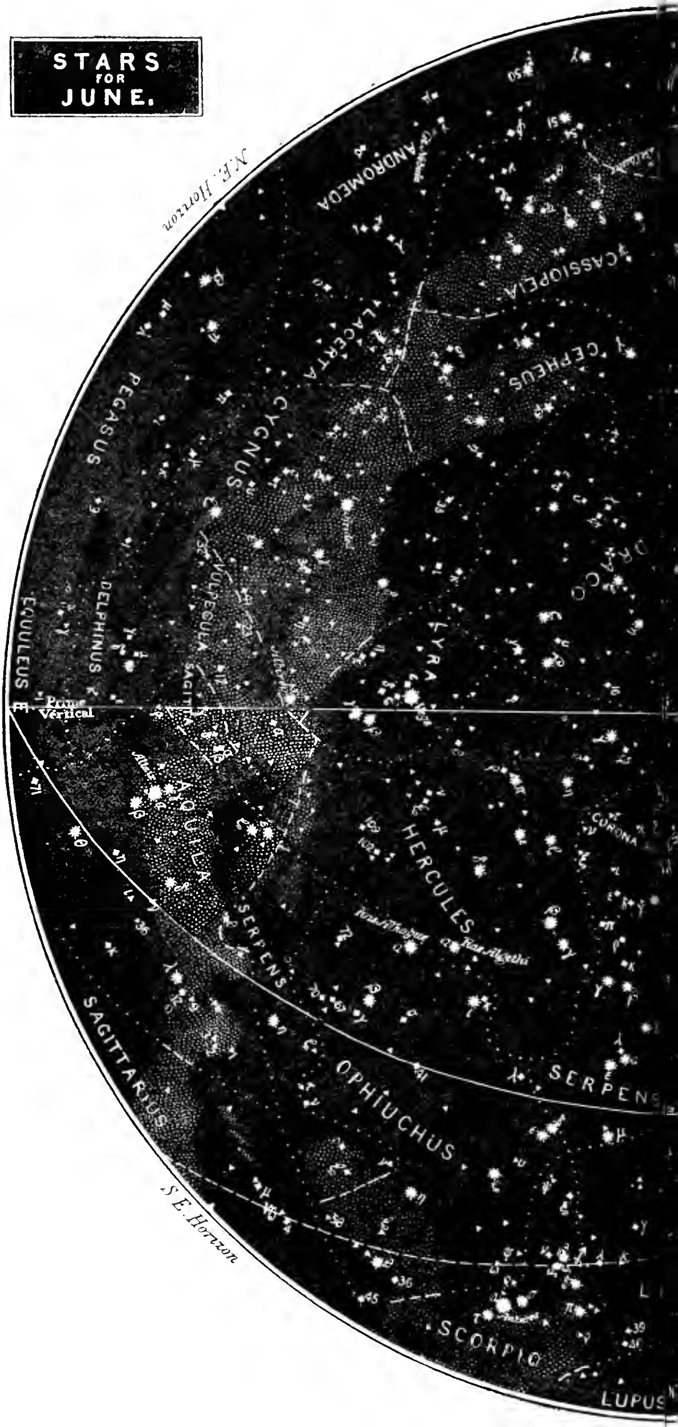
Fig. 2.

strips of cardboard pinned tightly over them. Be careful to set the wings symmetrically, as it adds greatly to the appearance of the insect when in the cabinet or store-box. Practice, however, is the only thing required for this, and the experience gained by setting half-a-dozen moths is worth volumes of description and theory.

Mr. Francis George Heath has accepted the editorship of the *Journal of Forestry*, the new volume of which, just commencing, will give considerable space to all subjects interesting to lovers of the country.

Messrs. Smith, Elder & Co., have announced for immediate publication the first volume of a work on "Human Morphology," by Mr. H. A. Reeves, of the London and other hospitals. The book will consist of seven hundred pages, and will contain five hundred and fifty illustrations, and is likely to become a standard authority in its department. Two other volumes are to follow, and each will contain tables and numerous illustrations. A work on diseases of the breast, by the same author, and containing new views, will also shortly be issued.

STARS FOR JUNE.



OUR STAR MAP.—The circular boundary of the map represents the horizon. The map shows also the position of the Equator and of that portion of the Zodiac now most favourably situated for observation. The hours are scarcely suited for observation towards the end of the month, but at that time the map for July will be issued, which will be suited for times two hours later. Moreover, it is easy to take into account an hour's motion of the stars, and so to observe on June 21st, at 10 (and so on), when the skies will be sufficiently dark. The names of thirty-nine stars of the first three magnitudes are given below.

On May 30, at 10.30 p.m.
On June 3, at 10.15 p.m.
On June 7, at 10. 0 p.m.
On June 11, at 9.45 p.m.
On June 14, at 9.30 p.m.
On June 18, at 9.15 p.m.
On June 21, at 9. 0 p.m.

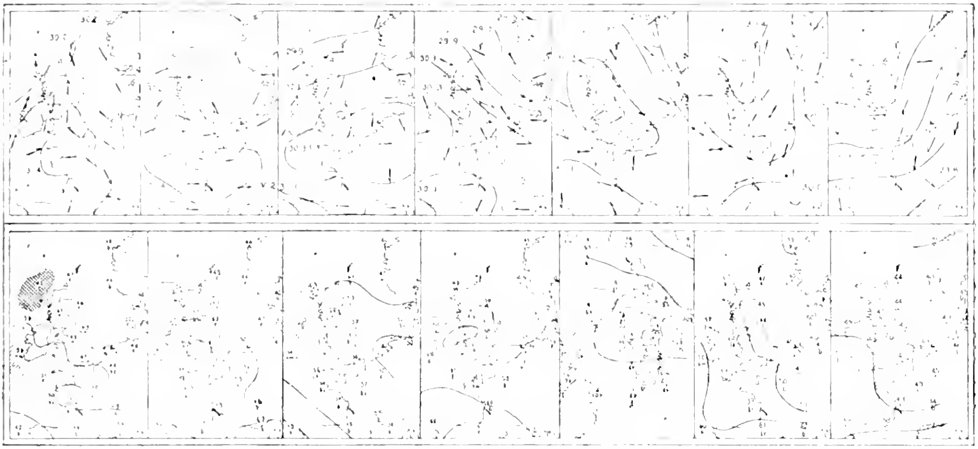
ARABIC NAMES OF STARS.

The following table exhibits the names of all the stars of the first three magnitudes whose names are in common use:—

α Andromedæ	*Alpheratz*
β ———	*Mirach, Mizar*
γ ———	*Almach*
α Aquarii	*Sadalmelik*
β ———	*Sadalsuud*
δ ———	*Skat*
α Aquilæ	*Altair*
β ———	*Alshain*
γ ———	*Tarazed*
α Arietis	*Hamal*
β ———	*Sheratan*
γ ———	*Mesartim*
α Aurigæ	*Capella*
β ———	*Menkalinan*
α Boötis	*Arcturus*
β ———	*Nekkar*
ϵ ———	*Izar, Mizar, Mirach*
η ———	*Muphrid*
α Canum Venat.	*Cor Caroli*
α Canis Majoris	*Sirius*
β ———	*Mirzam*
ϵ ———	*Adara*
α Canis Minoris	*Procyon*
β ———	*Gomeisa*
α Cassiopeiæ	*S[illegible]di[illegible]ali*
β ———	[illegible]
Canopus	[illegible]
β ———	[illegible]

α Cephei ...	... *Alderamin*
β ——— ...	... *Alphirk*
γ ——— ...	... *Errai*
α Ceti ...	... *Menkar*
β ——— ...	... *Diphda*
ζ ——— ...	... *Baten Kaitos*
ο ——— ...	... *Mira*
α Columbæ ...	... *Phact*
α Coronæ Bor. ...	... *Alphecca*
α Corvi ...	... *Alchiba*
δ ——— ...	... *Algores*
α Crateris ...	... *Alkes*
α Cygni ...	... *Arided, Deneb Adige*
β ——— ...	... *Albireo*
α Draconis ...	... *Thuban*
β ——— ...	... *Alwaid*
γ ——— ...	... *Etanin*
β Eridani ...	... *Cursa*
γ ——— ...	... *Zaurac*
α Geminorum ...	... *Castor*
β ——— ...	... *Pollux*
γ ——— ...	... *Alhena*
δ ——— ...	... *Wasat*
ε ——— ...	... *Mebsuta*
α Herculis ...	... *Ras Algethi*
β ——— ...	... *Kornephoros*
α Hydræ ...	... *Alphard, Cor Hydræ*
α Leonis ...	... *Regulus, Cor Leonis*
β ——— ...	... *Deneb Aleet, Denebola Deneb*
γ ——— ...	... *Algeiba*
δ ——— ...	... *Zosma*
α Leporis ...	... *Arneb*
α Libræ ...	... *Zuben el Genubi*
β ——— ...	... *Zuben el Chamali*
γ ——— ...	... *Zuben Hakrabi*
α Lyræ ...	... *Vega*
β ——— ...	... *Sheliak*
γ ——— ...	... *Sulaphat*
α Ophiuchi ...	... *Ras Alhague*
β ——— ...	... *Cebalrai*
α Orionis ...	... *Betelgeux*
β ——— ...	... *Rigel*
γ ——— ...	... *Bellatrix*
δ ——— ...	... *Mintaka*
ε ——— ...	... *Alnilam*
α Pegasi ...	... *Markab*
β ——— ...	... *Scheat*
γ ——— ...	... *Algenib*
ε ——— ...	... *Enif*
ζ ——— ...	... *Homan*
α Persei ...	... *Mirfak*
β ——— ...	... *Algol*
α Piscis Australis	... *Fomalhaut*
ε Sagittarii ...	... *Kaus Australis*
α Scorpionis ...	... *Antares, Cor Scorpionis*
α Serpentis ...	... *Unukalhai*
α Tauri ...	... *Aldebaran*
β ——— ...	... *Nath*
η ——— ...	... *Alcyone* (Pleiad).
α Ursæ Majoris ...	... *Dubhe*
β ——— ...	... *Merak*
γ ——— ...	... *Phecda*
ε ——— ...	... *Alioth*
ζ ——— ...	... *Mizar*
η ——— ...	... *Alkaid, Benetnasch*
ι ——— ...	... *Talitha*
α Ursæ Minoris ...	... *Polaris*
β ——— ...	... *Kochab*
α Virginis ...	... *Spica Azimech, Spica*
β ——— ...	... *Zavijava*
ε ——— ...	... *Vindemiatrix*

WEATHER CHARTS FOR WEEK ENDING MONDAY, MAY 15.



Explanation of Charts.—The two charts for each day show the general condition of the weather over Western Europe at 8 a.m. In the upper chart the height of the barometer is expressed by "isobars," the value of each line being given in figures. The prevalent winds are shown by arrows, which are drawn flying *with the wind*, the force being indicated thus: ⋙——→ = a heavy gale; ≫——→ = a gale; ——> = a fresh to strong breeze; ——> = a light to moderate breeze; and ⊙ = a calm. In the lower chart the weather is indicated as follows:—b = blue sky; c = detached clouds; o = overcast; m = misty (hazy); f = foggy; q = squally; r = rain; h = hail; s = snow; l = lightning; and t = thunder. The general distribution of temperature is shown by "isotherms," and the readings at certain places are given in figures. Diagonal lines = rough sea, the shading being proportional to the disturbance.

The above arrangement has been devised in response to a great number of suggestions. It will, probably, be that which we shall eventually adopt for good; but we may be able, perhaps, hereafter to carry the charts to a later date each week. If so, this will be done without break of continuity.

Notes on Art and Science.

Fire Risks and Electric Lighting.—The Society of Telegraph Engineers and of Electricians have appointed a very influential committee to consider and report upon the rules which they would recommend for adoption for the prevention of fire risks arising from the use of the electric light.

Crystal Palace Exhibition.—As at present arranged, the Electrical Exhibition at the Crystal Palace will close on June 3. Already several exhibitors have begun the work of removing their goods. The Right Hon. the Speaker and a number of Members of both Houses of Parliament, including the Electric Light Committee, will visit the Exhibition on the 27th inst.

Engineering.—At an extraordinary meeting of the Society of Telegraph Engineers yesterday week (18th inst.), Mr. Stroh repeated his admirable lecture on "Attraction and repulsion due to sonorous vibrations, and comparison of the phenomena with those of magnetism." On the occasion of the first delivery, the *élite* of the world of physical scientists attended, and it is universally admitted that Mr. Stroh, by his marvellously-executed experiments and lucid explanations and deductions, has gone a long way towards demonstrating the nature of magnetism.

Electro-Magnetic Repulsion.—At the Physical Society on Saturday, 13th inst., Professor Guthrie exhibited his modification of Arago's experiment, in which a rotating disc of copper repels a horseshoe magnet suspended vertically above it from the end of a chemical balance beam; a plate of glass being interposed between the disc and magnet to prevent the air from disturbing the results. Professor Guthrie gave a table of observations made on the repulsive force for different speeds of rotation of the disc, and these showed that the repulsive force varies in proportion to the square of the speed of rotation.

Electric Fire-Alarms.—In the annual report of Captain Shaw, chief of the Metropolitan Fire Brigade, it is pointed out that by reason of the system of electric fire-alarms and the excellent telegraphic intercommunication between stations, &c., the various fire-engines and men of the brigade can be concentrated on any given spot in a very much shorter time than was formerly the case. Captain Shaw reports that the electric fire-alarm system has been found to work well, and it is to be greatly extended.

The Recent Magnetic Storm.—The curves given by the Kew magnetograph during the week ending April 22, when auroral displays were so common in America, and earth currents so prevalent in the inland telegraph lines and Indian and Atlantic submarine cables, clearly indicate a severe "magnetic storm" or disturbance of the terrestrial magnetic field. According to Mr. Whipple, the indicating magnets at Kew were somewhat disturbed on April 14, but remained quiescent until the night of the 16th, when they became strongly affected at about 11.45 P.M., and from that time the storm raged until 8 P.M. of the 17th. The maximum reduction in the earth's magnetic force took place at 6 A.M. of the 17th, and a little after noon on the same day both forces became so increased that the registering speck left the field of the receiving instrument for nearly two hours. A second period of disturbance began about 3.40 A.M. of the 20th, which was violent up till about 2 p.m., and gradually diminished in intensity until 7·45 a.m. of the 21st. During this period the magnetic force fluctuated largely, but not to the extent occurring on the 17th. The fact that there were at the time two unusually large spots crossing the sun's disc is certainly in support of the theo that these magnetic storms are associated with sun spots. Mr. Whipple accounts for the abrupt commencement of magnetic storms by the supposition that possibly a sun spot only produces such an effect when cutting certain lines of force, which he imagined might extend for a limited angular distance around the earth's radius sector. It is to be hoped that magnetic observatories will be multiplied, especially in the southern hemisphere, as Professor W. G. Adams has pointed out, and it is satisfactory to learn that the French Government have decided to equip such an observatory at Cape Horn.

To Correspondents

Letters to the Editor.

[The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with their writers. All communications should be as short as possible, consistently with full and clear statements of the writer's meaning.]

All Editorial communications should be addressed to the Editor of Knowledge; *all Business communications to the Publishers, at the Office, 74, Great Queen-street, W.C.*

All Remittances, Cheques, and Post-Office Orders should be made payable to Messrs. Wyman & Sons.

**** All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.*

All Letters or Queries to the Editor which require attention in the current issue of Knowledge, *should reach the Publishing Office not later than the Saturday preceding the day of publication.*

(I.) Letters to have a chance of appearing must be concise; they must be drawn up in the form adopted for letters here, so that they may go untouched to the printers; private communications, therefore, as well as queries, or replies to queries (intended to appear as such) should be written on separate leaves.

(II.) Letters which (either because too long, or unsuitable, or dealing with matters which others have discussed, or for any other reason) cannot find place here, will either be briefly referred to in answers to correspondents, or acknowledged in a column reserved for the purpose.

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

"God's Orthodoxy is Truth."—*Charles Kingsley.*

Our Correspondence Columns.

THE LATE MR. THOMAS DUNMAN.

[409]—I trust you will find me space to record the inexpressible grief with which I read in this week's Knowledge of the death, under the most painful circumstances, of Mr. Thomas Dunman. Though personally unacquainted with him, I am sure many who have read his excellent words of wisdom and kindness in the Essays on Life and Duty which he wrote for the "Universal Instructor," will feel with me that they have lost almost a personal friend.

It is difficult to avoid the conclusion indicated by the melancholy example of poor Thomas Dunman, viz., that "science does not pay;" certainly in his case it did not. Often, doubtless, but inadequately remunerated, he laboured to improve an ignorant but unappreciative generation, only to fall an early prey to an overzealous devotion to them and to science.

I earnestly hope the publicity which you have given to the fact that the bereaved family is unprovided for, will induce many friends to come to their assistance; I shall esteem it a privilege to be allowed to add my contribution, and I feel sure there will be many like myself, who will only await an opportunity to pay a similar tribute to the memory of him who has been so sadly cut off in the flower of a useful and earnest life.

May 19, 1882. J. L. W.

AURORA BOREALIS.

[410]—Aurora Borealis here last night; brilliant from about 11.15 to 11.45; three principal masses of vertical pencillings, like an aerial Staffa, one under Polaris, another about Auriga, a third in Gemini; the limits E. and W. Cassiopeia and Cancer, and altitude about that of the former. Colour not pronounced; pinkish drab, perhaps. This is the only aurora I have seen here in six years, and follows on the hottest weather ever known in the second week of May; definition at night magnificent; therm. 64° at midnight a night or two ago. Stiff E. wind.

Pornic, France. Hallyards.

TOBACCO AND CONSUMPTION.

[411]—The light that has recently been thrown on the cause and nature of consumption (see Prof. Tyndall's letter) has prompted me to propose tobacco as a preventative and cure for that disease. Some years since, I heard a lecture in New York, by the Turkish Consul for that port, who said consumption was comparatively rare in Turkey. The lecturer claimed that freedom was owing to the peculiar manner in which the Turks inhale the "supreme solace." Your Turkish smoker not only draws the smoke into his mouth but inhales it into the lungs; and it was further stated that the lungs of a Turkish smoker after death were found to be stained a light yellow or buff by the condensed products of the combustion or distillation of the tobacco. The lecturer claimed that these products kept up a constant irritation in the air cells of the lungs, preventing any accumulation of morbid matter. How true all this may be I leave to others better qualified to decide. The importance of the subject makes the slightest facts belonging to it worthy of consideration. It struck me, on reading the late discovery of organic germs of consumption, that not only might the smoke and its constituents act as an irritant on the lungs, but that the condensation and deposition of the empyreumatic oils, &c., might prevent the germs taking root. But a word as to how the Turk smokes. From what I can learn, his shortest "cutty" is fully a yard long (the "tchibouk"), and we all know the construction of the "hookah," and the tempering and purification it exercises on the smoke passing through it. Such smoke, after passing through the long tubes, must lose a great portion of its active and biting properties, and, again, Turkish is not a very pungent variety of the "weed"; so, after all, such a mode of smoking is not so pungent in its effects as it would seem at first sight. Now, don't let these remarks induce any one to try the experiment of taking smoke into the lungs from a short pipe, and using "shag" tobacco, or a violent fit of coughing, and perhaps vertigo, may be the result. If the experiment is tried, let it be with a cigarette or a long-stemmed cherrywood pipe, using the mildest of tobaccos. I have for some time smoked that way myself, and find that the smoke frequently loosens little round globules of mucilaginous phlegm. Some people refuse to believe it is possible to take the smoke entirely into the lungs. They say it gets no farther than the throat, and thence out of nasal passages; but one can draw a good mouthful of smoke, then take a deep breath through the mouth, and speak or repeat the alphabet, and then emit the smoke, proving the smoke to be taken down *into* the lungs. Whether these suggestions are of value I leave to the faculty to determine. If tobacco is really a remedy, I would rather see us a nation of smokers than a nation of consumptives.

W. B. Wicken.

ENCORES AT CONCERTS.

[412]—It is a little too bad that the best singers at concerts, those with voices best worth careful treating, should be rewarded for their skill and excellence by being called on to repeat their songs. At a charming concert the other day, at the Alexandra Palace, an idea occurred to me about this, which strikes me as being original. Those who use unfairly the right of applauding a singer should be treated by the manager of the concert to repetitions (for the sake of practice) by the worst singers; while the singers who perform best should, as a reward, take a rest. Thus, Signor Maas, who, if not phenomenal in vocal qualities, is one of the very sweetest tenors of the day, should have been let off one of his four songs because he gave such perfect satisfaction with the other three. Instead of this, he was asked for more, which is unreasonable and unfair. When there is encore money, the manager of the concert is wronged; where there is not, the singer suffers. M. P.

A MOUNTAIN 3,000 FEET HIGHER THAN EVEREST.

[413]—In a journey through the Island of Papua, made by Capt. J. A. Lawson in 1871, that explorer discovered a mountain, which he named Hercules, the height of which he estimated at at 32,783 ft. above sea level. Has it never been visited or measured by any other explorer? It would seem strange if no expedition had since been sent to verify Lawson's result. E. C. R.

"Studies of Venus Transits."—Those sheets of the work originally published as "The Universe and the Coming Transits," which relate to the Transits of Venus in 1874 and 1882, are now published by Messrs. Longmans & Co. under the above title. Portions are now necessarily out of date, but all that relates (1) to the transit of Venus in 1882, and (2) to the comparison between the transits of 1882 and 1874, remain as trustworthy now as when originally published in the Monthly Notices of the Royal Astronomical Society. The chart which appeared in the last number but one of Knowledge, is copied from one of the charts (in two colours) illustrating these studies. Price 5s.

Answers to Correspondents.

•.• All communications for the Editor requiring early attention should reach the Office on or before the Saturday preceding the current issue of KNOWLEDGE, *the increasing circulation of which compels us to go to press early in the week.*

HINTS TO CORRESPONDENTS.—1. *No questions asking for scientific information can be answered through the post.* 2. *Letters sent to the Editor for correspondents cannot be forwarded; nor can the names or addresses of correspondents be given in answer to private inquiries.* 3. *Correspondents should write on one side only of the paper, and put drawings on a separate leaf.* 4. *Each letter should have a title, and in replying to a letter, reference should be made to its number, the page on which it appears, and its title.*

J. PERKINS. Volume I. ends with number 30. You ask why the price is so high. The answer, I am told, is simply that, to oblige customers, the publishers have disposed of copies of the early numbers and parts (to complete sets) which would otherwise have been available for volumes. Consequently, not one half, or one quarter so many volumes can be bound as they would have wished. To give you an idea how small the stock of volumes, I may mention that from a single colonial bookseller an order for more than one quarter the entire number of volumes then available was received within a few days after the volume had been announced.—R. G. You can readily express a number of many digits, all but the first few being cyphers, in a contracted form by indicating the power of 10 corresponding to the addition of so many digits. Thus instead of 1728183 followed by seventeen digits you may write 1728183×10^{17}.—A READER FROM THE FIRST. Thanks for your suggestion respecting scientific instruments on the three years' system. Will find room for it if possible; but if not, pray consider that our poverty (of space) and not our will declines.—H. H. You are not quite right, but the author of "The Stars and the Sun" is mistaken in that matter. When we see the first rays of the rising sun, we see him in the direction, with reference to the celestial sphere, which he had really occupied eight minutes earlier. The actual effect on the time of apparent rising is not, as you suppose, measured by the time light takes in travelling from the horizon to the eye, or as the author of "Stars and Earth" supposed, the light journey from the sun; but is the time occupied by the horizon plane in shifting through 22″ (the aberration angle) in the direction of the sun's motion on the celestial sphere, and therefore varies with the latitude and time of year.—CORCAGIENSIS. What has appeared about Earth's Population is not Mr. Connall's statement, but mine. It can hardly be said to need confirmation, for the calculation is sufficiently simple and obvious. Nor does it contrast with other statements similarly based on calculation. The present population of the globe could *stand*, I suppose, on five or six square miles. I would like to see actual *proof* that 36,627,843,275,075,845 persons have lived on the earth. Considering that the population of the earth has never probably exceeded 1,800,000,000, that total would imply a very long period during which the earth has been inhabited by man.—F. C. S. Thanks: but, alas, no space. I cannot see myself that the evidence proves the superiority of the boys, but that all work and no play makes Jack a dull boy and Jill a dull girl. I can find space for the extract alone. Your theory of the formation of the earth is not reconcilable with astronomical facts.—JUMBO. Your "banter" is good; I acknowledge with contrition that the stars in my monthly maps do not shine with all the brilliancy of stars in the heavens. I must get the printers to use the blackness of night for ink, and sections of the sun's surface for paper. Seriously, you would find it a good plan to punch holes through the map for all the brighter stars, then to paste tissue paper over the back, and hold the map before a good light. The W you speak of (Cassiopeia) can, however, be very well seen, I find, at twice the distance you mention (a yard and a half).—JAS. DEAS. The phenomena observed in cases of so-called "mind reading" are curious, even when cases where there may be deception are eliminated; but where is the evidence of magnetism? Will try, however, to find room for your experience. If not, remember that the reason will simply be want of space.—P. P. D. Thanks.—W. You have not quite correctly written your question; but it means that we are to find the value of the series whose general term is $(-1)^{r-s}\dfrac{|\underline{n+r-1}}{|\underline{r-s}}$ between the limits $r=s$ and $r=$ infinity. In this there is no difficulty: making r successively equal to s, $s+1$, $s+2$, &c., we get the series

$$\frac{|\underline{n+s-1}}{1}-\frac{|\underline{n+s}}{1}+\frac{|\underline{n+s+1}}{|\underline{2}}+, \&c., \textit{ad inf.}$$

$$=|\underline{n+s-1}\left[1-\frac{n+s}{1}+\frac{(n+s)\,(n+s+1)}{|\underline{2}}-\&c.\right]$$

$$=|\underline{n+s-1}\left[(1+1)^{-(n+s)}\right]=\frac{|\underline{n+s-1}}{2^{n+s}}$$

J. V. ELIASSON, W. M. WILLIAMS, E. CHAYTOR, and others. It is likely enough that a change from 2d. to 3d. weekly, with certain improvements which you suggest, would find us with a scarcely diminished circulation, and be considered a not unfair charge. But I should myself have strong objections to the change, and I shall do my best to prevent its being tried. So far, indeed, as mere fairness is concerned, such a change in price without any increase in the average expense of KNOWLEDGE, would still, I think, leave readers nothing to complain of; for, of course, the cheapness of KNOWLEDGE must be regarded as an experiment, and (it may be) a risky one, even with the presently constantly growing taste for knowledge. But we have faith and patience, which will, we believe, be justified. Our readers can do a great deal to help us, and to put any change such as you suggest entirely out of the question. If each reader in three obtained but one new reader, or if each in twelve obtained four, the publishers would turn an entirely deaf ear to all suggestions of change of price, and nearly all the suggested improvements would be made at once. As matters go, we look forward to that state of things a little later; and, to resume the first person, readers may rest assured I shall do my best to see that no change of the former kind takes place.

PHOTOGRAPHY.

WISCAP asks for information as to painting photographs on glass in oil or water-colours by a process called "Crystoleum." We never heard of the process. Any one who can paint in oil-colours could apply the process to glass. We doubt the practicability of painting on glass in water-colours.—H. H. The wet collodion process is not the thing of the past you suppose it to be, and, for certain purposes, is not likely to be supplanted by gelatine. Our scheme includes a series of articles on the gelatine dry-plate process. That no dark room is needed while working with gelatine plates is new to us.

ELECTRICAL.

H'SETT. There is a wide difference of opinion as to the origin of earth currents, and accordingly, our ideas concerning them are of the vaguest kind. During the earlier portion of the disturbance you refer to, the currents affected only the wires running N.E. to S.W., but subsequently wires in all directions were affected by currents which doubtless owed their existence to a number of conspiring and conflicting causes. How far the geological structure of the superficial strata affect, by their varying resistance, the passage of earth currents, it is impossible to say, but I should imagine very little indeed. Discarding the earth plate, and substituting a return wire (technically termed a loop), prevents almost entirely currents passing from the earth to the line. This demonstrates that, so far as the wire is concerned, the currents are not induced, but imparted. I heard of no warning being given by our meteorological office to the collieries, of coming magnetic and electric storms; and, in fact, such warnings never have been given by the society. The American Bureau is, however, a little less antiquated, and ventures to predict these storms, and that they may be attended by colliery disasters. One or two men in England are also working on this line.—J. F. R. I really should advise you not to attempt making a machine to support a single Swan lamp. You would find it a comparatively expensive affair. A Swan lamp requires a current of 1·2 ampères (see articles on Generators).—T. SINGTON. Better use small rectangular rods of good metal, and solder the junctions.

ECLECTICUS AND READERS OF KNOWLEDGE GENERALLY.—As soon as we have finished our description of simple forms of electric generators, the subject of "Electrical Units and Simple Methods of Measurement" will occupy our column for the "Amateur Electrician."

BIOLOGICAL.

"G. S." calls attention to what he is pleased to term "a couple of inaacuricies" (*sic*) in a biological note in KNOWLEDGE (page 613) of last week. The first inaccuracy refers to the statement that "with the exception of the Camelidæ, all quadrupeds have round *nucleated* blood-globules." The word "nucleated" should have been rendered *non-nucleated*, the omission of the "non" being a printer's error. We differ from "G. S." both in his spelling of the word "inaccuracies" and in his contention that the word "round" does not describe the blood-globules of mammals. He adds the expression should have been "circular and bi-concave." If "G. S." sees any difference between "round" and "circular" (applied to a blood-globule) he is very welcome to his contention. We did not describe the globule as to its form (bi-concave), since that question was not asked by the inquirer.

By a misprint, "three molars" were said to exist in each jaw of the old world apes as in man. The statement should have read *six molars* in each jaw.

[About two pages of "Answers" have been unavoidably held over.]

Our Mathematical Column.

SOLUTIONS TO PROBLEMS.

[P. 526]—In a triangle given $a+b$, $a+c$, and angle A, to construct the triangle.

We have received correct solutions to this problem (KNOWLEDGE, April 14) from R. F. R., E. N. Dalton, C. E., J. P. Morgan, J. B., Thos. Lyons, and others. The following construction and proof is somewhat simpler than any sent us; Mr. Dalton's is nearest to it, and is in principle the same; but the construction of a species of auxiliary triangle is unnecessary and inelegant.

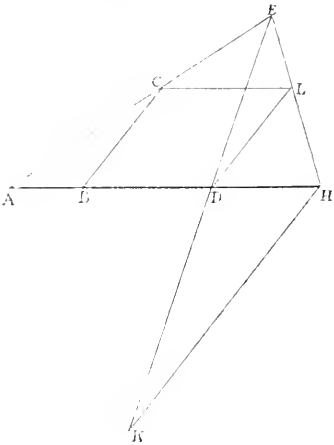


Fig. 1.

Let AD $= a+c$; AE $= a+b$; DAE = angle A and produce AD the less to H, making AH = AE. With centre H, distance HA, describe a circle cutting ED produced in K; draw DL parallel to KH to meet HE, LC parallel to AH, and CB parallel to DL. Then ABC is the required triangle.

For triangle ECL, being similar to EAH, is isosceles; and CLDB is a parallelogram. Now,

DL : KH :: EL : EH :: EC : EA

But KH = AH = EA;
therefore DL = EC = CL = BD
or BC = EC = BD
So that AC + BC = AE = $a+b$
and AB + BC = AD = $a+c$.

[33]—*To bisect a triangle, ABC, by a line drawn from a given point, D, without it.*

The neatest solution we have received of this problem is the following:—

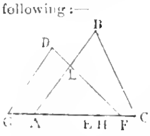


Fig. 2.

Continue out the side AC to meet GD parallel to AB in point G; and bisect the side AC in E. Then make, as GD is to AB so AE to AH. And again, make as GA is to AF so HF to AH; then, if the line DLF be drawn, it will bisect the triangle ABC; *i.e.*, the triangle ALF will be half of the triangle ABC. J. C. KELLY.

[Mr. Kelly does not give the proof, but it is tolerably obvious. Thus, by construction:—

GA : AF :: HF : AH
∴ GF : AF :: AF : AH
$GF^2 : AF^2$ :: GF : AH

Now triangle ABC : triangle GDF : : AC.AB : GF.GD
:: AC.AH : GF.AE

And triangle GDF : triangle ALF :: $GF^2 : AF^2$
:: GF : AH

Wherefore triangle ABC : triangle ALF :: AC : AE
:: 2 : 1 —ED.]

We have received a neat solution from C. E., and other correct solutions from R. L., Exon, J. P., and others.

[37]—*From a given point to draw two equal straight lines to two given lines, the straight lines thus drawn to include a given angle.*

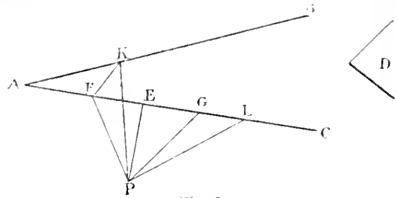


Fig. 3.

The neatest solution sent us is the following:—Let AB, AC, be the two given lines, D the given angle, and P the given point. First let P be outside the angular space BAC. Draw PE perpendicular to AC, and make the angles EPF, EPG each equal half the given angle D. At the point F make the angle PFK equal PGC, and cut off GL equal FK, and join KP, PL. KP, PL are the lines required. Since the triangles PEF, PEG are equal in every respect, PF equal PG. Again PF, FK are respectively equal to PG, GL, and the angle PFK to the angle PGL; therefore, PK is equal to PL, and the angle FPK to the angle GPL; therefore the whole angle FPG is equal to KPL, and FPG is equal to the given angle D.

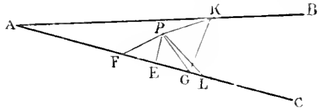


Fig. 4.

Next, let P be within the angular space BAC. Draw the perpendicular PE, and construct the triangles EPF, EPG as before. Make the angle PGK equal PFG, and cut off FL equal GK, and join LP. Because triangles PFL, PGK are equal (Euclid i. 4) in every respect, therefore, PL equal PK, and angle FPL equal GPK. Take away the common angle GPL, therefore angle FPG equal LPK, and FPG is equal to the given angle D.

[D. M. fails to note, however, that there are two solutions to each case. If we make angle PGK′ equal to PFA in the first case, and in CA produced if necessary, make FL′ equal to GK′, then PK′ will equal PL′, and the angle K′PL′ will be equal to D. So in the second case we may make the angle PFK′ equal PGE, and take GL′ (towards A) equal to FK′; then will PK′ equal PL′.—ED.]

We have received various correct solutions of this problem from T. R., Nemo, H. A. N., E. Whitley, and others.

[38]—Let A be the centre of the disc at which its whole weight may be supposed to be placed, AB the radius of the circumference described by the centre A, AC the radius of the disc, C being in contact with the plane on which the disc rolls. Draw the vertical line AD to the level plane, and complete the parallelogram ED. Let w be the weight of the disc, AC its radius $= r$, AB $= r'$, AB + CD = R the radius of the track, angle CAD $= \theta$ and and vel of A $= v$.

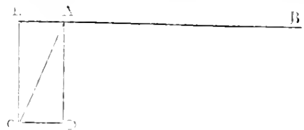


Fig. 5.

$$\text{Then } \frac{w}{g} \cdot \frac{v^2}{r'} : w :: \text{EA} : \text{AD}$$

$$\therefore \frac{v^2}{gr'} = \frac{\text{EA}}{\text{AD}} = \tan\theta \text{ ; hence } r' = \frac{v^2}{g\tan\theta}$$

$$\text{Now CD} = r\sin\theta \therefore \text{R} (= \text{CD} + \text{AB}) = r\sin\theta + \frac{v^2}{g\tan\theta}$$

—D. M.

[This solution can only be regarded as approximate; the assumption that the whole weight may be regarded as collected at the centre of gravity being inadmissible in a problem of this sort.—ED.]

Our Whist Column.

By "Five of Clubs."

A GAME FOR STUDY.

WITH reference to this game, *O. P. Q.* makes *Y. Z.* win the odd trick. He hits on the right line of play, but errs in causing *Z.* to discard from weakest and *B.* from strongest suit, though strength in trumps is with *A. B.* G. D. BROWN makes *A. B.* win the odd trick. He causes *Z.* to return his partner's lead of spades, though the only chance for *Y. Z.* lies in *Y.* having strength in Diamonds. T. D. Mackenzie notes that a small club being led, if *B.* finesses the Knave (which he does not approve), *A. B.* will make eleven tricks. Such a finesse in one's partner's suit, and that suit trumps, would be unpardonable. If Ace is played, our correspondent thinks A. B. should make the odd trick. The following is the play in Cavendish :—

THE HANDS.

A.
Clubs—K, 10,9,8,7,6,2.
Spades—K.
Hearts—K, 4.
Diamonds—K, Kn, 9.

Y.
Clubs—Q, 4, 3.
Spades—Kn, 8, 5.
Hearts—A, 5, 2.
Diamonds—A, Q,10,2.

B
Dealer.
Y Z
Trump Card, Club Five.
A

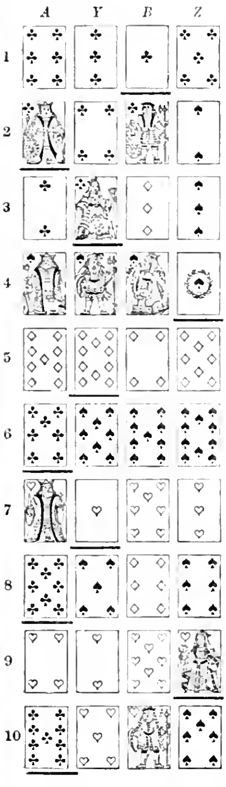
B.
Clubs—A, Kn.
Spades—Q, 9.
Hearts—Kn, 10, 9, 8, 7
Diamonds—7, 6, 4, 3.

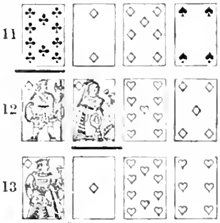
Z.
Clubs—5.
Spades—A, 10, 7, 6, 4, 3, 2.
Hearts—Q, 6, 3.
Diamonds—8, 5.

Score.—*A B*, 3; *Y Z*, 4.

THE PLAY.

NOTE.—The underlined card wins trick, and card below it leads next round.





REMARKS AND INFERENCES.

1. *A* leads the penultimate (the ante-penultimate is not *de rigueur* with a six-card suit; but here would have been very suitable).

2. *B* returns the best of two left. *Z* discards from his longest suit, the opponents having shown superior strength in trumps.

3. *A* leads a trump, though thus two fall for one, to show his partner his strength, and that one trick from *B* will win the game. This *Y* and *Z* equally perceive. *Z* sees that nothing can save the game unless *Y* is strong in diamonds (*B*'s discard showing that *B*'s suit must be hearts). *Z* discards another spade, as he must retain the power of leading his partner's suit more than once.

4. *Y* leads his partner's suit. Luckily the honours fall in a single round.

5. *Y* finesses deeply, knowing that, if either King or Knave is with *B*, the game is up anyhow, and

6. Leads his best spade.

7. *A* leads his King, from King one small one—see Leads.

10. *B* discards his best hearts to show he has entire command of the suit.

There can be no doubt, I think, that on both sides the hand is played as good players would be sure to play it. "Cavendish" who considers *A*'s play sound throughout, remarks that *A*'s lead at trick 3 is unlucky, as it puts the adversaries on the only tack for saving the game. But *Y* can count the trumps as it is. He knows *B* cannot have more than one left after second round; and that, if *B* had none, *A* would simply put down the six remaining trumps and claim game. If *A* had not led a trump at trick 3 (which was certainly his proper course), he would have led the small diamond, on which *Y* would have put his ten as the only chance of saving the game. The result would have been the same.

We shall take an early opportunity to exhibit another case, somewhat similar in character.

IT DIDN'T MATTER.—My partner trumps my best card, or does not trump a doubtful card after I have called for trumps, or commits some other Whist enormity. We win the game, notwithstanding, for we have prodigious cards. If I suggest that there was no occasion to perpetrate the enormity in question, my partner triumphantly informs me, "It didn't matter." This view is altogether fallacious. It did not happen to matter in that particular hand, but my confidence is impaired, and it will matter in every hand I play with that partner for a long time to come.—Cavendish's "Card-Table Talk."

Our Chess Column.

VIENNA INTERNATIONAL TOURNAMENT.

The following is the gross score of the players after the finish of the Ninth Round on Saturday night, May 20:—

Blackburne ...	7	Steinitz	$4\frac{1}{2}$
Mackenzie	7	Wittek	$4\frac{1}{2}$
Winawer	7	Weiss	$4\frac{1}{2}$
Noa	6	Fleissig	$3\frac{1}{2}$
English	$5\frac{1}{2}$	Ware	3
Mason	$5\frac{1}{2}$	Bird	$2\frac{1}{2}$
Zukertort	$5\frac{1}{2}$	Meitner	$2\frac{1}{2}$
Hruby...	$4\frac{1}{2}$	Paulsen	2
Schwarz	$4\frac{1}{2}$	Tschigorin	2

Mackenzie has up to date not lost a single game, but drawn four. Blackburne, we are glad to remark, is also doing very well indeed. He lost both his first and second games, but since that he has won seven consecutive games. Mason is also doing well, his score being $5\frac{1}{2}$, which is also the total of Zukertort. Mr. Steinitz had again a bad day on Friday last, when he was beaten by Wittek, keeping his score at $4\frac{1}{2}$.

[*By Telegram.*] Vienna, Tuesday night.

Score of English team :—

Blackburne	8	Mason... . .	$6\frac{1}{2}$
Mackenzie	8	Zukertort	$5\frac{1}{2}$
Steinitz	$6\frac{1}{2}$		

Played in the seventh round, May 17th, of the Vienna International Tournament, between Herren W. Steinitz and B. Fleissig.

FRENCH DEFENCE.

WHITE. Steinitz.	BLACK. Fleissig.	WHITE. Steinitz.	BLACK. Fleissig.
1. P to K4	P to K3	21. B to B2	Kt f B4 to K2
2. P to K5(*a*)	P to Q4	22. Q to Kt3	K to Rsq
3. P takes P *en pass.*	B takes P(*b*)	23. Q to R4	K to Ktsq (*i*)
4. P to Q4	Kt to K2	24. Q to Kt3	K to Rsq
5. B to Q3	Kt to Kt3(*c*)	25. Q to R3	Kt to Ktsq
6. Kt to KB3	Kt to B3	26. Q to R5	R to B2
7. Kt to B3	Kt to Kt5(*d*)	27. B to Q2 (*j*)	QKt to B3
8. B to QB4	P to QB3	28. Q to R3	Kt to Q4 (*k*)
9. Kt to K4(*e*)	B to B2	29. P to QB4	QKt to B3
10. Castles	Castles	30. QR to Qsq(*l*)	Q to Ksq(*m*)
11. R to Ksq	Kt to Q4	31. B to B4 (*n*)	R to Bsq(*o*)
12. Kt to B5	Kt to R5	32. Q to R3 (*p*)	B to Kt2
13. Kt to K5	Kt to B4(*f*)	33. Q takes P	B to Rsq
14. P to QB3	B takes Kt	34. Q takes KtP	P to Kt4
15. R takes B	Kt to B3	35. B to Kt 3	Kt to Q2
16. R to Ksq	P to KR3	36. Q to Kt3	P to KB4
17. Q to B3 (*g*)	Kt to Q4	37. P to B3	K to Kt2
18. B to Kt3	P to QKt3(*h*)	38. P to B5(*q*)	QKt to B3
19. Kt to Q3	B to R3	39. Kt to B4	resigns(*r*)
20. Kt to K5	R to Bsq		

NOTES.

(*a*) Not usually played; the object is to confine the Queen's Bishop, and hamper Black's game.

(*b*) Perhaps to be preferred to P takes P, as the two Pawns on the Queen's side would, at a later stage of the game, be subjected to attack.

(c) W[illegible] w[illegible] [illegible]ly playing P [illegible] Kt.

(d) B[illegible] was afraid of Castling, on account of the commanding [illegible] White's Bishop, for after Castle's White might at once [illegible] with P to KR4, Kt to Kt5, P to R5, &c., that is to say, [illegible] the basis of attacking the Pawn on R2, of which we [illegible] the general line; therefore, Black wished to exchange that Bi[illegible].

(e) This again places another piece in a favourable position: Should B[illegible] play P to KB4, then his King's Pawn becomes weak, because unsupported by another Pawn, and therefore more liable to be captured.

(f) All this is merely wrangling for good positions, but Black is wasting time in trying to exchange pieces.

(g) This is Mr. Steintz's old style; Black cannot move P to QKt3 now, even if he wished to do so; he suffers from the inconvenience of having his Bishop blocked in.

(h) We shall see later on how the Pawn on B3 will fare.

(i) Black would be satisfied with a draw.

(j) Inch by inch of the ground is won; this is a fine move. He intends at the suitable moment to push on his Queen's Bishop's Pawn and use the Bishop for attacking on the Queen's side *vià* Kt4.

(k) Playing into White's hands; but the difficulty is, what to do? He dare not move the King's Knight, as White would play B takes RP. Had Black played Kt to Q2, White might have responded with Kt to Kt4, threatening the dangerous Kt takes RP, which would yield White a winning attack.

(l) White is in no hurry, he goes steady but sure; this move will further aid White as the Black Queen's Bishop's Pawn cannot now be advanced.

(m) With the object of avoiding a discovered attack on his Queen, but it cramps his pieces very much.

(n) White changes the originally intended move, for if he had played B to Kt4, then Black could advance the Pawn to B4, he having for that purpose played his Queen to King's square. White now threatens to win the exchange by Kt to Kt6(ch).

(o) R to K2 was the only other move at his disposal, but White would have different ways of continuing his attack. Black's Rook is brought into awkward play on account of the necessity of defending his QBP, showing plainly how a strong player will take advantage of a very slight weakness even.

(p) White pressed on in sometimes almost an imperceptible manner, and now he has gained the desired opportunity. He wins two Pawns and the game, he having by sheer good judgment outmanœuvred his opponent.

(q) A fine move. It further tightens his already strong hold. He intends playing his Knight to Q6.

(r) Black simply has no good move; he is crushed. If R to Ksq, then B takes BP. Besides B takes P. White also threatens to win by Kt to Q6. If Q to Qsq, then, of course, R takes P.

Played in the seventh round of the Vienna International Tournament, between Herren Zukertort and English, May 17, 1882.

Ruy Lopez.

White. Zukertort.	Black. English.	White. Zukertort.	Black. English.
1. P to K4	P to K4	23. P to QB4	P to KR4 (i)
2. Kt to KB3	Kt to QB3	24. Q to Q3	R to B3
3. B to Kt5	Kt to B3	25. Q takes Q	R takes Q
4. Castles	Kt takes P	26. P to B4	P to R5 (j)
5. P to Q4	B to K2	27. Kt to B5	P to R6
6. Q to K2	Kt to Q3 (a)	28. P to KKt3	R to Ksq
7. B takes Kt	KtP takes B (b)	29. Kt to R4	R to Kt5
8. P takes P	Kt to Kt2 (c)	30. R to KBsq	Kt to Bsq (k)
9. Kt to B3	Kt to B4	31. K to B2 (l)	Kt to Q2
10. Kt to Q4(d)	Castles	32. Kt to B3	Kt to B3
11. R to Qsq (e)	Q to Ksq	33. Kt to Kt5	R takes Kt
12. Kt to B5	P to B3	34. P takes R	Kt to Kt5ch (m)
13. B to R6 (f)	Kt to K3	35. K to Ktsq	R takes B
14. Q to Kt4	R to B2	36. P to Kt6	K to Ktsq
15. B to K3	K to Rsq	37. R to B7	R to K8 (ch)
16. Kt takes B	Q takes B	38. R to Bsq	R to K6
17. P takes P	Q takes P	39. R to B7 ([illegible])	Kt to K4
18. Q to QR4 (g)	Q to Kt3	40. R to K2	R takes P
19. R to Q2	P to B4	41. R takes BP	R to B3 (o)
20. Q to Kt3	P to Q3	42. R to K3	B to Kt7
21. Kt to K2	B to Q2 (h)	resigned (p)	
22. Kt to Kt3	B to B3		

NOTES.

(a) We do not at all like this move, but it is the consequence of Black's defence.

(b) The Bishop is usually captured with the Queen's Pawn.

(c) Looking at the position now, we must agree in [illegible] it as our opinion that Black has not at all obtained a satisfactory defence.

(d) To prevent P to Q4, and, in general, to hamper Black's game.

(e) Threatening Kt takes P, and taking immediate energetic action.

(f) This is very vigorous; if Black takes the B, then Q to Kt4(ch), and on Q interposing, Kt takes B(ch); but the question is, has Black a defence, in which case the B will be forced to retire later on.

(g) The object of this move is to attack the weakened Pawns on the Queen's wing; but, of course, he must wait his opportunity, and in the meantime Black might force matters on the King's side.

(h) Black is defending himself very well. Should the white Queen play to Kt7, then Black would retire his KR to Bsq, threatening R to Ktsq. We think, therefore, that the flank march of the Queen was ill-advised.

(i) Having got his Bishop in the good position on QB3, Black now assumes the offensive himself, forcing the Queen, after her little excursion on the Queen's wing, to return for the better protection of her royal consort.

(j) He proceeds undaunted; should White advance the P to B5, then R to Kt5, and after P takes Kt, P takes Kt, P takes P, and R takes P, the White Pawn on K6 will be weak, and eventually fall.

(k) Black's manœuvring is very fine; should the White Bishop retire to B2, he would proceed with either Kt to R2, with the intention of playing P to Kt4, or, perhaps better, Kt to K3 attacking the BP, to be followed, on the B again retiring to Ksq, by Kt to Q5, threatening R takes Kt, and then Kt to B6(ch), &c.

(l) An effort to extricate his King from the very uncomfortable position, and, at first sight, to be preferred to retiring the B to B2, thereby still further blocking up his King.

(m) This is indeed play of the very highest order. Black resisted the Kt to K5 wisely; now he keeps up his attack. K on the K's file cannot be thought of, for after Kt takes B, Black would win the exchange by discovered check.

(n) Zukertort would, of course, be quite satisfied with a draw.

(o) The winning move.

(p) If R to Ksq., Kt to B6 (ch), K to B2., Kt takes R (ch), and White has no resource left. We have no hesitation in saying that Herr English is following closely in the wake of Steinitz, Zukertort, and Blackburne. He is the man of the future.

NOTICES.